

Henry G. Small

Oral History Interview

Oct. 10, 2013

Interviewed by Trudi Bellardo Hahn

TH: This is an oral history interview with Dr. Henry Gilbert Small. My name is Trudy Belardo Hahn. It's October 10th, 2013, about a little after 10 am. And we're here in the Rush building in the Deans conference room. And the purpose of this oral history is to be a life history including your childhood, family, schooling, higher education, but we're going to spend most of our time on your professional work and career. So can we start with a...what you want to tell us about your early life and family?

HGS: Ok. I was born in Chicago, Illinois. We lived on the north side of Chicago.

TH: What neighborhood?

HGS: Ravenswood...it's called. My father was a high school teacher. My mother was a housewife but before that trained as an elementary school teacher. They had met in Southern Illinois. My father went to the University of Chicago, as did his brothers. So they had a good education. They had grown up in Chicago but my father's family. My mother's family...ahh...was rural and they had a farm in Illinois.

TBH: Southern Illinois?

HGS: Southern Illinois. And we would go there during the summer. My sister and I we'd spend our summers there. My grandfather was a farmer and a brick layer. So I got to see both the

country life and what it was to be a farmer without any tractors and power equipment only using mules and horses.

TH: Wow.

HGS: Chuckles

TBH: Wow. What a great childhood.

HGS: Hooking up the rake...and , you know...hooking up all the mechanical equipment that you find on a farm. Which is also pretty dangerous.

TBH: Did they put you to work during the summer?

HGS: Well...yes. And my mother was the one that allowed me to go with my grandfather on this horribly dangerous machinery. Which I was shocked. My mother was very interesting woman. I mean she...she was very sweet and very loving but on the other hand she said Ok Henry you want to do that? Do It! (laughs)

TBH: Uh...huh

HGS: She cut me a lot of slack

TBH: Do you think that was maybe the generation? That they weren't so hyper cautious.

HGS: Yes I think that there was...She did have some quirks. My father also perhaps. They were extraordinarily sanitary. When we went on a car trip, they would take a jar of cloths that had been soaked in soap. And we were to wash our hands before every sandwich in the car on a car

trip. So this...This was the other extreme. I think my father was a little more cautious and conservative than my mother. And he was a high school teacher; he had to kind of toe the line with his students and be a disciplinarian. And she was sort of a loose, a loose person who you could...anything goes. Not anything.

TBH: A bit of a germ phobia?

HGS: I think that was really my father...but who knows. It was an interesting childhood. I got to meet a guy down the road, at the adjacent farm, who turned out to be a very successful physicist in later life who went to England on a full ride scholarship, I think and got an academic position somewhere in England. He was a bookworm and I think a prodigy. For that area certainly and went to college early and we would sit around and have intellectual discussions. I wasn't as much of a reader as he was but I was still interested in science.

TBH: So this is still before you graduated high school?

HGS: Yeah, this was grade school.

TBH: Grade school.

HGS: I will give you my scientific resume, I had a chemistry set, I had books on gravity and the universe by George Gavoff. I read those and introductory chemistry things "these are atoms and elements and that kind of stuff". And I knew by the time I got to high school. I had a pretty good grounding in chemistry and I really didn't need the introductory chemistry course. So I ... that impressed some of my teachers. I also had an intellectual bent because I remember the first essay I that I wrote in high school--I think as a freshman--was on the nature of time and I still

remember this. It was the concept of time. And I loved this topic because I read about relativity theory, I knew that time could vary, you know, depending on how fast you were going and that intrigued me. So I asked my father if he could take me to a library where I can see some books and he took me to the John Crerar Library in Chicago, which is a very technical scientific library downtown. It's not there anymore because I looked for it and it wasn't there. It's in the Union Carbide building in downtown Chicago. And he loved going downtown because he was a historian. He taught history and civics in high school. And he would show me the different historical...and we would stop by the Chicago Historical Society and he would show me the Civil War stuff there. That fascinated me because we had an ancestor who was in the Civil War, which we had a record of and which I still am very interested. So he developed that. I was interested in history. I was interested in science. But we went to the John Crerar Library and I had never seen so many books on science. And I was fascinated. And I wrote this essay and it, he kind of criticized me; it was a little bit wordy you know. It's kind of pretentious and wordy and that kind of thing.

TBH: Your father said this?

HGS: He thought I was...because he was more into stylistic things. He was also an English major and very strict on spelling, which I couldn't spell worth a darn. Anyway, it was a good experience for me. I think any kid who aspires to be a scholar in any way getting that early experience, writing an essay, doing the research, looking at the books, reading them, trying to digest them and thinking about them. That's a great process. So anyway, I had a lot of interests. I was very introspective and kind of a withdrawn kid. I wasn't so withdrawn that I didn't have a social life. In fact, I was president of my senior class. Now the story behind that is that my father

was a teacher at the same high school and he had on occasion been the sponsor of the senior class. Not when I was a senior, because he intentionally pulled himself out of it but I knew from him all the things that should be done for the senior class that weren't being done. So I got up and gave a campaign speech promising that I would do all of these things. (Chuckles) that my father had told me should have been done but hadn't been done.

TBH: What's a for instance there?

HGS: Like they had this thing called senior hall. You get a warning you have to go there for an hour, maybe it was a half hour. And it was very really boring; people would just get up and make announcements. I thought...maybe this was my idea, I can't remember. Let's have entertainment. Ok. (Laughs) That was one I did. Another one was getting graduation gowns early so we could get our pictures taken in them. Because usually there wasn't enough time to get your picture taken. So I had all of these crazy ideas

TBH: That sounds reasonable.

HGS: I had a bunch of good friends. One of my friends came up with all these slogans with my name in them like All for Small or Have a ball in senior hall with Henry Small. It was...it was just a real hoot for me and being I was a studious kid up to that point. I had never really been consistently politically. Something however went wrong. And that was... I had recruited a couple of very funny guys in our class to do a skit. And they got up and one of the guys pulled a whiskey bottle out of his coat and his pants fell down. The teachers, particular elderly women teachers, just were appalled.

TBH: I don't know which is worse.

HGS: And stopped us immediately, called the principal, expelled these kids from school. The whole thing. Erupted into a kind of a ridiculous side show.

TBH: Yeah, and did you take the rap for that?

HGS: In part but not entirely. (Chuckles) I managed to escape without a suspension. Probably because my father was a teacher, student, etc etc and I was the class president. Oh

TBH: Having a ball with Henry Small

HGS: And they did have a ball. They enjoyed it. Kids love that. And I thought it was the funniest thing I had ever seen; it was like something out of ... Saturday Night Live. It was just wonderful. Okay, I'm kind of ...I'm kind of getting off on tangents here.

TBH: That's fine, that's just fine.

HGS: Posterity

TBH: These are things about Henry Small that people don't know.

HGS: No one would ever know them. So everybody in my...all the guys, we kinda had a club and hung out together and stuff but they all wanted to be engineers and we had kind of an intellectual club. And there was another club; it was very cliquey in those days. There was another one a bunch of hoods and thugs going around beating people up. We weren't in that club. This was sort of the student athlete higher aspiration group. But they all wanted to go to the University of Illinois and be engineers. Because that's just what you studied at the University of Illinois. I said I was interested ... I want be a chemist. Well another thing that happened in high

school that was so funny was that I took an aptitude test and I scored very poorly on it. Like I couldn't alphabetize fast enough or something. I can't remember what it was but they told me, well you shouldn't and there's no point for you aspire to an academic career. You should just go into sales or something like that. Of course I knew different because I knew that I could [do the work]. I don't know why I didn't do well on the test but and it was only one test. I didn't have any trouble getting into college or doing well. In fact I was Phi Beta Kappa in college so my high school teachers probably looked at that and said what this is... (Chuckles) How can we get this guy? But I had a kind of... I had such a powerful kind of life thinking about things. Thinking about scientific problems; I'm thinking about, music, or art whatever it was I knew that if I thought about it long enough, I could come up with an idea. And that was sort of the kind of... I had the confidence in my own inner ability to solve whatever they threw at me, given enough time. That was the essence because I couldn't solve it then and there. But if I took it home with me and thought about it for a day or two, I came back and I could. So that was my secret weapon. I wasn't real bright on my feet but on the other hand I knew how to think. I knew how to put the ideas together.

TBH: You persevered.

HGS: And I was very determined at whatever I did. I was very tenacious about pursuing whatever goal it was. So I did go to Illinois. I didn't get into chemistry right away because the class had already been filled. I couldn't get into an introductory chemistry course. So I took other courses. I took German. And I knew for some reason ... someone had told me think it was one of my uncles. And my uncles had gone to the University of Chicago; they were both physics teachers in high school so I had allthere were three brothers, my father and his two brothers. They were

all high school teachers, and the father, their father was a principal in a school. So they came from a long line of educators, high school educators, whatever you call them. But the physics guys said oh you have to take German because German's the language of science. Of course when they went to school in the 1930s and the 20s German was the main language. So I took German. And I did so well in German that I actually took a trip to Germany in the summer ... you know one of these kinds of work study kind of things. And I worked in a brick factory and I ... think it was in my sophomore year and I traveled around in a VW with a guy from New York. I met some interesting people. I'm glad I was able to do that. That was a real important kind of experience in my life, going to Europe and seeing a little bit of what the world was about.

TBH: From what you've said so far, you've run into a neighbor who was a physicist, you had two uncles who were physicists.

HGS: Yes, my connections to physics were unbelievable and it continued throughout my whole life.

TBH: You played with chemistry sets and your father was into history. So a lot of these foundations are being laid for the things that interested you and you got involved with later. Did German play out later in your career?

HGS: Oh yeah. Absolutely. It was essential. I almost made the mistake of becoming a German major. I took a German literature course and I was so fascinated with German and was so good at it. I had done so poorly in high school Spanish. And I was determined to do well in something else because I couldn't figure out why I wasn't picking up on Spanish; I think it was the teacher. But anyway, the German teacher I had was just wonderful. And another great teacher I had in

college was a guy named Stanley Elkin. Stanley Elkin became a very famous writer. He wrote sort of comic tragic comic or black comedy kinds of novels. I also ... I took a lot of different kinds; I basically had enough credits to have a BA. I didn't want a master's of science. So I took psychology. I took literature ... a wide variety of courses and I was interested in so many things. I really didn't know what I wanted to do. I wasn't even sure I wanted to do chemistry.

TBH: But you must have been getting a lot of A's everywhere.

HGS: Yeah, I was in my junior year. I worked so hard. I mean I just studied like all the time. I can't understand why I was so motivated to study. But it must have had something to do with my family. I somehow had internalized all of this education stuff. (Chuckles) I can go to school and study. I also could be what I wasn't ... I didn't have that active of a social life. That's another possibility. In Illinois there were so many students, you really had trouble meeting very many of them. I mean there weren't ... I could have gone into a fraternity but I didn't want to because you know I knew the distractions. I knew a couple of guys that went in and they did poorly, had poor grades and so forth. So I just sat around and I studied. And I would think ... I would brew about things sometimes. And I would obsess about things like gravity. (Laughs) Or inertia. Or electric fields. You know I just was a weird guy. And I would get to a point where if I fully understood something then I could see it in my head. Because I had a very visual sort of way of understanding what was the subject matter. I was also interested in music. I had taken piano lessons and I heard about this guy who was a chemist, his name was Hiller. He switched from chemistry to music and went into the music department. That intrigued me. How did you start out as a chemist and then go into the music department. Well turned out his claim to fame was that he programmed one of the first digital computers, which was called the Iliac at the

University of Illinois? It wasn't the first digital. I think the first digital was.... The Iliac; it was called the Iliac Suite. He wrote a program to have this computer write music and it was the first computer generated music composition.

TBH: Was it any good?

HGS: I have the record. It's on an LP. You can buy it at your local record store, if there are any. I don't think there are anymore. It was'nt that great. (Laughs)

TBH: Like a dog dancing on its hind legs. The fact that it does it at all.

HGS: It's a little bit better than a dog on the hind leg. But its... .He had programmed it in such a way as to capture different stylistic periods. So he could do something that sounds a little bit like Baroque or a little bit like Bach or a little bit like classical. You know it was based on transition probabilities between different note progressions so if there was a perfect fifth or a leap of a certain kind. (Phone rings in background). Oh, I gotta turn my phone off. (Answers phone...Hello. Ok. Alright. Thank you. Bye.)`Umm...

TBH: You hear about mathematicians who are fascinated with music. But this man he was a chemist?

HGS: I will say... I really, I seen the program on educational television but the Bartok String Quartet is on there. I'll never forget this. And I didn't understand what was going on but they were explaining and it was intellectually very interesting. And I just ... I wanted to pursue that. And I didn't think I could do it as a career or anything. I just was so fascinated with it. In

particular with this guy who went from chemistry to music? And this became another theme, the music stuff became another sort of preoccupation or obsession if you.

TBH: And did you later play, other than the piano. Did you ever play another instrument?

HGS: Oh, I picked up the guitar. I've seen every kid in the 60's play guitar. And did folk music kind of stuff. In fact we had a folk music group in high school. That was imitating various other ...

TBH: So you did have a garage band?

HGS: Yeah, we did, we did. As a matter of fact, we recorded some of our songs in high school. And we would perform in senior hall, like on a couple occasions. Our pants stayed up, fortunately.

TBH: And no whiskey bottles.

HGS: No. And my father, because he was musician ... He was a singer, like a frustrated Broadway guy. He wanted to sing in musicals and operas and stuff. But never got... He did it, in his early life. In fact he was on the radio. He was in a quartet. But he had a record player that could record. And I don't know if you've ever seen them. They have these tiny little records like this. And a needle that went around. And cut the groove as you were singing into a microphone. And so we have ... I found a lot of recordings of his singing. And then when we had this folk music group in high school, of 3 or 4 of us who thought we could play guitars and stuff. And we recorded a couple of things, like Tom Dooley...The Weavers. (Laughs)

TBH: I'm from the same era and I remember it well.

HGS: Hootenanny kind of stuff. Anyway. That's another sideline. And then later on in life I got interested in jazz. I took up the saxophone. Sort of imitating Garfield in that regard. He was a frustrated jazz musician. His mother was a musician as well. And then my son got interested and actually became a jazz musician. (Chuckles)

TBH: You mentioned.... Can we go back just for a second about your propensity to try to visualize things...to help you understand them. Did you also doodle and make drawings and things like that?

HGS: Yeah ... well every since I was a kid I had notebooks that I would write in. And this was another one of my crazy things that I started very early. I had written down all the things that occurred to me as far as you know scientific or mechanical inventions. Most of them were totally impractical. But I had this little notebook and at a certain point I started to write down ideas about science. Like I had this notion that atoms were like little solar systems and people lived on them.

TBH: Little people?

HGS: Yeah, little tiny people and that the solar system was an atom in a larger unit. So I had this notice of you know this hierarchal thing. We were all just a part of a bigger structure.

TBH: And we don't know that that's not true.

HGS: I carried this, well yeah I, the physics didn't quite work but I carried this idea around with me for many many years. And .. I would calculate for example distance from the nucleus to electron. And compare that to the distance from the Sun to the Earth, or Sun to Mercury or Sun

to other planets. Were they proportional? Was the mass of the nucleus...if you take the mass and distance and all the other factors into account were they, were they proportional? And I figured out that they kind of were. I was into the theory of gravity. Anyway, I had always written down ideas. I had notebooks and papers that I kept. Now the same habit kind of carried through in my later life. Obviously I was too busy in college to keep a notebook of what was going on. But when I got into my working life, I found it indispensable to keep notebooks. So I have 20 notebooks at home. I have a day book of everyday that I was at ISI and who I met and what I was doing. I reconstruct down to the most nauseating detail. In fact I was looking in there, I actually found the first day I met Belver ... Well anyway that's getting ahead.

TBH: And you, you wrote about meeting him. When you wrote Belver and Henry were you going back to your notes or were you just going from memory?

HGS: Yeah, I did look at my day books. And I had written down when I was going to come to Drexel and meet him. When I first met him I saw him when he was leaving the building and we said hi. That and then the elevator door closed. So when I got to the end of my undergraduate degree, I had taken a lot of different types of courses. And I Really didn't know what I wanted to do but I had this chemistry thing; I had the major and enough credits for that. And I said ok I just will continue. Why not. I mean I didn't know what else to do.

TBH: But you didn't have any grand vision of a career?

HGS: No, I didn't at that time. The Vietnam War had started. Around that time or just starting to get going. And of course getting a deferment was a big deal, you know. There was an educational deferment that you could get. I never got drafted; I think the reason, at some point

when I had reached an age where I was safe beyond draft age. I called the draft board and said, why didn't you take me? Why didn't you draw my name? I was just curious to know and I wanted to know why? Because so many people were drafted. He said well your name comes toward the end of the alphabet. And we had a big area. I was from Chicago, big neighborhood, lots of kids, lots of boys. And they were just doing alphabetic order.

TBH: Wow.

HGS: Talk about luck. Because I could have easily been drafted because at some point they would drop the student deferment.

TBH: But you weren't, another way people got out of it was to get married and have children.

HGS: I didn't pursue that route. So I applied for teaching assistantship at the University of Wisconsin.

TBH: In chemistry?

HGS: In chemistry and I went to Wisconsin. And taught honors freshman chemistry for 5 years, long time looking back on it. I spent a long time in graduate school. The first years I was in chemistry, I was in theoretical chemistry as a matter of fact, which was very mathematical. I would say it was too mathematical for what I really wanted to do. But I was working for a guy named Fensky, not David Fensky, another Fensky. Richard Fensky. He was a very prominent, molecular orbital theorist and...my job.... We did what was called semi-empirical molecular orbital theory, which was an approximation, quantum mechanical approximation, to the way orbits, a modeling of the electronic wave equations for electrons and atoms. Trying to predict

spectral lines so my job initially was to get the spectrum of a chemical, titanium tetra chloride, which was a very volatile chemical ... [creating] a smoke screen. I had a great uncle who had, in chemistry during the First World War. Can you imagine? The chemical agents that they developed. But he told ... I believe that he said that he had worked on smoke screen type of chemicals. Anyway, so it was highly reactive, it would react with water. I had to create a special device, I had to fabricate it with glass blowing techniques and it was a cell that, into which.... First you had created a vacuum, within the cell and you had to have special lenses on each side of the cell....a little glass thing, a glass tube. And you fill it with this titanium tetra chloride gas. And hope and pray that you had gotten all of the water vapor out of there. I sealed it off with my Bunsen burner. Or my blow torch I forget which. I would put it in a bag and take it on my bicycle and ride for a mile to the spectrometer building, which was in Madison, Wisconsin. And could be in the middle of winter, middle of the summer whatever. And time and time again, I would do this experiment and not get anything or I would get the spectrum of water or something. It was a special kind of spectrometer; it was called a vacuum ultraviolet spectrometer. So, I must have done this 50 times. Filled the thing up, take it on my bicycle, go over there. Test it; nope nothing. (Chuckles) Finally, finally, I got, I said yeah it looks like you got a nice peak here. So you're looking for peaks where the light interacts with the electronic orbits and you get absorption or emission or whatever. So my major professor was really excited...Wow lets write this up. It's a good article out of it. Well first of all I had the slightest notion at that point what the math was behind this. Well he had already done the math so he was ready to go. So he wrote the paper. I don't know if he even put my name on it. The whole was so discouraging. I said I don't want to continue doing this so I talked to other professors in the chemistry department and

I thought, maybe I'll go to inorganic chemistry maybe I'll go into biochemistry. I was just looking around. But I also know...

TBH: But why was it discouraging? Because it took so long?

HGS: Because it was. Well first of all the experimental end of it just took forever. And it was just the thought of having to go through that again for some other chemical was very discouraging and furthermore I would have had to have done all the computations and which was like a black box. I mean you didn't really know if these approximations made sense. And I wanted to understand what I was doing. I think that's the bottom line. I didn't really feel that I understood the rationale behind these equations. And maybe if I had studied longer at Harvard I would have figured it out. But it was too abstract for me I think and I should have known from the start, even though I had the background to do it and I could have done it. But anyway, I had met and taken a course during my first few years in grad school with a guy named Aaron Ihde [spelling?]. And it was a history of chemistry course.

TBH: His name was?

HGS: Aaron Ihde. Well he wrote a text book on the history of chemistry actually. He's an older gentleman very nice very portly sort of guy. And I said, gee, this is interesting stuff...chemistry has a history. The only history I ever heard up to that point was from my uncles who had told me and the physics teacher guy, said there was a big discovery just as I got out of college. It was the discovery of the neutron in 1931. And gee that changed everything you know. And I had studied it; I had studied physics without the neutron you know. I didn't know what the heck was going on and hearing now that we are talking about this. He had actually experienced obsolescence in a

sense of his whole knowledge base, which was based on something that hadn't been discovered yet. I mean that came later, that sort of obsolesce. And later when I met my wife, Lois, who just brought me my pills (chuckles) that I forgot. Her father was physician and he was an excellent cardiologist and diagnostician...basically told me the same thing that everything he had been taught--well not everything but a lot of what he had been taught in school, was obsolete. So I knew there was something to this history stuff, OK.... you cannot know something here and know something later and you're whole life changes as a result of it. So I was fascinated with that and I had had, as I told you, all these ideas of gravity and atoms and all this stuff just floating around, little did I know that I was recapitulating the history of science. It turned out that my theory of gravity in fact had been proposed shortly after Newton, in the 1800's I believe. No 1700 and it was this idea of particles flying all over and hitting things and that's what causes things to go down. Because we are being bombarded with particles from outside and you absorb the particles. The striking of these particles is causing gravity. And it actually works out to be an inverse square law. But it would have already been researched and rejected as a possible explanation of gravity and obviously replaced Einstein's theory. So I had been living the history of science in my head without even knowing it.

TBH: Yeah, curiosity.

HGS: Another problem and this what I did for my dissertation and when I went in the history of science I met a guy named Robert Siegfried. Who had taught the chem-lit course at Illinois while I was there. And I knew vaguely about him but I never met him when I went to Wisconsin and I met him. He was chairman of the history of science department. Robert Siegfried and did a lot of chemistry . So I was sitting in his office and I had just finished my masters in chemistry. I went

in to take my prelims and I looked and saw all these journals. This is the history of science; perfect, read those journals. And do the history of science by reading journals. You have to go beyond that. Then you have to read the diaries, you have to read the notes, look at the correspondence, etc etc. you have to do historical research. Historical research is very broad, very time consuming if you. I said ok. But I filed it in the back of my mind because here was a guy who taught chem-lit, who had all these journals and he really hadn't thought about how one would use the scientific literature of the history of science. So you could see that seeds were already germinating in my head. Anyway, I picked a topic by a major professor, Erwin Hiebert, a very nice, very wonderful guy, welcoming and we went to his house many times. We used to have seminars in his house and his wife was very welcoming and I just felt like this was a good place for me. But Erwin Hiebert [spelling?] was a historian of physics--well of physical sciences and mainly 19th century. Eventually he went to Harvard and became a professor at Harvard. So he was very well regarded and although he didn't write a lot of books that became big hits or anything like that, like Tom Cooley who I met later on. Bu I picked quantum theory. Now the old quantum theory is different from the new quantum theory. The old quantum theory and I had already had a theory in my head about how the quantum theory worked... it used classical mechanics, what Newton's laws it used a `quantum condition that had been introduced by Max Plank earlier in the century. And so it was kind of a hybrid of classical physics and quantum physics. The reason I picked the helium matter is that the hydrogen atom had already been solved by Neals Bohr in 1913. Neals Bohr was a Danish physicist who figured out that he could predict the spectral lines of hydrogen and the ionization potential of hydrogen by using classic mechanics in a quantum condition. So the quantum condition limits the electrons to different states of energy in their orbits but they are very physically determined orbits. The electrons go

around the nucleus and then when they change their orbit they give off or absorb a photon of light or quantum of light. And give the spectral line so that theory--the Bohr theory--had actually worked beautifully for hydrogen and was kind of like the paradigm you might say. And we were studying we had to read Kuhn this was just when Kuhn's book was published.

TBH: That was about 60

HGS: We were studying Thomas Kuhn Structure of Scientific Revolution. That was absolutely required even though the professors didn't agree with it. Nobody accepted it but it was required.

TBH: Provocative. Yeah.

HGS: Exactly. The most provocative book that anybody had read up to that point.

TBH: I remember reading it in my Doctoral program and it was a lot of food for thought.

HGS: So the helium atom was the next most complicated atom in the periodic table. It had two electrons instead of one. It had a nucleus that was heavier than hydrogen. So with only two electrons you say, hey, that's a piece of cake, I can solve that. You just put an electron here and one electron over here and here's a nucleus in the middle and they're going around. And their motions quatumnize and we'll just predict the ionization potential and get the spectral line just like Bohr did for hydrogen except it doesn't work. I was sitting at home trying to figure out how I could make it work. Ok so my history of science problem was something that i had worked on at home and I was trying to figure out why. I could get close but I couldn't get all the way there. So that fascinated me, why in the world.... That became what Kuhn called an anomaly. That was almost the... it wasn't the only anomaly in the old... this is called the old quantum theory but it

was important. So my dissertation in the history of science was all about that one particular problem and how a whole bunch of different physicists, including Bohr including well known people like Arnold Sommerfeld and Born, Max Born and a whole lot of other people who worked on this project for a decade before Heisenberg solved it with the wave equation and the quantum mechanics in the mid 20s. So that was the period by 1925 from the teens starting maybe, I started pretty far back. I started maybe 1910 or something and worked my whole way up to 25. What happened in that problem. And what I was able to show and I developed a model of scientific development in my dissertation was how scientists were confronted with something that doesn't work. Systematically vary the assumptions they will make... will start out with the weakest assumption. I said well maybe the quantum condition isn't really this maybe it's something slightly different and then they will start to question well "where are the electrons?" Are the electrons bumping into each other? Are they doing something funny? Is the mechanics of it more complicated? And then finally you know maybe the mechanical laws that we've assumed applied to the atom really don't apply. Maybe they're wrong and at that point all heck breaks loose and you have the makings of a scientific revolution. They've come to an impasse. So giving that is really a textbook example of a scientific anomaly in the Kuhn sense. But I was really looking at the progression of the problem. And what I developed in my mind, a kind of pictorial representation. When you have a theory and you have an experiment. The experimental result is like the spectrum or the ionization position which is completely those is empirically determined facts, if you want to call them that about the material helium. The theory represents other assumptions that are tied together in some way. So you can say well forget the ionization potential that's not important, you know. Well, you're not going to do because that's a pretty firm result. You can't really say that that was wrong. That they should go back and measure it again.

Because that's pretty reliable so you're really varying your theoretical assumptions first and trying to make a bridge. So the hydrogen atom was worked perfectly you had a beautiful bridge, a line connecting the assumptions that are made for hydrogen over to the ionization potential experimental result. So you began... I began to think of a network representation, a series of assumptions tied together with connections and tied together and then linked across from experiment to theory. So that was my first inkling of a kind of network representation of a scientific problem. I developed this idea in the dissertation; I made a presentation on it at a history of science society meeting later on. And Gerald Holden thought it was a kind of neat idea and

TBH: Gerald Holden?

HGS: Gerry Holden, yeah, he was a very well known historian of science at Harvard.

TBH: Holton?

HGS: Holton. You will find many many writings about science, from a very broad perspective he is famous for, he would sort of listed all the important, symmetry or simplicity things that were desirable in a scientific theory. Obviously things like empirical fit and theory agreeing experiments, so on ...That's his famous essay on Theomata but he wrote a lot of very interesting articles including some that are sort of bibliometric in their approach. But a guy that I got to know better, at one point published my dissertation and almost got to the point where it looked like there was a publisher but then there wasn't so I never got to publish. And it was probably a mistake on my part not to pursue that in fact some days I think to myself I got to go back and publish my dissertation. Some of my colleagues in the history of science, What? you're not

publishing your dissertation but at that point I had other things cooking. So the dissertation, I didn't actually finish my dissertation in Wisconsin. I took a job first. I went to New York to work at the American Institute of Physics in something called the Center for Philosophy and Physics. I don't know if you're familiar with this outfit or not?

TBH: No, but when I was making my notes that was my first introduction and as I was writing philosophy of physics, I thought well that's a new concept for me. History I get but Philosophy not so much

HGS: It was located in New York City on 45th street. And there was a guy there that ran that center, named Charles Weiner [spelling?]. I think later on after he left there, he went to MIT and became a professor of history or something there. I wasn't you know... I was at Wisconsin when I got the call to come if I was wanted to do this job; they were for looking for a historian like me with some internal... I was an internalist historian of science. That meant that I was interested in scientific ideas and I wasn't a social historian. I was more from the technical side. I was looking at the technical development of the concept, of a theory. And they were looking for somebody like that to be... because they needed someone to do the history of nuclear physics. I had been working on the history of quantum physics of the atom. And they were looking for somebody to do the nuclear part which is a different branch of physics. It came along slightly later. And they had a funded project from the National Science Foundation. So I was ... I said, I'm almost finished with my dissertation why don't I just go take the job, I'll just finish my dissertation in New York and I'll... I didn't realize how many distractions there were would be when I got to New York. So I went there and I started to work on the history of nuclear physics and one day I was sitting around and I thought how am I going to do a history of a field that I know virtually

nothing about, first of all. Well, I could just sit down and read all the source materials. I could but I wanted to try something a little more systematic and so I went down to---and they had a wonderful library this was called the Neals Forth Library, which you may be familiar with because it was later moved from New York down to College Park, Maryland. And the Neals Forth Library Center for History of Philosophy and Physics are now somewhere although I've never been to the location down there. It is in College Park.

TBH: There is a physics research institute there yeah.

HGS: Well this would be more of a history of physics. And it's very much comparable to what later became the heritage...

TBH: Chemical Heritage Foundation?

HGS: Yeah, heritage foundation for chemistry. This was physics. But the physics one had existed earlier and they did oral history interviews; they collected books, they collected manuscripts. I had to go down to the Institute for Advanced Study and clean out the basement, to get Robert Oppenheimer's reprint collection. But the crazy thing about it was there were rooms and rooms of these reprints and we were taking back... we were taking them from the Institute for Advanced Study campus--which was a huge campus--to New York City, which had was one of the most congested spaces.

TBH: Right this is not...

HGS: It was insane but here we rolled up with a van full of these reprints, then unloaded them-- basically put them into a room. I don't know if anyone looked at them but they were filled with

interesting notations and stuff. Because that was one of my history projects. [But by for the library?.] I also had to do oral history interviews and I had to edit interviews and stuff like that. But mainly my job was to do something about the history of nuclear physics as a research field and I had to think of how I was going to do that methodologically because I had never had that sort of assignment before. I had only been focused on a particular problem within atomic physics and that was like a really limited kind of thing. And by the way the German comes in because I had to read all these articles in German.

TBH: Ahh...ok.

HGS: The original German, that was tough--it was really a tough thing. But the nuclear physics was much more an American kind of project. It was less European and what Charles Winer [spelling?] director of the center was interested in the guys who came from Europe and primarily Jewish physics comes from Europe to America and work on their physics.

TBH: From World War 2?

HGS: Yeah or in the 30's. They would come over during the 30's and you know started. And really an amazing people. And that was his...

TBH: One of the biggest brain drains of all times

HGS: So, I had the... I said okay I got to think of a new way of doing this. I went down into the Neals Bohr library; not only did they have books about history of physics and history of science. One of the world's greatest collections I'm sure. And the librarian there--and his [?] name Joan Warno. In an adjacent room, a little tiny room were all the physics journals from earlier in the

century so everything I needed was in that little room; all the issues of *Physics Abstracts*, which was called *Science Abstracts* early on. And all the issues of the *Physics Review*, which was the main journal of physics earlier in the century for American physics. It was true they didn't have all the German journals but that was ok. I wasn't about to read every journal article anymore. Well it just turned out it was a room a little bit bigger than this but all the walls lined with books. At the other end of the table there was an attractive young lady sitting there and I struck up a conversation with her and it turned out that I ended up marrying her.

TBH: This is Lois?

HGS: Yeah, this is Lois. This is the famous Lois who has been my wife for all these years since then.

TBH: Did she....was she in this...what was her...why was she there?

HGS: She was there because she was the executive secretary of the American Physical Society. Because AIP was a confederation of different physics societies. American Physical Society all these very specialized, like the Vacuum Society, or the Realigy [spelling?] Society, all these things that were rolled into physics. And they had and this was their offices. So she would go to all the physics meetings--the American Physical Society meetings--which is a big sort of umbrella society for physists and take notes on their executive meetings. And she was a great note taker and she would take good shorthand, all that kind of stuff. And she was... I thought she was a physicist. Who in the world would be sitting in a library with all these journals and not be a physicist; turns out she was reading New York magazine.

BOTBH: Laughter

HGS: So it was one of the luckiest strokes in my life. But it just shows you how you can meet somebody if you're interested in libraries. If you want to meet somebody who's got an interest in a library, go to the library and meet somebody. Just strike up a conversation.

TBH: Go to an art gallery to meet somebody interested in art.

HGS: Exactly.

TBH: Who was, before we go on, Joan Warno?

HGS: Joan Warno is the librarian...

TBH: And that's W-A-R-N-O?

HGS: Year something like that...She was the head of the Neals Bohr library and they had some assistant librarians there but she was the head librarian and Charles Winer [spelling?] was the head of the center for history.

TBH: But in my notes I noted that you went there as a research associate from 69-70 but from 71-72 you were the acting director.

HGS: Yeah, because Charles Winer [spelling?] went on sabbatical to the Neals Bohr laboratory, Neals Bohr institute I believe it's called, in Copenhagen. He went there so he could pursue his historical research but he was on a sabbatical and I believe there was a need to hire a director, a temporary acting director. And I also got... that was where I got to meet Kuhn; he was on the advisory committee for the center for physics. I had actually met Kuhn earlier at a lunch/dinner meeting with my major professor at Princeton Institute for Advanced Studies. My major

professor, Hebert, [full name?] was living in the barracks and Princeton had to use barracks where visiting scholars could live and he invited Kuhn, who's also a professor at Princeton, to come for dinner. And I learned there to my shock and dismay that Kuhn had been thinking about doing a history of helium--the helium atom. And if he had done that, it would have made my dissertation so unnecessary. He would have trumped me. Scooped me or whatever you want to call it. So I was so relieved that he never did that. However, he did ... he had collected ... because he was in charge of a project to document the history of atomic physics and they had deposited the papers of that project at a bunch of different libraries around the country, these libraries like John Pilbrin [spelling?] and Paul Foreman. Tom Kuhn said they would go around and interview people for the ... this is called the quantum physics archive, I believe. And there was a copy of the archive--it's on microfilm. The copy of the archive for the Philosophical Society in Philadelphia which I went to use. So they had different places around the country where they deposited these microfilms for people, for scholars. But there were reels and reels of Bohr's notes, for example, on the helium atom--on the helium problem--which he was unable to solve. And Kuhn was very intimately familiar with this. So he could have quite easily done the history of helium problem. But he didn't. So that's why I'm so grateful to him and in my own dissertation on the helium problem I had used the American Philosophical Library version of the microfilms, so that was fortuitous thing. But to get back to the Niels Bohr library. The reason I got into bibliometrics and got a job with ISI was because I saw on the shelves of this little tiny library all really what I wanted to do history with and that was the journal metadata and the classification headings from the physics abstracts. And so I literally spent day and night coding entries in *Science Abstracts* and also *Physics Abstracts* and coding each and every nuclear

physics article that I could find in *Physical Review* for author, institution, keywords, cited references, classification headings, the whole gammit.

TBH: And you did this by hand?

HGS: I did it by hand. I used long sheets of paper like that or I used 3 by 5 cards. I ...you know I was the one of the more obsessive compulsive researchers I think that has ever come along.

TBH: Sometimes you have you to start that way to get a feel for it right?

HGS: I don't know, maybe. Gene [Garfield] was just as compulsive as I was but I was determined to find a new way of doing physics with the physics literature. The things that I had sort of seen a glimpse of in Bob Seegreets [spelling?] office at Wisconsin. I finally had realized I had my opportunity to do that. I collected all this information, every data element that I could conveniently collect, and I combined it in different ways like, for example, keywords. I would collect a certain number of keywords per article and they would be in the titles or if they had an abstract it would be in the abstract of the first paragraph.

TBH: This is before authors were asked to assign their own key points?

HGS: Oh yeah, way before. We're talking about literature back in the 30's and 20s where you were lucky if they followed any kind of convention whatsoever. So coding this was an enormous challenge but I found that the cleanest data was the science abstracts classification headings cite to the core analysis, articles that were assigned to two or more subject headings in the science abstract. And once I had got the pattern together I could show that the subject category.... I show them in a map. And the map changed as new subject headings were added or as a linked formed

between existing subjects heading and a new subject heading. So I was able to show how the field evolved in terms of its relationship between subject headings. I thought my gosh this stuff is interesting. I'll write this stuff. I wrote that particular piece of my research out submitted it to *ISIS* which is the lead journal in the history of science. I don't if you've ever heard of it or not? Founded by George Smarten [spelling?] and had all this fantastic history associated with it. About a year later found out that they rejected my paper. (Chuckles) and one of the reviewers was my major professor, Scott Herbert. It was awful. It was a terrible experience.

TBH: And was it a blind review? Did he know it was you?

HGS: Yeah.

TBH: He knew it was you.

HGS: The criticism went low; this was like a bare bones history. You got to add some flash you gotta put some meat on the bones, they're just too sparse too abstract. You're trying to...

TBH: So this was about 71, 72 this is before your landmark article in 73.

HGS: Sure. I start working on it very soon after I write [the first, rejected article?]

TBH: How did you happen to land at ISI? Was it through a personal connection with Eugene?

HGS: No. So I was really.... I'd done all this work. I had written this report and this is really the touchstone of my whole career...this report that I wrote for the American Institute of Physics, which is called bibliometric indicators of the development of nuclear physics *The Physical Review* 1927-34. And I have literally all of the maps of the classification headings; I have the

keyword maps which were core keyword snaps keywords are linked there. I had the author co-citation maps you could see formations of authors because I had coded the references and so forth. All these maps. I put them in here and I found out that our grant to study the history of nuclear physics was running out and I had to get a job. So basically I was in the job market. I had finished my dissertation, I took my orals back in Madison, Wisconsin but after I had started my work. So there I was in with all this data you know. Truckloads full of coding sheets and 3 by 5 cards. And had done a report, hadn't published it you know yet. But submitted it to the NSA [need identification?] and so forth. But no real job prospects. I did a job market analysis in the history of science, which was really bad and....

TBH: If you were not an academic

HGS: No, I ...

TBH: I mean did you aspire to be?

HGS: I aspired, I could have been, I mean...to me it was just bad timing. I had one nibble from a place I could have been to be a history of science professor of something like that. But I had determined that I would try to make a go with this bibliometric stuff using a journal or two. But obviously the idea of coding all of the articles of science was just ridiculous. I couldn't do it. So I got a hold of a brochure, a pamphlet written by somebody called Carol [need full name?] and I actually saw the reference to it in library and it listed all the ANI [need full name?] services that had tapes, magnetic tapes. It was an inventory of outfits or ANI services or commercial or otherwise that generated this information. AIP was actually one of them because they were one of the first to generate this type of bibliographic metadata information in the physics literature

and there had been a major project on this using that data at MIT called the TIP project. There was a guy up there named Mike Kessler who had done bibliographic coupling and they knew about this. I went over to the other part of AIP, which was the physics information division which was across the street from the main AIP building. And I met a guy named Arthur Hershel who was head of that division. He said oh yeah you ought to go talk to Garfield in Philadelphia. (Both chuckle) or maybe you should talk to Morton Allan. Morton Allan [spelling?] was the vice president in the company but not somebody who....he happened to know that Mort was looking for someone to hire. So that was how I found out. And I had also written letters to all these people, abstracting services: biological abstracts, psych abstracts, all the abstracting and indexing services. I sent letters to lots of places: do you have a job? And I remember I got one from Dale Baker, and he sent me a graph of, you know, the number of abstracts. And said sorry we don't have any openings for historian--ones that use bibliographic information. But I'm fascinated with the idea. I wish I had more time to do it myself. Here's my graph. But another person I met in the information division [at what organization?] He was very important to me. This was Sam Simitatavich [spelling?]. I probably...a name that is not familiar to many people, even in information science. But he was from South America--an Argentinan. Probably more of a computer scientist, but his charge was to maintain the American Institute of Physics classification system. So he had developed aldermen [correct word?] that used bibliographic problems to find groupings of papers using coupling from the data. The date either AIP had or from outside. He would go to the library nearby, called the Engineering Societies Library, which is only 2 blocks away. It was a comparable organization to AIP but for engineering, but they had a library that had the *Science Citation Index*. Sam would go over and I would watch him occasionally do this and open the SCI and start writing and he had coding sheets. He would code

them on all sorts of stuff and by hand. And then feed it into his aldrege [?] when he got back. So he was creating categories or finding groupings of articles using a methodology that he had. So I everything was clicking, things were starting and I thought geez this is great. You can actually find a group of articles by using the *Science Citation Index*. I was a little excited by this as a prospect but obviously I didn't want to sit there and code entries from the Science Citation Index, at least not for a long time. So my notion was that I wanted to hook up with an outfit that had this data already and so ISI was one of the organizations and my letter there just worked like a charm. I sent them this report and I sent it to Ward Mallen [spelling?] who passed it onto Gene Garfield and Gene Garfield recently gave me a copy of the memo that he wrote when he first met me. First I met Ward who Ward came down or came up to New York from Philly and we had coffee in Grand Central Station. (Chuckles) And we hit it off. He was a historian, American history or something like that and worked in the park service for awhile but had also worked at the NSF, in the information—OSIS—division, isn't that what they call it? The one that had funded all the NI [?] services, although ISI didn't get a lot of money from them.

TBH: I've studied that history too. Garfield's frustration with the other people that were...

HGS: But anyway...but Ward had worked there, he knew their ropes in DC. He was looking for somebody to bring in grant money, projects like history. Gene Garfield was interested in the history of science. Who would have known? Well, if it hadn't been for Art Hershman [spelling?] I probably would not have followed up on that. So I met Ward and then shortly thereafter during this time a lot of things were happening. I got married to Lois, I finished my dissertation...you know a lot of things going on. And it was because of all this and getting the new job and trying to think what I would do when I got to ISI that I never really published. I didn't publish my

dissertation. There are a lot of things I didn't do. Because I saw what was on the horizon and I wanted to hold my tongue...

TBH: Just run, run, run

HGS: It was like if I went back to this I would be kinda like doing the old guard. I would be taking time away from what I thought I needed to be doing. And that was develop something like I had done for the physical review here with all the different data elements, the keywords, the authors, the institutions, the cited references, you know the whole spiel. But do that in an automated fashion with magnetic tapes and to do it across a wide swath of literature, not just one field. So that was kind of like my master plan. I actually wrote when I came to Philadelphia and had been given a job. I wrote up a long memorandum on exactly what I wanted to do. It was a little bit.... also Gene gave me a copy of the letter that I wrote to him and I wrote to actually more... or maybe I'll show you this letter. I got to spell out what I wanted, I wanted to look at clusters of keywords and citations and I wanted to see if you could identify newly emerging areas in science. And I wanted to make a product. That was a good line to throw in there. That was a good line to throw in there because I didn't just want to do it for the sake of doing it which is really what I wanted to do.

TBH: Ready to resume?

HGS: So I didn't get the job offer, I finally got that straightened out. I think I went to Philly and met Gene at an AAAS meeting. And we had a little meeting, and had I told Mort that I wanted some small amount of salary. And Gene said, "Oh, that seems like a lot of money." But he was a great negotiator. I can't remember the details; I think it went pretty well, because Mort wanted to get me. People in those days, you know, didn't get a whole lot of salary which was fine. I

realized that I didn't have any credibility; I mean, all I had was this report, I had a bunch of ideas, and things that I wanted to do that may or may not have turned out to make the company any money. As it turns out I think I did make the company some money, but they had no way of knowing that that would happen in the future.

GS: So my wife and I came to Philly. They were very, very gracious; they allowed us to stay in the company apartment for a while in the Society Till Hill Towers. It was just wonderful; I thought "Gee, this is great; man, look at all the perks you get when you work for ISI." And I wrote this memo, which I wish I had a copy of but I don't, outlining what I wanted to do. Getting there, and this is when we were down on 325 Chestnut--I believe, was the address of the building. I was right across the street is Carpenters' Hall, and you could walk out and go to Independence Hall, Carpenters' Hall, or you could do all this historical stuff. I thought, "This is great, you know, right near the Philosophical Society, the library, and a lot of restaurants. It's a wonderful area."

I immediately started working on.... I thought, "I gotta do the keyword thing" because my keyword thing really worked well here. I was able to show how this nuclear physics thing had evolved by joining together two types of keywords. And I thought, "Gee, they've got this thing called a permuterm index." A word index.

TBH: I'm sorry, one of the things you said...can we back up for a second, did you say "by combining two different types of keywords"?

HGS: In this report, I did a keyword analysis, among others, where I showed that they look basically like this. If you have these things, you have a group up here, this group of things called the disintegration group because when you have disintegration, it means that, in nuclear physics terms, the nucleus is disintegrating. But they would see alpha particles emitted or beta particles

or whatever. All physical particles being emitted. Down here you had what was considered isotopes. Initially, they didn't know why isotopes were separate; they didn't see any connection between isotopes and disintegration. Disintegration was radioactivity, but what did it have to do with isotopes? Well, the connection was established when the neutron was discovered. Because the neutron is what gives rise to isotopes because it creates heavy versions of elements by adding the neutral particle to the nucleus, you increase the weight, that's an isotope. So the isotope of hydrogen is deuterium and so on. Radioactivity, on the other hand, what does that have to do with the nucleus of the atom? Well, it's because the nucleus of the atom is disintegrating, or it's giving off particles and losing mass in the process. So the discovery of the neutron, in effect, joined those areas together, and now you have an integration of the two fields. And this was based on a keyword analysis. And I thought, "Wouldn't it be great if I could do that with the permuterm index?" I started working on it; of course, the difference between doing it with a confined, limited literature like physics, or nuclear physics even, and the broader field of science is that you don't have millions of homographic problems that you would run in to in a complete... because I wanted to do something that was over the whole database. I didn't want to do just one little field. I had big aspirations, you might say. And it turned out there were too many words that meant different things--that were the same word but meant different things, like the word plasma with blood and physics. Every time I tried to do something like this with the permuterm index, I ended up seeing a lot of false connections. Which to me...

TBH: Word patterns that actually were nonsensical.

HGS: If you going to use words, you have to do something much, much more sophisticated. So what was I going to do? The next step was to let me look at the citation index, why not? Why not look at the citation index?

TBH: Because it's there.

HGS: Just sitting there. It's the main thing at ISI; it's a thing everyone thinks of, it's a citation index--that's what it's all about. So I looked up a famous physicist, I always go to physics for some reason, it's like my first.... Murray Gell-Mann, I think by then he may have even won the Nobel Prize. Anyway, I saw a paper, and gee, look at all these, how many citations this paper got. Because this was the printed volume of the SCI, it showed the list of citing papers under each entry. I said, "That's interesting. Let me see. I'll pull these papers, these citing papers out, get them from the stacks." We called it the stacks; you could order all the citing papers and usually they were for some current period. I might have been looking at the quarterly index, for example. So they would be all papers from that quarter that were listed under the main entry for a Gell-Mann paper. I then looked at the papers that cited his, and I looked up all the references that those papers cited in addition to Gell-Mann. By doing that, lo and behold, I found some other papers that were also highly cited, and it turned out that they shared a lot of the same citing papers as the Gell-Mann paper.

TBH: They were co-cited.

HGS: I sought and gradually what emerged was a network. And that's what my first paper was about, [The Co-citation in the Scientific Literature]. Was a handmade network of papers that were highly cited and were highly co-cited. And people said, "Well, that's not really an algorithm," et cetera, et cetera. Well I knew that. What it showed to me was I couldn't find that in my earlier work because I had only coded the first five references in each physics paper. So I only had a sampling, maybe a 25% sampling of all the references I could have taken. Here I was looking at the whole thing and looking at a much broader set. This is only one journal; this is only the *Physical Review*. Here I'm looking at a whole set of journals that were in physics, and

catch all the references from all the articles. So it was a much stronger pattern. And two things struck me. One was the strength of the connection between the articles, the number, the sheer number of co-citing articles. And the other thing was the sharp focus of the topic. It was really a very specific topic. Which I should have known from the bibliographic coupling work because you can get these very tightly grouped thing. But it was striking to me. So I wrote it up, and this was probably within the first two months of my being at ISI. I realized that this was going to be fruitful. So I quickly wrote it up. Probably by summer, I don't know exactly when, I ran the article over to [Art Eliason?] at Biosis who was working there; he used to work at ISI, but he worked at that time at Biosis. And I submitted it to the American Society for Information Science, JASIS.

TBH: Was [Art Eliason?] editor of JASIS at that point?

HGS: I believe he was.

TBH: He was, but I don't know exactly what the years were.

HGS: I believe he was; I think he was. I know that he edited the journal. I know I wanted to submit it by hand.

TBH: Yeah, I know that was the period he was....

HGS: This was the period. I was actually taking a bit of a risk because I hadn't gotten permission from Gene or Mort or anybody to do this, but I was so excited about this article, and I was so determined. At this point, I knew that my *ISIS* article had been rejected. So I knew that I basically had come to a dead end in the history of science as far as presenting this type of work, and I needed to find another outlet for it. The obvious choice--I was working for ISI--was an information science journal. And JASIS seemed the place to do it, and that's how I made that switch. I felt, on the one hand, I would have preferred to do this in the history of science but the

history of science professors said, “Oh, we can’t have any of that stuff in our book; we’ve got to have regular history. We have to have narrative history; we have to have people looking at diaries, and letters, and stuff like this. None of this quantitative stuff, bibliometric stuff, we don’t want that.” So I hit this stone wall.

TBH: This is fascinating because I remember hearing, years ago, from Belver [Griffith] his claim that bibliometrics was the only part of the field of information science that was truly original.

HGS: A little bit of an exaggeration maybe.

TBH: Maybe. But he said everything else is derivative from the social sciences or history or psychology. And bibliometrics is truly... we can call it our own.

HGS: Sounds like a Belver-ism.

TBH: Yeah

HGS: He was in to making statements of that kind. I wouldn’t go that far. I definitely think it wasn’t; it was different even for information science.

TBH: But nobody else wanted it; it was sort of an orphan child.

HGS: Yeah, it was definitely an orphan.

TBH: And now it’s a very important part of information science.

HGS: It took years and years. I’m amazed it’s developed the way it has...Approaching the journal and getting an article published in JASIS was to me very important. I remember getting comments back on the paper from some external people who Gene had sent the article to. They weren’t that complimentary. There was still a problem, I would say, with this type of work, but at least I had gotten my foot in the door, you might say.

TBH: As they say, bad attention is better than no attention at all. Yes?

HGS: Exactly...Anyway...

TBH: Is it about this time you connected with Belver and the folks at Drexel?

HGS: Yes, it is.

TBH: Were you still down on 325 Chestnut Street?

HGS: Yeah, we were down on Chestnut Street. And I would come take the subway up to see Belver. As I mentioned, I have in my datebook the exact day that I met him. I met him first at ISI because he had worked with ISI a couple years previous. They had a conference, and there was a book published as a result of that conference in which Derek Price had participated and other people who were fairly well known. The book is a fairly prominent book, except I can't remember the name of it; I think its *Communication amongst scientists* or something like that. It was yellow. Most important characteristic of a book. But he had worked with ISI coming up with ideas, research ideas. So when I got there, they obviously wanted to get me together with him. Belver came in, although it was a short visit, and I think he was just there to see somebody else. He liked to come and check his citation index write-up. I remember that. That was the first thing, he would just go right up to the shelf "let me look up myself."

TBH: I'm sure he's not alone in that.

HGS: Yeah, I think so. So the first time I met him was in an elevator, well, as the elevator door was closing. And I heard the name as "Belva" B-E-L-V-A; he had this really interesting tide-water Virginia accent, like a southern gentleman type of accent. And he sorta was that way, as you well know. We made an appointment; I called him and came up here to Drexel. And we were sitting in his office.... I walked into his office, and it was just the craziest place I had ever saw. I mean, there were socks hanging on chairs; there was a squash racquet thrown around. He loved squash; he had equipment all over the place. He had gym shoes. "What kind of professor is

this anyway?” But he was into athletics; he was into sports; he was into sailing. He had pictures of boats on his wall, I remember that. And he would tell you, he would spend hours and hours telling you of his latest sailing adventure. It wasn't too long, we talked about writing papers, “yeah, we're going to do something with this clustering stuff and co-citation.” He had read my paper; he suggested I change something in the title, Gene also suggested. They made these little suggestions. Now sometimes I would ignore them; I would try to pick and choose which things that I would do, so I could say I was doing it. Some people said don't use the word “new” in your title because fifty years from now it's going to really funny. I remember that was one criticism.

TBH: It's a good point.

HGS: I kinda shrugged that one off because I wasn't thinking fifty years ahead, believe me; I was thinking about the next day, practically. But then we would go off to the Green Tree Tavern, which used to be over here. His office was down in the corner of this building on the first floor, near the entryway looking out in that direction. So I would come up from the subway and come up through there and then go into his office on the first floor. He had a nice office and a very wonderful secretary, I can't remember her name. But I spent a lot of time over here talking with Belver. I think I was, probably, talking with him once a week for that first year. We had many, many discussions. And so we became involved in doing the first papers that came right after my paper...which I listed them under acknowledgements, but they didn't really have anything to do with the paper itself...I know there was probably some political thing to be adding Garfield's name to it, for example. I didn't know what the etiquette was, and I may have stepped on some toes along the way, but I felt, and I still feel, if you don't have anything to do with the paper, your name shouldn't be on it. That's still my feeling, and a lot of people feel just the opposite.

They add everybody's name to it no matter what they did. Ok, well...nowadays there's a lot more "collaboration."

TBH: Team.

HGS: Team. Team stuff. So there's less of an emphasis on individual contribution which is ok. Anyway so the first paper that we wrote actually was a collaboration between Drexel and ISI. And that was the paper "Science Studies" and then that was a two part paper so we wrote two. But we had different grad students. His grad students, because he had an NLM grant, I was able to get an NSF grant, but that didn't kick in until probably the next year. So we had funding, and he could hire grad students. So we had [Sandy Day?] and [Judy Stonehill?], two of the grad students here at Drexel who worked on the algorithms, the clustering algorithm. And the thing that I did was to figure out how to automate the co-citation calculation from the tapes. I used the tapes just by sorting and permuting the cited references within an article. I was able to do this all with tape manipulation. In other words, in those days there wasn't a lot of memory in the computer that you could mess around with. At least at ISI, everything was done by sorts, massive sorts, that was how I was able to automate the creation of a co-citation index where you have an article here and it's co-cited with an article there and here's frequency and here's some kind of normalization factor. That was something that I feel was a major breakthrough because it allowed us to automatically create this entire file, this entire network of co-cited articles across all fields of science and we could do that just by tape manipulations. And then the next step would be to do the cluster analysis on top of that. We turned that over to [Judy Stonehill ?]and [Sandy Day?]; we gave them some sort of specification, I can't remember exactly what. But I really wanted to keep the clustering methodology simple so I just said let's do single link clustering, I'm not worried about anything too fancy. Later on, I know, efforts were made to

move to somewhat more sophisticated methodologies as far as clustering, but the single link worked surprisingly well for that kind of article level data because the links were so specific and so strong that you could create a reliable structure based on just the highly cited and highly co-cited information which is different from words; it's different from journals.

People say you shouldn't be using single link cluster, that's too simplistic. On the other hand, it works depending on the unit of analysis. It won't work very well for journals, as I found out later, and it won't work very well for authors either because the authors are also too general a unit of analysis.

TBH: What, I'm trying to think how to ask this. The criticism that you had been getting from the historians of science saying these patterns are interesting...

HGS: Bare bones...

TBH: Bare bones. You really need to supplement it with interviews, with surveys, with diaries, and so on. With you and the people who you were collaborating with did you attempt any of that or did you have anybody attempt to corroborate any of your findings?

HGS: Later on I did. I never got to the point, because we were doing this primarily using published literature and people who were alive; it was not easy for us to get diaries. But we could interview them and we did do interview studies for a couple of case studies I did later on. Like [1:46:53 ??College & King??] case study we did interview, survey; so we didn't collect diaries because these people are still alive.

We finally did something later in my career which was to create a physics citation index, I keep coming back to physics, I have to apologize, but we did this as an NSF grant, we created a historical citation index for physics, and we actually did some more traditional history of science type stuff, not a lot...

TBH: In your estimation, did the historians of science themselves, ever, ultimately pay any attention to this?

HGS: No. And somebody like [David Edge?] who was just a sociologist of science also didn't want to have anything to do with it, and that's why he wrote the essay "Why I am not a co-citationist." That was a very interesting essay. It was published in the newsletter of the 4S Society. Garfield reprinted it in his *Current Contents books*. But that was an interesting essay from our standpoint because it was basically a rejection on the side of the sociology people that they didn't want this kind of co-citation stuff to be adopted as the main goal of sociology. And sociology, at least the way the [Edinburgh?] schools thought, was supposed to have to do with scientists' behavior, preferably in the laboratory. That was a different point of view.

TBH: They didn't recognize there was value in complementarity?

HGS: Well, no, and that was surprising.

TBH: But you did have contact and even co-author, if I remember right, maybe not co-author, you did work with [Diana Crane?].

HGS: Yes, [Diana Crane] I got to know here, but she was really more interested in doing the kind of work that Belver was doing. And they were both somewhat in the sociological school because Belver had worked with [Nick Mullins?], and [Nick Mullins] was a social-network sociologist. So there's a lot of currents that are coming together here. When I first got to Philly and got to know Belver, I got to know his circle of associates, which didn't directly involve Diana. But I got to know Diana as somebody who was interested in bibliometric data, interested in tapping into the bibliometric [data?] which we hadn't done much of to that point. Her book, called *Invisible Colleges*, is really a review article, a long review article; she's not doing any original research there. So she wanted to get her hands into that data and so did Nick. [Susan

Crawford?) was another person; there were a whole bunch of people who came from sociology of science or network sociology, I'm not sure there was such a thing as network sociology at the time. [Nick Mullins?) was certainly one of the leading characters; [Lowell Hargens?) was involved, and then he was a sociologist.

TBH: [Hargens], that's not a name I know. H-A-R-G-I-N-S?

HGS: He collaborated with – H-A-R-G-E-N-S – he's still around; I talk to him every once in a while, nice guy, wonderful guy. Nick unfortunately passed away; he had cancer, Hodgkin's I think. And then Derek Price, it was a whole cast of characters that sort of eventually gathered around this. Now I was very fortunate because, in the first place I worked, it was also a kind of a meeting, a gathering place for people in the sociology of science and history of science, so I got to meet Kuhn--at AIP I got to meet Kuhn. I got to meet [Harriet Zuckerman?) and [Bob Burton?), he would come by, visit the local New York people, and I got to meet lots of historians of physics who would come through. Here, when I got to ISI, people would drop in who were interested in getting data, and then I would come here and I would meet all of Belver's friends in the sociology of science. I met Bill Garvey, who I used to work with; I met [Derek Price] who would come down to give talks here at the school. Derek and Belver were close buddies. The network just expanded; the network of my acquaintances that were involved in information and more in sociology.

TBH: So this was a real advantage being close to Drexel and Penn.

HGS: And Penn. Because at Penn I got to know Arnold Thackery. Because I was a history of science guy, I had known a guy, named [Rober Kohler?), I don't know if that name is familiar to you. I don't know if he's retired or not, but his wife was working at the Chemical Heritage Foundation for a while. In fact worked for ISIS, the journal. He was one of people who was

involved in editing the articles in journals. But [Bob Kohler] I had known from my AIP days and history of science meeting days; he got a job here at Penn, and his area of expertise was biology so he was like a historian of biochemistry. And I got to know him, but I also got to know Arnold. I attended the colloquium at the History of Science department at Penn. And then Arnold asked me if I wanted to be a senior fellow in their department so I was a senior fellow along with a bunch of other people that he had brought in. He was linked in with Burton, people like [?Al Kennah?], and a whole bunch of people at Columbia. And they went off and did a stint at the Institute for Advanced Study in the Behavioral Sciences and got us involved in a project that they were working on, which was a critique of the science indicator thing at NSF. That resulted in a joint paper I did with Gene and [Morton Allen?] on science indicators. I forget the exact title of it; it's in the book called *Toward a Metric of Science* that was a result of that whole encounter at the Institute for Advanced Study in the Behavioral Sciences out in Stanford, I think. We went out there at one point. I was getting to meet all these sociologists and all these people involved in studying science from a social-network standpoint. Belver was very interested in that, and he had worked with Nick on a paper where they talked about groups of scientists in scientific revolutions. He was trending toward more social-network stuff, more so than I was. My eye was always looking toward mapping science. The analysis of the whole database, rolling it up as high as I could take till I could see the specialties of science, for the document level then I wanted to group the specialties together. So I was very much thinking of a hierarchical structure where you could look at a map, like one of these maps I had here, but from the highest possible level. Where you see physics, chemistry, biology, psychology, neuroscience, all the fields of science laid out in a two dimensional space. And eventually, that's what we got to. I think Belver was a little less interested in that than working at the level of the individual scientist and the network...

TBH: And in communication...

HGS: In communication among the scientists because that was really where he was coming from. So I was kind of moving in that direction.

TBH: Well it's now ten after twelve.

HGS: Wow.

TBH: Yeah, but we've covered a lot of ground. But there's still some things, like in the middle of your ISI years, and you haven't talked about your contract activities, and things after that. But actually you've covered a lot because I was interested in your coauthors and other influential people and you've mentioned many of them. So I'd like to know what you want to do. Or do you think we should stop for today?

HGS: Well I'm willing to continue...

TBH: I mean do you want to continue now, and do you think we can wrap up in a half hour or so? About one o'clock?

HGS: That would be fine, if you're up to it.

TBH: Sure.

HGS: Let's just get as far as we can get.

[Brief break]

HGS: Now that I've picked up a head of steam here, I might as well keep going...I can't imagine anybody finding this interesting because of details about me. I mean who cares, right?

TBH: No, I think this is fascinating.

HGS: There's a lot of angles I haven't mentioned yet because I'm looking at my cheat-sheet here and I'm see all these things I haven't talked about.

TBH: Well, I think to get the proper scope for this, for the life of Henry Small...

HGS: Believe me, I'm not going to write the autobiography.

TBH: But you write very well.

HGS: It's not that interesting...

TBH: No, I think it is. How a boy from Illinois grew up on a farm and in a city could end up at ISI. What about some of these other activities that you did and the people you were connected with?

HGS: Well I was involved with the 4S Society, I don't know if you're familiar with that.

TBH: A little bit, yeah.

HGS: When I got here I looked around for societies to join. And obviously, there was ASIS (ASIS&T) and because I had submitted my article to JASIS...and I had already been going to the History of Science Society meetings, and made a couple of presentations there, so I was a member of the History of Science Society. And, as I said, I was a senior fellow over at the history of science department there. But there was also this group of crazy people that formed around 1975-76, something in that area, and they had started, I think their first meeting was in Berkley. But in order to understand that, I have to tell you about [David Edge] who was the editor of the social studies of sciences journal, it was called Science Studies. And that was where Belver and I published our first two articles, right? Then I published another one on college, and it was my first case study, few articles immediately after our two part paper. And I published them in the social studies of science journal where [David Edge] was the editor. I was active in the society, and the first official meeting at the society, I believe, was in 1976 where, it was in Cornell, and I presented work I had done with [Diana Crane] on social science citation index. We presented a mapping of the social sciences. Belver was a little annoyed with me because he wanted to do that with me, but Belver was very slow. He was so methodical, and so...because,

he had other issues. I just want to say, I love Belver, I love him as I did a brother, but he was very difficult to work with. He was so methodical and slow; he would just disappear for a long time. And just I couldn't deal with it.

TBH: You could say he had bit of a writer's block?

HGS: I think that's putting it mildly. When he got going, he got going very well. For example, we did in a series of interviews with a guy who got a Nobel Prize for his work on the hepatitis virus by the name of [Baruch Blumberg?], he was a Philadelphia guy. He got the Nobel Prize and worked at Fox Chase. We went out to Fox and interviewed him. We got all sorts of information about the cluster because we had this cluster identified with [Barry Bloomberg?]. And we said, hey, let's go interview him. You know that would be our first inclination. We got a cluster, it a contemporary object, it has all these different people in it, some are from New York, some are here and there, let's go interview these people. And that became a pattern that I'd repeat for different case studies that I did. Each case study would be based on a different cluster so I was being motivated by the data that we were generating from the science citation index. We had all this data on something called "Australia antigen" but turned out to be the discovery of the hepatitis virus. He called it "Australia antigen" because of the reaction that he got from Australian aborigine blood. You know, one of these weird discoveries. But Barry was very open and he talked with us, got a lot of information. But Belver never wrote it, and that was a pattern with him; we never did publish that, which is a really terrible shame because we had all that good data. Ok, well, I was going to start talking about the 4S Society... [Society for Social Studies of Science]

TBH: You say it got going about '75, that by '77 you were the editor of the newsletter.

HGS: I didn't become the editor of the newsletter, and I was on the board of the Society for a while. Ultimately the group proved to be hostile to our work, to quantitative work; it's not unlike the history of science was hostile. Because that's where social constructivism was developed. There was a group of people from Edinburgh, among them [David Edge] but also [Barry Barnes?] and [David Bloor?] and [Mike Mulkey?] and, I could go on, [Steve Woolgar?], all very nice people, but very committed to the idea that science is a socially determined, not a rational enterprise. That it was essentially constructed by social forces. And, of course, that will reject anything in the eternal history of science, any sort of notion of "discovery" or Kuhnian type of revolution that goes out the window. Everything was relative, knowledge was relative; there was no absolute ground in truth to science. And I couldn't accept that because part of my background in science, and so forth, I didn't see the need to go to that extreme. Eventually the French guys got involved because they were the original deconstructivists, and you have people come in like Foucault and Bourdieu and Latour and many, many others. Some big names, and there was a literary side to this whole thing which is continuing even today--it's flourishing; it's dominant in sociology, almost. And it's anti-quantitative. So anything quantitative was immediately suspect because it was "objective," it was "scientific," it was, obviously not to be trusted. I wish now that I had written an article supporting and showing how bibliometrics supports the rationality of science--that supports the objectivity of science. Because I think that citation data is, in fact, one of the most important advocates for the truth of science as a field, as a human endeavor. To say that this all a figment of people, like evolution is all a figment of somebody's imagination, then I might as well be a creationist or I might as well believe in anything. If I get enough people to support my idea, I can say the world works in a completely different way and get away with it. Well I don't think that's true, and I think it has to do with the network of evidence that gathers

around and is evident as shown by the citation database. The network or reasoning that leads one from one area of science to another area of science actually forms a network of confirmation.

Now that doesn't mean you can't have a mistake or a fraudulent piece of research that will corrupt that network and may have to be pulled out of there. But by and large...

TBH: Because of the sheer numbers of independent work?

HGS: Yeah, because you've got independent minds working in concert, forming trails of dependency. My work is dependent on yours.

TBH: But doing it unconsciously, I mean, in a sense, forming the patterns unconsciously because they're each just working independently.

HGS: Yeah, to a large extent they are working independently. They're confirming or disconfirming each other's work, but it's still a network, they're still working within this system. And it's not as if we can just ignore that and say global warming doesn't happen because I don't want it to happen, because my beliefs are this. And my beliefs are just as valid as yours because everything is relative; there is no truth or you can't say this is more likely to be true than that. It's all about that. And that's what I saw the 4S Society going toward.

TBH: Well, do you think it had to do with the name of it? The 4S is social studies of science.

HGS: Yeah, it did, to some extent. They tended to be anti-science; they tended towards wanting to critique the authority of science. That science does not have any authority over our lives. They wanted to very much say that because science is totally socially constructed and determined by social forces and that it's got nothing to do with what's true or what's real, it's all about what people want it to be. That they agree to move is the case. So that's why I felt uncomfortable, and they felt uncomfortable with us. So we had this [David Edge] article and a big argument at this first meeting in California. And I got up and objected to what [David Edge] was saying. He was

criticizing our work, publicly, and I objected to that. Now at that time, I didn't feel like I had to quit the group, in fact I continued with the group, and I became the editor of the newsletter, despite the criticisms and the hostility that I was beginning to feel from the members, particularly from the European members, the Edinburgh people for example.

One time [Steve Woolgar] came up to me, I was in the men's room I think, we had an intellectual encounter in the men's room where he accused me of believing in reality, or something of that nature. I can't remember what it was, but it was sort of a moment I couldn't get out of my mind: here's a guy coming into the bathroom and arguing with me about...

TBH: Was this after happy hour, maybe?

HGS: That I shouldn't believe in the objectivity of science. It just seemed so bizarre. And Belver sort of shared this view, and the social network people didn't have that concern at all.

So at our first meeting, I was going to say that these 4S meetings were very famous for huge arguments among fairly senior people, and at the first meeting of the society where I gave a paper with [Diana Crane] who was not at all of this ilk, I mean, she was a sociologist but she wasn't a constructivist. There was a big argument between [Derek Price] and a guy, an historian of science, [Everett Mendelsohn?]; I'll never forget this because it just sort of boggles my mind to this day how people could get up in public and say such horrible things about each other and how academics were not immune to this nasty sort of rhetoric. It had a little bit to do with the battle between quantitative methods and qualitative; with Derek supporting quantitative and [Everett Mendelsohn] being more on the side of the qualitative. I should have realized at the time that eventually this was not going to pan out, that we would have to split up, and go our separate ways.

TBH: So nobody was thinking at that time, complementarity?

HGS: No, my way or the highway, basically. But I got to meet some really interesting people; I got to meet [Bruno Latour] who was working at *Laboratory Life* and *Science in Action*, and we actually helped him collect data from the science citation index which he used in his different books and studies. And he was very much of a relativist, at that time anyway, and a constructivist. I was playing both sides of the street, as it were, and even in one of my later papers, I came out sounding a little bit like a constructivist, and that was “Cited documents as concept symbols” paper where I basically said you can cite a paper and construe it in any way that you want, and, in fact, a consensus can evolve in the community about what that paper means which is diametrically opposed to the actual meaning of the author of the cited work. So I was kind of coming out for a constructivist view of the act of citation, and people point that out to me quite often that “you were a constructivist back in those days”...not exactly, not exactly.

TBH: I have a recollection; tell me when I would have known this, or learned it. Was it Garfield who wrote a piece about why people cite and he had what 12 reasons?

HGS: A lot of people wrote papers like that. One of them was working there, named [Mel Weinstock?], was working at ISI.

TBH: Maybe it was [Weinstock]...

HGS: I think it was [Weinstock], he was a really interesting character, a very funny guy, and bright, and somewhat of a scholar also; he wrote papers with Gene and for Gene that were pretty good. But Gene also wrote about it; and also, I wrote a review paper on this, all that people who making citation classifications or reasons for citing. A whole history of people writing papers about that. Of course, I got very interested in citation context or what are now called “citances,” the passages around the point of citation, the sentences around the point of citation, very fascinating stuff.

TBH: One area that you haven't touched on is beyond Belver, what was the nature of your other connections here at Drexel? [Howard White] was here, [Carl Drott], later [Kate McCain].

HGS: Yeah, Carl was very early. Carl came early. When I first started to come over here to talk to Belver, we had a session here-- might have been Derek was involved too-- but I saw this guy get up and make some really impertinent remarks. And I said, "Who is that student? Who is so rude?" "That's not a student; that's professor Drott..." Carl was one of the very interesting characters.

TBH: It's pretty interesting hearing your perspective on this because when I was a doctoral student here--I started in '79 and I got my PhD in '84. I sat in on a number of those sessions, Derek and others came, and I was shocked at the...

HGS: The frankness of the exchanges...

TBH: Yeah. Where I came from, the University of Kentucky, they didn't talk like that. They didn't insult each other, and they weren't so brash. It was a culture.

HGS: I think part of it was Carl and Belver like to play off each other that way. But later Kate came, and then I think, Howard, but that was quite a bit later.

TBH: Well Howard was here when I was here; he was a very junior professor.

HGS: That's right.

TBH: And Kate was a doctoral student; she and I were doctoral students at the same time.

HGS: Ok, I had that a little bit wrong. I can't remember exactly where they came in.

TBH: I don't think you ever explicitly coauthored anything with Kate, but I noticed a couple of times you'd be in her acknowledgements...

HGS: No...She's always been a great resource because she knows the literature so well, and we share some research interests. I think when they came up with author co-citation that they went

off in that direction and I more in the mapping side of things. I never got that interested in author co-citation because that seemed to have more to do with social network, which I wasn't that crazy about pursuing as a research area. I was more interested in looking at the whole file, trying to find patterns, and doing science and maps, and stuff like that. But to finish the 4S thing, I did become editor of the newsletter; worked with [Jerry Cast?] and a lady by the name of [Riki Kuklick?], over here at Penn. And that was when I was working, I got to know Riki...she was the junior faculty person at the time, I'm not sure what school she was in, but she was very bright. We worked jointly on editing this newsletter for the Society for Social Studies of Science. Then we finally went our separate ways and that was a period of time where we didn't have a separate society. But eventually in '83, in the '80s, we formed the International Society for Scientometrics and Informetrics, that's when we had an official society to do this sort of stuff, and we could all talk to each other, up to that point.

TBH: Amongst friends...I wanted to come back to a statement you made early on about when you were first hired by ISI. They weren't sure whether you could ever make money for them, but you did. And I notice that after '78 your title was Contract Research and Chief Scientist. So tell me how you made money from them.

HGS: Well, initially all we did was to get grants and contracts from National Science Foundation, National Institute of Health...

TBH: These were all to do research?

HGS: They tended to be evaluation type grants. They would have programs. It was very applied research, for the most part. Some of it was basic because we got our basic funding to do the mapping stuff from NSF and from NIH, from [NLA?] specifically, not from NIH. So we had a number of grants that enabled us to do this, and it totaled a few hundred thousand dollars, up to a

certain point. I can't remember if that's the point where the title change occurred. The history of my involvement with ISI has been gradually more and more commercialization. The most recent period has been extremely commercial because we actually had bibliometric products that were available - online. For a long time we didn't have any "products," per se. The first sources of income were grants and contracts. The next wave was sort of like doing customized studies for people.

TBH: This is the kind of thing that [?Elizabeth Deberso??] was involved when she worked for you?

HGS: Yeah.

TBH: For the record, these customized studies, you would go into a large organization. What kind of organizations?

HGS: It could have been a university, could have been a country. We did a lot of work for foreign governments. Occasionally, it was a company, like...

TBH: Like a pharmaceutical company?

HGS: Like a pharmaceutical company...But most of it was not... most of it was for universities. We were creating a dataset. Somebody wanted to know all the papers written by people at Temple University, or a NIH program wanted to know all the papers that they wrote...

TBH: Because they wanted to establish the impact of their organization?

HGS: Then they wanted to look at the scientists, who were working and how often they were cited, who they were collaborating with. It very often had an evaluative component to it.

TBH: To demonstrate worth and value?

HGS: And then we would actually also be involved with selling the entire database under a contract, under a leasing arrangement. We had for years and years a contract with the National

Science Foundation to do the science indicators. And one of the reasons I was hired was to try to get that contract away from the people who had originally gotten it, the CHI group which was in New Jersey under [Fran Narin?].

TBH: What is CHI?

HGS: CHI, I think it was originally Computer Horizons, Inc., but I think they changed their name to be under the acronym. They were in Haddonfield, New Jersey. They were the ones who for years and years got the contract with NSF to do the science indicators which were the counts from different countries and tended to be national and field statistics that were then rolled into this biannual report called Science and Technology Engineering Report which was done by the National Science Foundation.

I can say this now to you and on tape/ tv, here, that I never really wanted to get that contract. It was a lot of work, a lot of number crunching and a fair amount of hand-work just to unify all the variant names and things. And it would have been completely all-consuming...

TBH: It was very labor intensive.

HGS: I had a very small staff. And I really had my eye on these other research issues that I wanted to pursue. So I'm saying this, if the contract didn't come our way, if it had been available to us (which it wasn't) we would have pursued it; we would have done the work. But as it was, we sold the data to NSF so that was a piece of luck. We would then turn around and provide the data to other countries...each country would evolve its own homegrown science indicator program. You had the French operation, for a long time they didn't have anything in Germany, but there was stuff going on in the UK and in Japan.

TBH: So they were not replicating the ISI database in any way, more specific to their countries. They were just using your data?

HGS: Well you had to have the whole thing because it wasn't enough to get your own country's papers, usually. You could do something with that, but in order to do a comparative analysis you had to get other countries as well. Now you could limit, use half a dozen, sometimes you can do that, just go for the EU countries, let's say, or go for the Pacific Rim countries, whatever. There are different ways of slicing and dicing; we had every way in the universe to slice and dice the data. And we created bibliometric datasets not standard bibliographic ones. Bibliometric datasets are pre-massaged in order to be unified, to be numerical in their output so that you get impacts, you can calculate percentile, you can calculate each index's, you know, the whole thing. It's very much now a routine business kind of thing that not only a company, like Thomson Reuters is involved in, but the people at Elsevier. It's big business now. And that's one of the reasons why I thought it's probably time for me to get out.

TBH: So you retired in 2010?

HGS: Well I didn't really retire; I went to work for this other company.

TBH: Yeah, but I mean when you retired from ISI, and then you went to work for this new business.

HGS: Scitech Strategies.

TBH: Scitech Strategies. And they're around here?

HGS: Well, the guy who runs the company [Richard Klavans] is from [Paling??]. I shouldn't say he runs it--he was the originator of the company, the founder of the company. But then [Kevin Boyack?] came in, and he's from Albuquerque; Kevin is now the president of the company so he's really running things now.

TBH: Small world. I actually was office-mates with [Judith Klavans?] at University of Maryland for several years.

HGS: I don't know if that's a relative or not; I think he told me once that his sister is a different Klavans.

TBH: Really? Because somebody else told me, when in preparation for this, that he was either married to her or a sister of [Judith Klavans].

HGS: Could be.

TBH: I just took their word for it.

HGS: But, I'm pretty sure he told me she was...

TBH: I know who told it to me now. I was chatting with Howard the other day because I'm going to be doing an interview with him too, and he's the one who pointed out the connection. But he may have made the assumption...

HGS: Ok, well, news to me. Because I was told his sister's in New York and worked at Columbia University so I'm not sure...

TBH: Well I don't think she's at Maryland anymore, so...

HGS: Maybe she was, maybe this is the same person...

TBH: So what's the nature of your work for this company?

HGS: We're working on a project for the government on emerging areas of research, and I was brought in, and we also have access to the Elsevier group's data. For me, it was an opportunity to do something that I always wanted to do, and that is combine bibliometrics with full text and then be able to automate the extraction of citation context. What I call citation context, what other people call citances--it's what people say when they cite a paper. So now I can go in, and I can take one of their research funds, they use co-citation, they use other techniques as well. And I can pull in a fair amount of full text for the citing papers for that research fund, for that cluster, and show how the papers are being cited from the standpoint of the full text. Something I always

wanted to do and I had only been able to do it by hand. At ISI, or Thomson Reuters, I had to get the journals, I had to look them up, I had to photocopy the articles...It was just a pain. And when this opportunity came up, I said, "Geez, I've got to take advantage of this" because not only are they going to have the citation data--at this time it was [Scopus?], it wasn't ISI, but still it was citation data, and they were going to have a substantial number of full text journals from which I could automatically extract the citance, citation context information. And then I could begin to linguistically analyze that in connection with the citation. It was like the pieces of the puzzle that finally come together.

TBH: A "citance" is spelled C-I-T-A-N-C-E?

HGS: Yes, it was kind a little "name" that was invented by somebody out, [Marti Hearst?] actually, but I had called them all, for many, many years, citation contexts. Some people call them citation contexts, some people call them citances, but basically it's just the sentence of the cited. So I've really been doing, I would say, mostly basic research with that information for Dick and Kevin. I've been trying to identify linguistic markers for emergence to go along with the bibliometric markers because it is the bibliometric profile that emerging area is very strong. It has growth characteristics, it has characteristics of the cited references being very young, it has other characters like when there's a big innovation in a field of science you have growth in the area, the citing of new papers, new papers becoming highly cited, and you have the continuity of those highly cited paper in the future. All of these factors can be combined into an indicator for emergence. So that's what we've been engaged in, what I've been engaged in, in particular.

TBH: Who's the target for this? Who's the audience?

HGS: We have a sponsor for this project, and it's a big defense department, it's IARPA [??]. I am not directly involved in that project, but I'm sort of supporting Kevin in his role so I'm just

happy to be involved in this at my age and doing what I feel is really interesting stuff in a way I was never able to do before. Even though it's not exactly the "methodology" that I came up with originally...

TBH: The computing power in the databases, and all that, are available now...

HGS: But enough of what I did, and what Gene did...It has very close ties with what Gene did with historic graphs. We're plowing over the same field; we're just finding new ways of looking at it. And I don't have to go anywhere, I don't have to go to the office; I can work at my house and we're all telecommuting with each other...

TBH: Well do you have intellectual contacts now? You described Belver's old intellectual community.

HGS: I go to meetings. I went to the ISSI meeting which is the group I was president of for a while, and I maintain those contacts. I have many colleagues around the world that I enjoy meeting and talking things over with. I occasionally collaborate. I collaborated with a guy in France, by name of [Michel Zitt?]; we came up with a new method of counting citations based on a fractional counting of data which I had proposed back in the '80s. And he decided he could develop it into something on a journal level which I think is a really nifty idea. Also before I left ISI, I was working with a guy at the Wharton school, one of the grad students there. So I collaborate a little bit; primarily working with Dick and Kevin though.

TBH: That's great. Do you have anything still professionally on your wish-list? That "gee-I-wish-if-I-only-could"?

HGS: Yeah.

TBH: Still the curious kid?

HGS: As I was mentioning, I feel like we really haven't made the case for the importance of science and the rationality of science to counter the anti-science bias that seems to be out there in the world.

TBH: Do you think it's in the world; it isn't just this country? Sometimes I despair of being an American.

HGS: Well, there's a lot of it in this country which is really, really unfortunate. It's directed at the findings of science, not just scientists. It has to do with funding, the inadequate funding of science. The lack of appreciation of a technical education; of the scientific, mathematical underpinnings of our modern society. I mean, it's so ironic to be talking about this; here we are in the middle of a government shutdown, in a sequestration. It's hurting people; it's hurting people at NIH; it's hurting patients; it's hurting researchers all over the country.

TBH: It is. I mean, it's really killed some scientific research projects that are time dependent; I mean they're dead.

HGS: Yeah. I know so many people who are hurt by this, and it's so stupid, and they're not asking for a lot of money, compared to what the defense department gets, to what we're spending in Afghanistan...

TBH: No good outcome to this.

HGS: This is what's really bugging me, and I'm saying how can I use the information and the knowledge that I have in this world of bibliometrics and citation and analysis, and my version of the sociology of science, which is not a constructivist version, it's not the kind of thing that says science is just a bunch of arbitrary social conventions. It's action; it means something. It's a measure of the truth value of information. And if we can get that kind of message out into the world, if anybody was willing to believe it, which I "seriously" wondered sometimes whether

people are even open to this rational discussion. To justify the funding and justify our support of science and our belief that it is a source of important authority. That would be an ultimate objective for me, but, you know, I'm getting old; I don't know if I could even tackle something of that magnitude. But I remember the AAAS, all the dire warnings and so forth about what's happening to American science, and I feel bad. I feel like I ought to be doing something, and I'm not sure how I could do anything...

TBH: It probably needs to be tackled on many fronts including going back to elementary, high school education which is in very bad shape in many places...

HGS: That's for sure. I'm not a science educator, per se, but I feel like, if there's some way I could use what I have been researching on all my life to support the recognition that science is an important thing to do. If there's something I could do. I know Gene shares the same feelings as I do on this.

TBH: To be a ghost writer for somebody who is a scientist who will run for high office maybe. A speechwriter.

HGS: I don't know if I could fit that description or not. This is just something that bugs me. There's only so much more research that I'm going to be able to do in my lifetime, but this is something that I feel strongly about. And I feel like maybe there's something I could do, maybe not. But in the meantime, I enjoy going to meetings and meeting colleagues; I'm not even adverse going back to the 4S meeting which is now a humongous meeting, I mean just incredibly huge.

TBH: But you're not going to the ASIS&T meeting?

HGS: I didn't plan to, but I feel bad about my not going... You're bringing up all these issues that I feel guilty about... I feel like I should go back to ASIS&T more than I have; I just haven't...

If you think this is enough, we can knock it off here... I've skipped a lot of years in between.

TBH: It was a lot of years, period... This that you're writing now, will this be published somewhere?

HGS: Maybe. I think so. At least be a document that I can put up somewhere, and maybe it'll be published. People have talked about wanting to get something up. I don't want to come across as somebody who's just focusing on their life and their experiences; I'd rather be focusing on the future. That's always been my problem. I realize I'm a historian of science originally by training. So people, "Why is he doing this history of information science?" And I've always felt a little funny about that, and the reason was that I do agree with Bruno Latour in some sense that it's better to be a participant of than an observer of something that you've never been involved in. You can learn more from, let's say, entering a tribe, being like an anthropologist, going and experiencing something that you've never experienced before because you can see it more objectively. If I do the history of information science, it's going to be a biased view; it's going to be from my perspective. And I don't even think it's all that interesting to dredge the history of co-citation analysis, clustering, or any of this. People can read the papers; they can see what happened, it's a pretty open book.

TBH: Others, Garfield, have documented a lot of this, very well too.

HGS: Yeah, it's been documented; it's been pretty thoroughly documented. I've always felt that being a native at something, you're better off not doing the history of what you have been doing, but doing the history of what others have been doing but you haven't been involved in.

TBH: Assuming you're willing to spend enough time to understand what you're seeing.

HGS: Yeah, it's a lot of work, but then you have to immerse yourself in it. Like I said, looking to the future and doing a little more original research is really what my priority has always been. We're in this funny race with the data. We get new data every year, and every time there's a new Nobel Prize, there's a new discovery, that's really what gets my juices flowing. A discovery in science, something we didn't know before, that's phenomenal. And if I can look at that from a bibliometric standpoint and understand how that manifests itself in the network of citations with the co-citation that to me is the most exciting thing. And every year there's a new discovery, so I'll never be able to quit. Because the future is where it's at; it's where the excitement is. I almost think it's sort of like farming which my grandfather did; every year there's a new crop and there's something in that crop that's going to astound you and that's what I really enjoy the most. I'm really very present oriented; I don't like reflecting too much anymore on the past. I like looking at the present and the future, I really do. Maybe that's why I'm a bad historian.

END OF INTERVIEW