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CONVERSATIONS WITH HOWARD SHEVRIN
I
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Summary: Howard Shevrin is born in 1926 in New York. During World War 2, he is a front line soldier in Germany from December 1944 till February 1945. After the war, he obtains his PhD in psychology and child development at the New York Cornell University. In 1954 he joins the Menninger Foundation in Texas, where he starts doing subliminal research with Lester Luborsky and Charles Fischer. He also completes his analytic training there. In 1973, he joins the University of Michigan as a professor of psychology and as chief psychologist at the Department of Psychiatry. With the help of Bob Berry, he founds *the Ormond and Hazel Hunt Laboratory* and starts doing research with Bill Williams, head of the neurology EEG lab. In the following years[2], a number of post-docs join him, among whom Linda Brakel, Edward Bernat, Phil Wong and Michael Snodgrass. In one of their major studies, Shevrin and colleagues show that psychoanalytic clinicians are able to derive the unconscious conflict rationale from the subject's accounts, which is consciously unrecognized by the subjects themselves, but nevertheless recognised by the brain EEG characteristics

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Life

Ariane Bazan (A.B.): Thank you very much for agreeing with this interview Dr. Shevrin, I would like to ask you some questions about your life, about your work and about psychoanalysis. You were born

1. All footnotes in the "Conversations with Howard Shevin" are from the author.
2. Not mentioned in this interview but nevertheless important to know is that, parallel to his research, Howard Shevrin has continuously maintained an active psychoanalytic practice till his retirement in 2005.

in 1926 in New York. Tell us about your first encounter with psychoanalysis.

Howard Shevrin (H.S.): I first heard about Freud and psychoanalysis when I was an undergraduate student taking psychology courses. I was always interested in reading the philosophical psychologists. People like James, Dewey and Angel who made up a school of American psychology, called the functionalists. They wrote at the turn of the last century late 1800's, early 1900's, and I was very taken by them, especially James. Then, when I took courses as an undergraduate in psychology I came across references to Freud. Actually before I went into the service, I had not taken any psychology, so in chronological order whatever I heard about Freud was very indirect. I never read anything of Freud until that time in Oxford.

A.B.: So you were in Oxford at that time?

H.S.: Yes, I was recovering from a war injury in a military hospital in Oxford. I was once in the library, a very nice little hospital library. And I came across this book called *The Interpretation of Dreams* by Freud (1900*a*). It was the Riviere translation. As I started to read it my reactions were extreme. I was fascinated and at the same time I was shocked. You might say disbelieving. But I always returned to reading it no matter how often I was shocked and not really believing what Freud said.

A.B.: How old were you at the time?

H.S.: I had just turned 19. When after several months I recovered and it was time for me to return to my army unit, I went to the library and stole it. People don't steal books from the library but I did. I felt this is what I'd like to hold on to. I still have it on the shelf of my library at home. You can see it is stamped, if I'm not mistaken, "Winston Churchill Hospital Library".

A.B.: So you were in the Second World War as an 18 year old?

H.S.: Actually, I was two or three months short of 18. It wasn't very heroic, I might say. My father had found out that if you volunteered, they'll send you to a university. First they teach you something before they actually send you to combat training, I volunteered and I was sent to Cornell University. It was called *The Army Special Training Program* in which they were trying to give young men some college background and to teach them various things that the army might find useful. I was sent to Cornell to become an engineer, something I was not interested in. I was there just for a semester, taking courses in chemistry and physics. Then the army decided they needed these men who studied at Cornell to carry a gun. So, within four or five months,

we were shipped out and I found myself assigned to "basic training" in Georgia, a southern state where I spent about 12 weeks learning how to become a soldier. I was in the infantry and ended up being a 60 mm mortar gunner (see Fig. 3).

Fig. 3: The M19 Mortar was an American 60 mm mortar developed in 1942 from the Mortar M1 during the Second World War. The M19 Mortar fired a M49A4 high explosive round to a minimum range of 45 meters and a maximum range of 1814 meters with a rate of fire of 8 rounds per minute, or 18 rounds per minute for short periods.

H.S.: A mortar looks like a little piece of plumbing, really a small 60 millimetre tube. It has a ball joint at the end that you fix into a plate flat on the ground. It has a sight that makes it possible to fire the mortar at any distance and direction. It's known as a high angle weapon, meaning that it can shoot over a hill, not like a rifle straight ahead. The mortal shell looks like a little missile about 20 inches long, weighs about two pounds. You feed the shell in from the top of the tube. As the shell slides down the tube it gathers momentum so that a pin fixed in the bottom of the tube can perforate a cartridge in the shell which then explodes like a bullet and sends the shell back out of the tube with increased velocity. So, that's what I actually carried around. You get trained with all kinds of weapons, but they decided I would be a mortar gunner and I was sent to an infantry division in Virginia that was getting ready to be sent to Europe.

A.B.: And so you finally went to Europe.

H.S.: We left for Europe on what is our Columbus Day, October 12th 1944. First we found ourselves in England for about 2 months where we underwent further training. That was very nice actually. It was in one of the cities in England, Bournemouth; that is on the channel. Of course we had to do a lot of training so we didn't have too much free time, but when we had free time, it was pleasant. We were there two months until early December and then we headed across the channel and we landed near Honfleur. We pitched our tents in the fields. It was cold, as you might imagine. So when I had free time I would wander into the outskirts of the city, and I remember one time there was this little fair going on with merry-go-rounds and there were these kids. It was dark because they didn't have enough electricity. They were very careful about the light. I remember reacting to it, "My god, this is a fair, but it looks like somebody's nightmare." Although the kids were very subdued they were having some fun.

A.B.: So Honfleur was liberated at that time?

H.S.: Oh yes, by that time France had been liberated, and the Germans were just on the other side of Aachen, that's where we were heading. We were put on trucks and were transported to Belgium. We were heading north, I think in the direction of Rouen. We had Thanksgiving on a very rainy day. It was practically a storm. We were sleeping in two, three feet of deep mud and it was cold. In the midst of that, it was Thanksgiving time, so they brought out food for us, turkey and all the trimmings. We had these mess kits, they were called, that were made of tin. Once you unfolded them you had two different compartments. The cooks would throw the food in and the rain would cover it. You were drenched through and through. And then we went to Belgium by truck. I remember we went through this city, it was very early in the morning, people were crossing a square, and I saw a sign that said Tongeren. We went through that and we ended up in this little village. Apparently not a village that most people know about but I remember it very vividly, it was called Membruggen. There we were billeted in a farmhouse. The farmer had a free room and he put a lot of straw down, and we spread our sleeping bags on the straw and that's where we would sleep. We were not far from the frontlines. It was extremely cold, bitter cold, deep snow. People had not much to eat and the cattle were starving.

A.B.: You ended up on the front.

H.S.: Yes I was in combat roughly from the beginning of December 1944 to the end of February 1945. That was all in Germany, it was called the Siegfried line (see Fig. 4), but it was not

well defended. At first we had an easy time. Our main enemy was the weather. Many American soldiers in a matter of hours developed frozen feet and gangrene. When it was not caught in time they lost toes and feet. My feet were frozen, everybody's feet were frozen. It was painful when they thawed out.

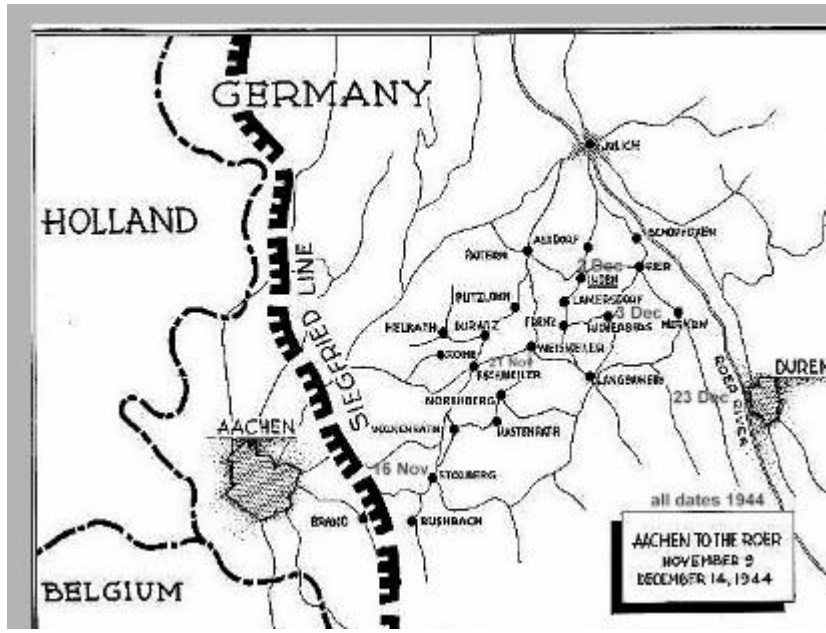

Fig. 4: The Siegfried line was a defense system stretching more than 630 km with more than 18,000 bunkers, tunnels and tank traps. It went from Kleve on the border with the Netherlands, along the western border of the old German Empire as far as the town of Weil am Rhein on the border to Switzerland.

A.B.: And it's that injury that actually brought you to London?

H.S.: That's right. That was the million dollar injury because it left no lasting harm and it got me out of the rest of the war. Just when I left, the American army was beginning it's spring offensive. That's when they captured the Remagen Bridge[3] crossing the Rhine and that

3. The Ludendorff Bridge at Remagen – the last standing on the Rhine – was captured by US soldiers on 7 March 1945. Although the bridge's capture is sometimes regarded as the "Miracle of Remagen" in U.S. histories, ultimately, only a limited number of troops were able to cross the Rhine before the bridge's collapse. However, the psychological advantage of having crossed the Rhine in force and in pursuit of the retreating Wehrmacht improved Allied morale. Despite the best U.S. efforts, on 17 March 1945, ten days after its capture, the Bridge at Remagen

was the beginning of the end of the war in Europe. That was spring of '45. And so I was glad to get out of it although surprised that I did because it didn't seem much like an injury to me. Anyway, I got back to the United States, I returned to college, I finished my undergraduate degree, I majored in psychology at the City College in New York. Then I went on to Cornell University to get my masters and PhD in psychology and child development. Then I taught for a year at a small school called Vassar. When my contract was not renewed, I discovered there was a place called the Menninger Foundation. I had read a number of Karl Menninger's books.[4] He wrote these popular books which sold many copies: essentially very nice clinical material all from a psychoanalytic point of view and I was very impressed with those writings and with what was going on at the Menninger Foundation in Kansas. Since I had no clinical background and no clinical interest, it was nice to read. But I was heading for an academic career. However, I needed a job. I had a wife and a kid by then. I saw a flyer about this postdoctoral scholarship at the Menninger Foundation and I thought I didn't have much of a chance but I sent an application anyway because the stipend for the scholarship was more than I was earning as an instructor. So I applied and was not really expecting anything to come of it. To my surprise I received a letter that I would be interviewed for the fellowship at a psychology meeting in a nearby city.

A.B.: What were you looking for at that moment?

H.S.: Mainly I was looking for a teaching job, but the fellowship would pay me more money than I was earning then. It didn't appeal to me to go out to Kansas, to live in the Midwest, in a red state as we call them now. And Aliza [Aliza Shevrin, Howard Shevrin's wife] wasn't altogether excited about going out there, we were big city New York people![5] At the meetings I met Herb Schlesinger for the first time who

succumbed to the cumulative damage from German bombing and collapsed. Still, the Ludendorff Bridge remained important as the first point at which Allies crossed the Rhine.

4. Karl Menninger (1893-1990) was an American psychiatrist and a member of the famous Menninger family of psychiatrists who founded the Menninger Foundation and the Menninger Clinic in Topeka, Kansas. The Menninger Foundation is a well known treatment and training center for psychoanalysis in the United States. It is known for its openness to innovation in psychotherapy.

5. Aliza Shevrin is a native Yiddish speaker and is considered the foremost translator of Sholem Aleichem, having translated eight other volumes of his fiction, including many children's stories. Sholem Aleichem (1859-1916) was the pen name of Sholem Naumovich Rabinovich, a popular humorist and Jewish author of Yiddish literature, including novels, short stories, and plays. He did much to promote Yiddish writers, and was the first to pen children's literature in Yiddish. His work has been widely translated. The 1964 musical *Fiddler on the*

was an analyst. He asked me a question that I was totally unprepared for: "Are you interested in coming to the Menninger Foundation because you want an analysis?" Well, I knew what an analysis was of course, but I couldn't say "Oh my God, the reason I wanted to go there is to get an analysis." What came in to my mind was the time they asked Marie Antoinette: "People have no bread", she replied: "Let them eat cake." I wanted bread and he said: "Do you really want cake?" So I said, "No, that's not my interest." I met another person at the meetings, the chief child psychologist, and she conducted a more or less ordinary interview. Well, I got the fellowship to my surprise. And that started my career at Menninger's in 1954.

A.B.: And were you primarily interested in psychoanalysis?

H.S.: No. I certainly had an intellectual interest in psychoanalysis, but I was already by that time interested not only in an academic career, I was interested in doing research. But the thought of getting some clinical training and treating people was also appealing to me. Unlike other fellows at the Foundation, it was not my primary interest, it was another important interest. The Menninger Foundation was an excellent place, with excellent training and excellent opportunities. Within the first year I started to do some serious research with a couple of senior people. The first one I started working with was Lester Luborsky, who was really one of the outstanding figures in psychotherapy research.[6] He and I started doing subliminal research[7] together on the Pötzl procedure.[8] Another person who I got to know

Roof, loosely based on Sholem Aleichem's stories about his character Tevye the Milkman, was the first commercially successful English-language play about Eastern European Jewish life.

6. Lester Luborsky (1920-2009) received his PhD from Duke University, becoming a clinical psychologist who spent his lifetime career bringing two worlds together – psychotherapy and scientific research. He went on to the Menninger Foundation in Topeka from 1946 till 1959 where he received his training in psychoanalysis and research. From 1954 till 1959 Luborsky collaborated at the great "Menninger Foundation Psychotherapy Project". He was pivotal in the development of the Health-Sickness Rating Scale (HSRS) which was used in the project. He returned to Philadelphia in 1959, and was on the faculty of the University of Pennsylvania ever since. Lester Luborsky is one of the founding fathers of psychotherapy research for 50 years: he developed methods to investigate what makes psychotherapy work and is recognized for his development of 36 operational measures of clinical concepts including the HSRS, and the groundbreaking central relationship pattern measure – the Core Conflictual Relationship Theme.

7. See note 4, p. 220.

8. Otto Pötzl (1877-1962) was one of the most respected representatives of the Viennese school of psychiatry. One of his lectures at Vienna University, in which he presented experimental research into dreams and thus confirmed the Freudian theory, led Freud to invite him to attend the sessions of the Vienna Psychoanalytic Society. In June 1917 he delivered a paper on the theme of "*Experimentally provoked dream images as an illustration of Freudian dream analysis*.". For the Pötzl procedure, see also p. 303.

rather well in New York at Mount Sinai, Charles Fisher[9], was an analyst who actually repeated the Pötzl experiment and published a series of very interesting papers, and worked with a number of psychologists, finally including me. I recommended Doctor Fisher for a visiting professorship at Menninger's because he was very interested in collaborating with me on an experiment combining the sleep-dream cycle with subliminal stimuli.

A.B.: That idea came from you then?

H.S.: Yes, I approached him about it. Fisher started out with subliminal research, which he did for a number of years, making important contributions to that field. But then he got interested in sleep-dream research and he shifted to this research almost entirely. So I suggested to him that he could join me in a combined study at the Menninger Foundation. By that time I had developed the rebus procedure (see Fig. 5, p. 376) and he was quite intrigued by that. So the opportunity to combine these two procedures was very appealing for him. We worked together and completed this experiment. It must have been about 1965 or 1966 and we published the paper in 1967 (Shevrin & Fisher, 1967).

A.B.: So you were quite some time at the Menninger Foundation.

H.S.: I first had a postdoctoral scholarship from '54 till '56 and then I almost went to Austen Riggs, which was a much smaller version of Menninger's in Massachusetts, a beautiful state.[10] One of the real geniuses in psychoanalytic theorizing, David Rapaport was at Austen Riggs.[11] He had been at the Menninger Foundation. He had left before I came to go with a group to Austen Riggs. There were a number of people who later became important figures in psychoanalytic theory, people like Roy Schafer[12], Merton Gill[13] who left with Rapaport for

9. See note 9, p. 223.

10. Austen Riggs is a small, non-profit open psychiatric hospital in Stockbridge, Massachusetts specializing in the psychotherapeutic treatment of psychiatric disorders. Established in 1919, Riggs is known for its internationally-recognized tradition of providing intensive psychodynamic psychotherapy in a voluntary, open, and non-coercive community. Noted psychoanalysts who have worked at the Center include Erik Erikson, David Rapaport, Merton Gill, Roy Schafer, and Margaret Brenman-Gibson.

11. See note 2, p. 219.

12. Roy Schafer (°1922) is an American psychologist and psychoanalyst, who has emphasized a psychoanalytic concept of narrative. For Schafer, an important purpose of the analytic process is that the analysand regains agency of her own story and of her own life.

13. Merton Gill (1914-1994) is an American academic psychoanalyst who achieved prominence as a theoretician who discussed fundamental matters like the nature of psychoanalysis as a therapy and as a subject for systematic observation and research. In his

Austen Riggs. Rapaport and his group created a very lively atmosphere in research at Austen Riggs. Rapaport was an intriguing, charming and difficult man. He had an open position which he filled every year with somebody who would work closely with him. Rapaport wasn't a religious Jew, but he was very Jewish in one respect: in the old country young men, early adolescents went off to the yeshiva without a penny in their pocket. The families in the town in which the yeshiva was located invited these scholars to have dinner with them. The families would take turns with these kids, they were not more than thirteen to fifteen, right after their Bar Mitzvah.[14] And that was their only meal of the day. If they weren't invited, they didn't eat. I remember my father-in-law would tell us – he was a rabbi – that if he missed a meal, he would go into the garbage cans. That was supposed to be good for you. It builds character and you'd find out what life was really like. That was Rapaport's attitude, so he offered me a salary on which I could not support my wife and kid. I had to turn it down. But by that time the Menninger Foundation itself had come through with an offer which was much better than Rapaport had offered. Also, I worked out an arrangement which was new at the time: I would spend half time doing research and half time doing clinical work. I already had developed a laboratory. Luborsky and I published, I think, 3 or 4 papers together (see bibliography). Then Lester got very interested in psychotherapy research and was not interested in going further with the subliminal research. I stayed with it and I don't regret it. I would love to have worked with Rapaport but not under those conditions.

A.B.: You also had received clinical training by then?

H.S.: That's another interesting thing from the point of view of my involvement in psychoanalysis. I did start my clinical work almost immediately when I was a postdoc and it was excellent training, I started out mastering psychological tests and shortly after that, I began to see patients in psychotherapy under very good supervision. I was quite content with that. There were people around me who were doing

later years he investigated how the analyst was perceived by the patient and published *Psychoanalysis in Transition: A Personal View* (1994).

14. Bar Mitzvah is one of the terms to describe the coming of age of a Jewish boy or girl. According to Jewish law, when Jewish children reach the age of majority (generally thirteen years for boys and twelve for girls) they become responsible for their actions, and "become a Bar or Bat Mitzvah." The service is often followed by a celebratory meal with family, friends, and members of the community.

analytic training. It never interested me at that time; I was satisfied with what I was doing.

A.B.: So you weren't in analysis?

H.S.: No, not at that point. Then, inevitably, one of my supervisors asked: "Are you interested in analytic training?" I was, yes, but I had a wife and a family – I think by then we had two kids – and I said: "How am I going to pay for it?" The other reason why I was reluctant is that at that time the American Psychoanalytic Association was not very friendly to non-physicians: the people who were considered to be suitable for analytic training had to be MD's [medical doctors]. They were working out a program, called *waivers*. They gave you a *special waiver* if you were interested in research: if they felt you were talented and could make a contribution, then they would waive the necessity for being a physician. That didn't appeal to me: I had to fit a special category and I had to convince them I was a researcher. Also I had to sign a statement saying that once I received my training that I would not seek to go into private practice. They didn't want the physicians to have competition, of course. I had no intention of going into private practice, but nevertheless, it didn't sit well with me. Then one of my supervisors said "You ought to consider this." I decided that if they were able to help me to finance it, I would apply. So I went into analytic training. I had a very good analyst and I got a great deal out of my analysis. I started seeing my patients with supervision. I remember my first supervisor who was a crusty old Dutchman, Van der Walls. He was a big, tall man in his sixties. When I came in for my first supervision session he said: "You are doing research!" And I said "Yes Doctor Van der Walls, I'm doing research." He says: "How old are you?" I told him I was about 33. He then told me: "An analyst shouldn't do research till he's 45." Totally misunderstanding what his position was, what animus laid behind it, I very naively said: "But Doctor Van der Walls if I would wait until 45, I would never be able to build a career as a researcher." He didn't like that and we did not have a good time in supervision. Anyway, I completed my training. In those days it was a long time because they had this silly system, where first you had to be in analysis at least a year. Then you were recommended to start your courses which were 4 years, so you were in 5 years. You could not start your control case until you had finished your courses, which meant you didn't start your control case till you were 5 years into your analytic training. Then you had to have 3 cases and one of them had to terminate. I graduated in 1969; this was while I was still at the Menninger Foundation.

A.B.: How did you come to Michigan eventually?

H.S.: When I was beginning to feel that the Menninger Foundation had a number of institutional problems, mainly having to do that it was run by a family. The old people died, and the sons I didn't think were of the same quality as the older generation. I didn't think the place had a real future – at least not for me. I started looking around. By that time I had a sufficient number of publications so that I could qualify for an academic position. At first I was offered a position as chief psychologist at the Albert Einstein University in New York. I almost took it. The chair of the Department of Psychiatry, whom I really liked, Morton Reiser very much wanted me to come: "A research building is going up, you will have lab space, you'll have this and that."[15] But then I heard that he was leaving Albert Einstein for Yale. The person who would take over as chair, I didn't like at all and he was not really interested in me. So I said no. To this day Reiser[16] thinks I made a mistake. But I said, "No, I didn't make a mistake. I wanted to work with you, but then you go to Yale. You should have offered me a position at Yale." That happened around 1966 before I finished my analytic training, which was another reason why I wasn't eager to go. But the main reason was that I didn't think that it would work out for me and that turned out to be the case. The person who did take the job didn't last long and he was a good person. Then I was looking around for a number of years. The person who was actually one of my supervisors for many years at Menninger's, Marty Mayman, was now at Michigan. He had a professorship in psychology. He told me there was a position opening up in the Department of Psychiatry as a professor of psychology and as chief psychologist. That was 1972, and I applied for the position. The then head of the department was analytically trained, but not necessarily analytically friendly. There were a number of analysts in the department at that time, but I didn't anticipate that in a matter of two or three years, it would be completely changed. The analysts in the department would be replaced by biological psychiatrists, something that had happened all over America in the seventies.

15. Morton Reiser (1919-2007) is an American psychiatrist who built the Department of Psychiatry at Yale, led the American Psychoanalytic Association, and pioneered in the rapprochement of psychoanalysis and neuroscience. He was a pioneer in the field of neuro-psychoanalysis, a seminal thinker in the study of the mind-body relationship, and an educator who brought attention to psychological and social factors as well as biology in training healthcare providers, psychiatry, psychosomatics, and psychoanalysis.

16. Who is still alive when Dr Shevrin speaks this words.

A.B.: How did you establish your own lab at the University of Michigan?

H.S.: I came to Michigan in 1973 as chief psychologist. I had already done quite a lot of research; I had a number of publications. The first couple of years I wanted to reconsider what I was going to do. Before the end of the seventies, I was able once again to have a laboratory and I started doing my research. That's when I found Bob Berry. It was maybe two, three years after I had arrived. I got a call from this person who said he was interested in my research and he wanted to speak to me. When he came to see me, the first thing he said was, "What can I do to help you?" The question took me by surprise. Then I said to him "I need a laboratory." And he said: "I'll get you the money, ok." His grandfather, who had been vice-president for engineering at General Motors, had already given a lot of money to the university. That's how it started: within a couple of months Berry had raised $35,000. This is about '77-'78. With that money plus money the department contributed I bought the lab equipment.

A.B.: Which was a tachistoscope?[17]

H.S.: No, the tachistoscope (see Fig. 6, p. 377), I got before that from the Office of the Vice President of Research. Bob Berry raised about $35,000, but I needed $75,000, because I needed an EEG machine[18], a computer, and a number of other things. So I went back to my chair and said "This guy has raised $35,000, and I need 75, can you kick in the rest?" He was so surprised that he said "Ok." Berry's money mainly came from his grandparents on his mother's side, Ormond and Hazel Hunt. Berry's father had been the head of surgery at the Michigan Medical School and his mother was a pianist. It was her side of the family that had money. That's why the lab is called *the Ormond and Hazel Hunt Laboratory*.

A.B.: You said you were reconsidering your research. What did you want to do at that point?

H.S.: By then I had published about thirty papers, mainly in psychological journals, one or two in psychoanalytical journals. I had got grants from the government when it had not been difficult for psychoanalytic researchers to get grants from Washington. I wanted to go in a different direction and needed to get some people together. I

17. A tachistoscope is a mechanical device used in the laboratory to present visual stimuli for a very brief time period (e.g. one or several milliseconds), see Fig. 6, p. 377.
18. Electro-encephalogram or EEG, see also p. 272.

had an engineering and computer science collaborator, Bill Williams[19], head of the neurology EEG lab, Ken Koi and his engineer, Bob Marshall with whom I collaborated on several research projects not directly related to what I really wanted to do, but it gave me access to an EEG lab that I did not yet have. Bob Marshall was very important for the beginning of my research. He was a very unusual man. But then a new neurology chair came in and he wanted nothing to do with EEGs, especially not the way Koi was doing it. So the laboratory was closed. At that point I didn't have the means to do it, but I had some good engineers who really knew their stuff. With Marshall and Williams and Williams' students and myself and the money from Berry and the department we established the laboratory. That's when we really got started.

Work

A.B.: What was your research direction?

H.S.: During the period of time before establishing the lab, I had given considerable thought to what I needed to do next. I resolved that it would need to be a clinical study with the important addition of objective EEG measures. I got together a group of analysts who were interested in research to do what finally became "Clinical Study 1" (Shevrin, Williams, Marshall, Hertel, Bond, & Brakel, 1992). There were a number of psychologists who were interested in research but they wanted analytic training and analytic training is expensive. They couldn't work out a deal with my department and so they left their appointments at university and went into private practice to make money. No one of them could actually develop an academic career. This was ultimately very destructive to the field because we couldn't get enough bright capable people who could do research. I kept talking about that in our analytic institute, but they could care less. At that time psychoanalysis was still doing reasonably well although it was at a turning point. Then my department [of psychiatry] was shifting away from psychoanalysis to a biological approach. I was very shortly to become about the only analyst left. Thank goodness,

19. William Williams, is professor in Electrical Engineering and Computer Science and Biomedical Engineering. He joined the Faculty of the Department of Electrical Engineering and Computer Science at the University of Michigan and was appointed Professor in 1974. His interests include the theory and application of signal processing and communication techniques, especially, but not exclusively, to biological problems. He has a particular interest in time-frequency distributions and their applications in nonstationary signal analysis.

Linda [A.W. Brakel] showed up and a few others and I began to put my plan into action.[20] This research was what I was really interested in doing and everybody would tell me "You can't do it, it won't work"…

A.B.: … but it finally worked because you did do "Clinical study 1". Can you briefly explain what it is that you have been studying?

H.S.: My interest was in developing a methodology which would make it possible and valid to study what our significant psychoanalytic processes are. It started out with the unconscious. I had already done some work like the rebus study to deal with primary process aspects. *I always had those two goals: to establish the existence of the unconscious and to discover how the primary process works unconsciously*. I was trained in ego-psychology theory with an emphasis on unconscious conflicts. I undertook, with a group of analysts, to take people who are suffering from some significant symptom for which we would assume that there is an unconscious conflict. We would do an evaluation in a psychoanalytical way, aimed at understanding the unconscious conflict and select words that were related to that conflict, as well as words that indicated how the patients consciously experienced their symptom. Then we would go into the laboratory and present these words subliminally and supraliminally and collect brainwaves. That gave us an objective convergent measurement: no one could say that the brainwaves were influenced by the clinicians' judgments. The clinicians were nowhere around when this was going on! I had the benefit then of Bill Williams and his students who were developing this very unique way of analyzing brain waves with time-frequency distributions[21] that Selin [Avyente][22] is continuing now at a much more sophisticated level. The then post-doc student Ed Bernat[23] worked out a joint program with Bill and so lo and behold some things worked out that made the research possible. The group was very enthusiastic about the prospect of doing the study, although we all knew it would be difficult and time consuming.

A.B.: What were the main results of this study?

20. Linda A.W. Brakel, co-director of the Ormond and Hazel Hunt Laboratory, see p. 222.
21. An ERP is a time series of brain responses to stimuli measured on the scalp; "Fast Fourier Spectrum" transformation converts a time series into a frequency domain data that depicts energy or power at each frequency. A time-frequency distribution provides a view of power for each time and frequency bin.
22. Selin Aviyente, see p. 223.
23. Edward Bernat, see p. 224.

H.S.: The main results were that when *the words the clinicians had agreed were related to the unconscious conflict were presented subliminally* – i.e. at one millisecond – *the brain waves of those words*[24] *could be classified as going together on the basis of common time-frequency features*. That was not the case for the waves related to the conscious symptoms words. On the other hand, when we presented the same words supraliminally (at 30ms), the brainwaves no longer put the unconscious conflict words together but they did a better job of putting the conscious symptom words together. Now the other thing was that we had the same little measure as we are using now, the HOQ that is related to repression.[25] When we correlated the subject scores on HOQ repressiveness against the experimental effect – defined as greater classification for the unconscious conflict words subliminally than supraliminally – that correlated quite significantly, at .77. The more repressive you were, the better your brain classified the unconscious conflict words subliminally than supraliminally.

A.B.: So this means that these unconscious conflict words had a special status…

H.S.: … but only when they were presented unconsciously, then they go together. When they were presented supraliminally, so that they can be seen consciously, the brain waves treat them as if they are unrelated. Moreover, we would write each word or little phrase [used in the study] on a piece of paper and mix them up. At the end of the experiment we would say "Here are 32 words, these are the words you saw. Could you put them in as many categories as you feel they belong and give us names for the categories." When it came to the conscious symptom words, they were placed in maybe two categories – e.g. six in one, two in the other – they were really seen as going together consciously. The unconscious conflict words were placed in five or more categories. Consciously they were not seen as going together. So, we had the brain wave evidence, we had the evidence from the HOQ and we had the evidence of how the participants categorized the words. We interpreted the results to mean that *when the unconscious conflict words were presented consciously, there was a repressive inhibitory process at work which kept the brain and their*

24. Remember, these words were *inferred* by the analysts from the conscious story brought by the participants, they were at no point indicated as such by these participants, let alone indicated by them as pertaining to the conflict underlying their symptoms. [Note by *A.B.*]

25. The Hysteroid Obsessoid Questionnaire (Caine & Hope, 1967) is a clinically designed scale that measures hysteroid (avoidance and repression) versus obsessoid (intellectualisation) neurotic defense.

own conscious awareness from seeing any relationship among these words. But the analysts had seen the relationship among the words, because in their judgment they were significant aspects of the unconscious conflict. So, unconsciously they were put together, consciously they weren't. And if you were more repressive, you did it even more.

A.B.: So, consciously the brain has the time to put some inhibition process in effect?

H.S.: But bear in mind that the person was unaware that his or her brain was inhibiting anything. Now *we also found out that inhibitory processes were going on in the absence of any consciousness at all.* This is some of the things that Michael [Snodgrass][26] found out in a phobia study.[27] So, we published a preliminary report on the "Clinical Study 1" in 1992 in *Consciousness and Cognition* (Shevrin et al., 1992) and I got this letter from Grünbaum.[28] He had published a very substantial criticism of Freud (Grünbaum, 1993) and felt Freud's theory was circular. Our method could not be criticized in that way, because things that were going on were independent of each other: the clinicians' selection of the words, the presence of an unconscious conflict, and the brain waves were totally independent. So Grünbaum wrote me a letter that said that yes, I had found evidence for an unconscious conflict, but the design did not make it possible for us to say that that unconscious conflict was the cause of the symptom. And he was right, so that's why we went on to do "Clinical Study 2". That's what makes "Clinical Study 2" so important… Now in the mean time a lot of other research studies were going on, I had a series of post-docs, some good, some bad. Mike [Michael Snodgrass] came along,

26. Michael Snodgrass, co-director of the Ormond and Hazel Hunt Laboratory, see p. 220.

27. See also Howard Shevrin Paris' paper p. 311-341: "Abstract of the phobia study". "These findings are based on a study of spider (n = 10) and snake phobics (n = 7) to whom spider drawings and control rectangles were flashed subliminally at the objective detection threshold (d' = 0). Event-related alpha synchronization (α ERS) was obtained to these subliminal exposures. We found that for spider phobics in contrast to snake phobics the extent of the a-ERS effect in favor of the phobic stimulus as compared to the control stimulus was *negatively* correlated with N100 amplitude and spider detection, but *positively* correlated with spider fear. This is, the more the phobic stimulus was able to elicit an inhibitory a-ERS response as compared to the control stimulus, the smaller the N100 attentional response to this stimulus and the later it came; moreover, the spider detection was worse and the spider fear higher. Both N100 amplitude and latency predicted changes in spider fear ratings from before and after stimulus presentations. Lower amplitude and delayed latencies predicted no change or greater spider fear."

28. Adolf Grünbaum (°1923, in Cologne, Germany) is a philosopher of science and a critic of psychoanalysis and Karl Popper. See G. Van de Vijver (ed.), *Psychoanalytische Perspectieven*, vol. 16, 1998, nos. 32-33.

he stayed, Ed [Bernat][29] stayed for a while, Phil [Philip Wong][30] stayed for a while. And then we became the group that you have been participating in.

A.B.: "Clinical Study 1" has also been published as a book?

H.S.: Yes, the book is called *Conscious and unconscious processes: psychodynamic, cognitive, and neurophysiologic convergences* (Shevrin, Bond, Brakel, Hertel, & Williams, 1996). It addresses three very different constituencies: psychoanalysts, neurophysiologists or psychophysiologists, and cognitive psychologists. Our task was to make sure that people from these different backgrounds would be able to appreciate what we were trying to do. For example, the opening section on theory has three chapters, one is called "Psychoanalytical Theory", another is called "Cognitive Theory" and a third "Psychophysiological Theory." The next section is on methods and again the first chapter is the psychodynamic clinical method, the next is the subliminal cognitive method and the third is the psychophysiological method. This principle is followed throughout the book. The last thing I would mention is that a considerable part of the book is made up of three very detailed case presentations and that in these case presentations, the protocols we work with are very liberally included, so you as an analyst or clinician can follow what we did and what we made of what we were looking at. And you can arrive at your own judgment whether these data we were using, based on the interviews, make sense to you, whether it clicks with you own clinical understanding. So the book isn't simply the account of an experiment, it is using the experiment as a means to draw together these three different frames of reference and to show how they can converge in a way that strengthens each of the methods by being included with the other two – to show that you couldn't achieve by anyone of these methods what you could achieve by combining them. That is essentially the gist of the book.

In gesprek met Howard Shevrin I

Samenvatting: Howard Shevrin is in 1926 in New York geboren. Tijdens de Tweede Wereldoorlog vecht hij aan het front in Duitsland van december 1944 tot februari 1945. Na de oorlog behaalt hij de titel van doctor in "de psychologie en de ontwikkeling van het

29. See p. 224.
30. See p. 224.

kind" aan de *Cornell University* in New York. In 1954 vervoegt hij de Menninger Stichting in Texas, waar hij subliminaal onderzoek opstart met Lester Luborsky en Charles Fisher. Hij vervolmaakt er ook een psychoanalytische vorming. In 1973, wordt hij aan de universiteit van Michigan benoemd als professor in de psychologie en als hoofdpsycholoog van het psychiatrie-departement. Met de hulp van Bob Berry, sticht hij de *Ormond and Hazel Hunt Laboratory* en start er onderzoek op samen met Bill Williams, het hoofd van het EEG lab neurologie. In de daaropvolgende jaren, vervoegen een aantal post-docs hem, waaronder Linda Brakel, Edward Bernat, Phil Wong en Michael Snodgrass. In één van hun grote studies, tonen Shevrin en collega's aan dat psychoanalytisch gevormde clinici in staat zijn de rationale van het onbewuste conflict af te leiden uit het verhaal van subjecten, die zelf niet in staat zijn dat rationale bewust te herkennen, terwijl het statuut ervan wel bevestigd wordt door de EEG hersenparameters.

Sleutelwoorden: Shevrin, Psychoanalytisch Onderzoek, Subliminaal, Onbewuste, Geschiedenis.

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