MANDEL COHEN

You said when you went out to give a talk in St Louis, they said to you "Hey you really should have the credit for DSM III" – but your reaction was that you felt it might be better to look for who was to blame for DSM III. When did you go out to St Louis to give the talk?

It would have been at the time that Eli Robins was awarded an honorary degree – more than 15 years ago. I've given several talks out there. As a matter of fact, although I never worked there, they look on me as a founder of their psychiatry department because Eli Robins worked with me when he was a resident in neurology and psychiatry. He was a very smart boy I can tell you. He went out there eventually. They gave him a lowly job. The department was half-psychoanalytic at the time but he cleared the psychoanalysts out completely and then trained some younger people who took over departments here and there. At one time in the early 1960s, there were according to William Sargant, only two people with professorships in the whole country who were not psychoanalysts. Eli Robins and me. Most departments wouldn't have anybody who wasn't a psychoanalyst. You can't prove this but it's true.

At that particular gathering, I outlined what we had done and the place for what were later called operational criteria in our work. We did research on hysteria and manic depressive disease but especially on anxiety neurosis for the Armed Forces – this is a condition that's also called neuro-circulatory asthenia, Da Costa's syndrome, soldiers heart, effort syndrome, irritable heart. We thought it was all one condition. What we did in this and in our manic-depressive and hysteria papers was to define precisely what symptoms you had to have the condition – for example, it might be 6 out of 10 features to be included. All we were saying was if you want to repeat our work this is what you've got to do.

Like us, DSM said that if you've got 5 of the following for instance you've got this disease. But they've also introduced diseases like attention deficit disorder, which I don't believe exists, although good people think so and borderline personality disorder which I don't think exists. Schizoaffective disorder, I'm not sure about. I'm not even sure that the division of depression into unipolar and bipolar is sound. In our paper, we called it manic depressive disease. It was called manic depressive psychosis before that but we recommended the word psychosis be dropped. There was a question on the Board exams - what's the difference between psychosis and neurosis and the standard answer was that in a psychosis you had delusions and hallucinations or you were crazy or that you did not know the nature of reality. As we said in the paper we thought asking a resident in medicine to know the nature of reality is a little difficult.

There's another disorder DSM III introduced that's a billion dollar item - post-traumatic disorder, Vietnam Syndrome. I gave a talk at the APA in Canada in which I pointed out that I'd examined these patients and it seemed to me they had anxiety neurosis or depression and Battle Dreams. But battle dreams are normal for a soldier. One of the best examples of this was Harry Hotspur in Shakespeare, a fierce warrior who was having battle dreams in bed, who jumped out of bed with them. His wife said cut it out - you're in bed with me, you're not supposed to be doing this. And he said it doesn't mean a thing. Sailors dream about sailing, wine pressers dream about making wine and soldiers dream about fighting. That's all there is to it. But battle dreams have been there from the beginning of time. The cases we were seeing, I think were anxiety neurosis but I feel it was doctors and social workers that put the battle dream concept into the head of these guys.

How did you come across the idea of operational criteria? You said it was because you were trying to study anxiety.
Yes, I got involved in studying the anxiety disorders for the Army – the Office of Services Research (OSRD). There were about 10 or 15 different names for the anxiety disorders associated with Wartime conditions going back to the Civil War this country. A man named Da Costa wrote a beautiful paper about the condition and what he considered the cause and symptoms were.

You did this work with Paul Dudley White, the eminent cardiologist. How did you get mixed up with him?
I used to be a cardiologist and as a young person I got some recognition in this area. My early work was on the mechanism of heart failure in pregnant women. We did a series of studies on rheumatic heart disease, which by the way has almost disappeared. That's an interesting topic that nobody has done anything about - what causes a disease to disappear. If we knew it we might be very important. Rheumatic fever has disappear and with it Syndehams Chorea. There's a neurological disease in the Pacific that has disappeared - not a big one but a small one, like multiple sclerosis. We assume it's a viral disease but we never found the virus.

I met White when I came over to Mass General, to help start a psychiatric service and to do some research with Stanley Cobb. Cobb was the Professor of neuropathology and he worked in the neurological unit in City Hospital. He was asked by the Rockefeller Foundation in 1935 to start a Psychiatric Department in Mass General. They wanted it to be "scientific". So I went over there with him to work in psychiatry. Along with the circulation work, I was doing two bits of research - one was on drugs for epilepsy and the other one on anxiety attacks. Dyspnea was one of things that linked these together. Well what do you do in cases of dyspnea if you catch them in the middle of an attack? At the time one of the standard things was to look at the speed of circulation, which was something I had learnt to do on the pregnant women.

You know something, despite the number of people who have written about anxiety states and anxiety attacks, most of them have never seen an attack. I've probably studied 1000 cases and I've seen one attack only. Later on we tried to cause attacks. We had one of our girls there surreptitiously to witness what was happening but as one of the patients explained – I wouldn't have an attack in here, she said, I feel safe in here in the hospital.

I gave a talk for Rodrigo Munoz at his society on anxiety attacks and when they occurred and our results. I pointed out that most people don't see one. I asked them to raise their hands, whoever had seen one. Most of them said they'd seen none or one. Rod said he'd seen thirty. He said now I don't want you to misunderstand - I had a trial to make a little money on an emergency ward and that's where he saw them. He didn't see them on a psychiatric unit.

As for reproducing anxiety attacks, we were able in the 1940s to have the patients rebreathe CO-2 and about 80 % of them got attacks, which they said were much like or just like the natural attacks. We didn't have any other patients or controls, who had an attack. So we decided it was the CO-2, which would bring them on. We have shown that a number of things became abnormal during the anxiety attack such as blood lactate levels, even though nothing appeared to be abnormal at baseline. Later Pete Pitts showed that a lactate infusion could also trigger an anxiety attack. He was a very energetic young doctor, who lectured often in St Louis. Based on our finding that blood lactate was high in anxiety neurosis, he said ah ha and gave 8 or 10 persons a solution of lactate. I think that six of them or something like that had the attacks but controls didn't. Since then there has been a whole field of lactogenic anxiety attacks.

We did other things - like scaring the patients. We showed them a violent war picture, talking to them about unhappy things but the only way we could produce an attack was with CO-2. But the way we did the CO-2 was important. We did basal metabolism on all patients. We also had an apparatus with soda and lime, which takes all the CO-2 out so they breathe oxygen. Then in some cases we could surreptitiously disconnect the soda and lime so that they began to breathe CO-2. We got on to this after the apparatus was accidentally disconnected once. Now over the years, going back to World War 1, several people noted that these patients were particularly sensitive to CO-2. For example, these patients couldn't swim, primarily because they couldn't breathe because they got scared. It was also noted that they couldn't wear a gas mask. And that was true in World War 2 also. So that led us into rebreathing and a lot of other discoveries.

Now as you know, these patients cannot go into crowded places. If they go to Church, they'll sit in the back seat so that they can leave if they had to. We tested carbon dioxide in the elevators and in basement of Filene's Department Store, which was where the ladies said they couldn't go shopping. This was a bargain basement that was very crowded. But the CO-2 levels were always normal even when it was crowded. So anyhow, when we started doing research, we just continued to use the same standards as we had in the rest of medicine.

One other thing was this. Doing the anxiety neurosis study for the army, we did all kinds of tests on the patients. But we also tested the tests. We gave them every psychological test known to man. We said in our paper that the Rorschach test and the Thematic Apperception Test were not useful in our hands. Now they were given routinely and cost $100 and the TAT had been developed by Harry Murray. He was the professor of psychology at Harvard, a good friend Dr Cobb's and he ran this clinic for patients at Harvard. Dr Cobb as a result wanted me to cut this piece out of our paper. Although I listened to him, I didn't cut it out. He didn't do anything. Fortunately I could keep my labs.

Another thing we did later was to devise a method to work out how moist your hands were. We did this because it was said that in neurocirculatory asthenia, Soldiers Heart, these cases had moist palms. But we found that at the draft board, everybody had moist palms. There were methods, where you put some starch on the palm and it turned blue when they got moist and you could investigate the amount of blueness. This was difficult to assess which led us on to another method. Something I learned out of this was that you can get more help from general industry than you do from scientists or scientific drug houses. What we did was we asked ourselves - how do you dry something? First we thought towels and then we thought blotters, so we went to a blotter manufacturer. Then we cut up blotter and put in on the palms of these soldiers. We would weigh it beforehand and then again. We might have a 10 or 12 milligrams change. The test was sensitive. We found in one patient who had had a sympathectomy that one hand had a reading of 1 and the other a reading of nearly 20. In a proper case of hyperhydrosis, you might have a value of 50 milligrams but in our soldiers who acted as controls and soldiers with NCA, the results were exactly the same. We took people who worked in the hospital then and they had a lower rate, so we came to the conclusion that suspense was probably the cause of it. These soldiers who came in to have the test were clearly not just normal controls.

People who didn't like our research would say it was bad because we were using the medical model, whatever that means. And it is true that in our depression paper we didn't note all of the psychological symptoms but we did note poor concentration and suicidal thoughts and none of the controls had these things. As far as the other symptoms, such as pains in the chest, these were simply medical symptoms in a range of different patients. In addition to using healthy controls, we used a bunch of sick people from around the wards for our controls. In the first study I'd done on anxiety neurosis, we started to use as controls social work psychiatry students and certain lady psychology students. It turned out that a few of them had the same breathing patterns as the patients. When we worked a number of these people up, we found without fudging it that they had the disease. So we then used working women rather than psychology students as controls. I wouldn't say this is a scientific observation but I think that there is a higher proportion of psychological disease in doctors going into psychiatry, as well as in psychologists and social workers than among other groups. Maybe something has been done on this but I think it's very obvious.

But we have digressed a long way. I was going to tell you about Dr White. He was one of the nicest people I have ever met in my life. I have said on occasions that if there is such a thing as a Christian, I think he was one. For instance, if you wrote a paper he insisted that the other people put their name first. Sometimes he thought he hadn't done enough to have his name on it.

During the War, doctors went to the Army hospitals and gave clinics on this and that. And my team was Dr White and me. And of course they all wanted to talk to Dr White because he was world famous. They would ask him questions and he would frequently say you know I haven't done much work on that lately but Dr Cohen has and he'll be better able to answer your questions. Now the truth of the matter was he knew more about it than I did but it was just his way of helping me and other young people.

I had known him before because I was known as physiologist ever since I had done the work on the circulatory system in pregnancy. When I came to the General, although my job was a psychiatric resident with a $200 a year salary, I was doing cardiovascular studies on the anxiety disorders. They didn't do these things at the General. Dr White's department was very very clinical. Over at the City Hospital, or at the Semmelweiss, they did all these physiological studies and they didn't regard Dr White as a scientific doctor because he didn't do these tests. Anyhow, as it turned out in the General, when the medical service needed venous pressure measuring, they put the requisition in to psychiatry and I would do the venous pressure.

Then when the Army decided they wanted to study Soldiers Heart, they asked Dr White to take it over. And he decided that he wanted a director who was both a psychiatrist and a cardiologist. I guess that was me. So they gave me this job as director of this OSRD team under Dr White. Actually it was no great honour because I was probably the only person who was both. But it gave us the opportunity to do this study from which we published 15 papers or so describing what these patients were like. We did a 20 year follow up and so on.

He was just very kind. He had some very important people come to see him who would stay in a local hotel. So he'd asked one of his younger people, such as me, to go round and see him, take an electrocardiogram and bring it back to the hospital. Of course, he took care of President Eisenhower. There is one funny story about this. Mr Eisenhower was not a very co-operative patient, so finally Dr White told him that if he didn't co-operate, he was going to drop out of the case and he could get another doctor. Anyway, it turned out Mr Eisenhower co-operated from there on, so Dr White went down to a book store and found a packet of golden stars – the kind you used to give children for good work. When he saw the president, he said that he had heard that he had been very co operative with everything so he was now going to make him up to the first ever 6 star general. And he took one of these little stars and placed it on his robe. I heard it again from Mr Eisenhower at a meeting in New York, a thousand dollars a plate, for the International Cardiac Association – he gave Dr White a golden stethoscope in return.

But you know his rank in Harvard was up to the end an instructor. They finally made him a professor. The reason for this was a team of Americans were going over to celebrate of some important medical event in Europe. Now he was the Chairman of the group. The rest of them were Deans, College Presidents etc but he was only an instructor. So when they got off the boat they made him a professor. He was very much amused by this.

Why would Harvard have been so slow to recognise him?
Well I think that to the powers that be, he was not a medical scientist. He was a good clinician. Actually, he had discovered two or three illnesses and how to treat pericarditis. Most of his discoveries were about diseases and symptoms and cures. Anyway, he felt, and I think he is right, that doing cardiac output really didn't tell you for instance when the patient was in heart failure. All of these people, who believed that it did, felt that he was against science.

Were the two of you then rather similar in some respects - not part of the establishment?
Well no, he was considered a great doctor, one of the greatest at the Mass General. They had several great people there. George Minot, who won the Nobel Prize for pernicious anemia, and John Crandon, who picked up the prize for Vitamin C - you know he was not much of a medical scientist either. Just did things in a simple manner. He didn't know about DNA or genomes but he still got the Nobel Prize for a simple idea.

They had a Paul Dudley White society, which right up till he died had a meeting once a year. At that meeting and at other times he would have everyone over to his house for Sunday morning breakfast. At one meeting, in amongst all his young admirers, he practically caused a calamity by having a coronary attack. They took him to the coronary care unit, in Mass General, the so-called centre of modern medicine. He later said he thought that coronary care units were unnecessary and unpleasant and that only a few people with cardiac problems should be in intensive care. To put everybody routinely into CCU he thought was wrong. This was a shock to the young doctors, indeed to everybody. CCUs were supposed to be one of the great advances. He usually found something to talk about.

So when it came to operational criteria, where did the idea come from?
No idea. I just thought that we were doing studies and we should try to be scientific. If you have a disease that had 8 different names, particularly when you wanted to do a family study – we showed that it ran in families – you would need criteria. When we were studying populations of anxiety neurosis in the Army, we found that those, whose problems came on in the Army, had had it in the family. Two or three groups later came out with familial data on anxiety neurosis and I always looked at what criteria they have used. Now in Washington University, they used our criteria. As a matter of fact in one of our first studies, Eli Robins did most of the work, so I put his name first on the paper.

This was a study on hysteria in men. There was an argument at the time that men didn't have hysteria. But nobody had studies it. We didn't have any male hysterics at Pratt Diagnostic Hospital, where I was at the time. We had 80 women with hysteria. In hospitals, which had a ratio of 50/50 men and women, we had no cases of hysteria in men. I suddenly woke up to this one day. So Eli went around to Army hospitals and Veteran's hospitals, where they said they had cases of hysteria. We had someone examine them the same way we examined the women with hysteria and we found that there were indeed cases. He found 50 cases.

But hysteria in men was different than it was in women. It was more like compensation neurosis. The women with hysteria didn't have any chief complaint. They just rambled on. The men usually talked about one thing – maybe backache. Another difference was that the women were friendly and talkative, the men were more disagreeable. They didn't like doctors. A major difference between them was that all the men had the immediate prospect of collecting something by virtue of their illness. And this was not a subtle psychological thing, it was collecting money, getting out of jail or getting out of the army.

In the case of the women, Sam Guze out in St Louis told us that there were only two other people who had the picture we had found of multiple symptoms – one of these was Briquet. We went back and read Briquet. He was interesting. Most other people then and until recently had just studied one or a few cases and on the basis of a dream or something had built a theory about hysteria. But Briquet in the 1850s or thereabouts had collected over 70 cases. This is what led Sam to suggest calling the disorder, Briquet's syndrome. The term hysteria was a problem. It was almost an accusation. We didn't use the term, even with the doctors we consulted with. We just said that people were nervous. Briquet also thought these patients had some kind of affective disorder.

You mentioned Briquet, who was the other person to find multiple symptoms?
Savill. His name is in the reference list in our paper on multiple symptoms. All these symptoms led Sam to suggest that in order to make the diagnosis of hysteria you had to have 25 symptoms. I found this hard to believe. But Sam found the patients. The difference between our work and that of others was in the large samples we looked at. The others, even Freud, built their theories on a handful of patients, maybe 9 or 10 persons at the most, who may have said a lot of unusual things to their analyst.

Talking about analysis, you were analysed by Hanns Sachs. Tell me about that?
Well I'll tell you, I didn't particularly agree with it but Dr Cobb thought that to be a psychiatrist you should be psychoanalysed. Hanns Sachs and Helene Deutsch were the two leaders in this area at the time. So I went to Hanns Sachs for about 2 years and he said I was through and I should start taking courses at the Institute and take some training cases. I went to the Institute for a while but I'd pull my hat over my face so that no-one could recognise me. Then I just decided that I had had enough. But at that point all young psychiatrists had to have psychoanalysis. The Rockefeller Foundation paid for it. In those days, a training analysis was about $35 an hour. I think Zilboorg got $100 an hour for treating George Gerschwin's brain tumour with psychoanalysis. He told the Gerschwin family that "he just puts on some of these fits". The family were told if he fell off the piano stool not to pick him up. He was then diagnosed and operated on later for a temporal lobe tumor.

Instead of going on with analysis, I got a Rockefeller Fellowship to go to Los Angeles to study with JM Neilson who was working on aphasias at the time. Later on this Fellowship I spent time at St Elizabeth's Hospital in Washington, which gave me my first exposure to a large hospital with a large population of chronic patients. This was a time when psychiatrists were still alienists and schizophrenia was still dementia praecox.

One of the things I was involved in early on, even before this was the early days of electroconvulsive therapy. Many years later, I was at a meeting that Max Hamilton was at. We got to be friendly. We spent three days looking for a good Indian restaurant. We went to many poor Indian restaurants. But in the course of it, I had an opportunity to talk with him and found out among other things that he didn't use his rating scale once in his own clinical practice. At this meeting in the New York Academy of Medicine, which was on electroconvulsive therapy, Hamilton said in summary that in cases of depression that didn't get well, if the doctor didn't give electrotherapy they should be charged with incompetence. A strong statement.

I've been involved in giving convulsions for a long time. I used to give fits to rabbits at the City Hospital. We had this machine which you could use to give fits. We had to find out how many milliamps it took to kindle a fit and then we'd tested out drugs such as the vital dyes and later diphenylhydantoin to see if they helped. This machine to test out drugs on rabbits had only one difference from an early ECT machine, which was that Mass General didn't have alternating current - we just had direct current. So what we did was to take the distributor out of a carburettor and link it up and that worked just as well. One day I said to Dr Cobb that with the machine we were using on rabbits we could give a convulsion that would be safer than giving chemical convulsions. We could put 2 or 3 patients in and monitor what we were doing. I wasn't really making a crusade out of this but he said he thought our aim was to stop fits not cause them. Not long after that these two Italians came out with their machine. But this was not any great passion of mine, it just seemed practicable.

We had this psychiatrist in town by the name of ?? . He was Irish Catholic. He had a clinic in Tufts and he moved over to St Elizabeths and his own private hospital. But he regularly gave ECT early on for patients with depression and he would have 30 or 40 patients who he would give booster shocks to. I don't know how long they kept on coming in but these patients used to come in, smiling, friends with each other and we used to do the booster treatments. In those days insurance covered only two weeks in a hospital stay, so he would bring them in and treat them almost every day and then discharge them and let them come back to the outpatients, which they could pay for where they had their booster treatments. I noticed that lots of these patients seemed to do well.

But there was an intense crusade, among the psychoanalysts and in the State department not to use ECT. Eventually they closed most of the State hospitals. There was a time when if I had a patient with depression and she needed to be in a hospital, I would send her to a State hospital where she could get electrotherapy. But now since these hospitals have closed there is no place to send them. The academic people got into this closing of State hospitals. It sounds like a noble cause. It sounds good moving patients from warehouses to smaller institutions but the truth of the matter is that many of those people had no place to go to.

So who do you blame for closing the State hospitals?
In Boston, Harry Solomon, who was a Professor at Harvard and Jack Ewalt, who was the Commissioner for Mental Health. But it was done in almost every State. Then they formed these clinics and mental health centers run by psychoanalysts, where there were also social workers and others who were all opposed to hospitalisation and electrotherapy. It doesn't matter who got the money, the problem was the patient couldn't get treatment. There were too many people who were opposed to this - clinical psychologists and social workers.

In Boston, you were clearly not part of the in group. Did you get much support from your wife?
She was a short story writer, very successful, published in all the good magazines. She won some prizes. She disliked doctors and hospitals. She wouldn't go to a medical meeting. She only ever heard me talk on a few occasions. She did come to Rome with the two children though. We had several trips to Europe with the excuse that it was a holiday for the family. She disliked doctors and hospitals unreasonably. One of our best friends who was her obstetrician said she was his best friend but his worst patient. The trouble was she had polio when she was a little girl and she ended up so she couldn't walk. The custom then was to do tendon transplants. They'd take a nerve that was working and transplant it to a tendon. Anyway she had three hospitalisations as a little girl. By the time I met her, she could hardly come into the Mass General Hospital.

She wrote one story about our obstetrician. He was a man of few words, Scots. He came over and said "you wrote a story about me". She said in her stories her characters were unrelated to anyone living. He came back a little later and said he kinda liked the story. When he was leaving he said how did you find out all that stuff. I don't know how she did it but she had a way of watching people. Once I know she was sitting with Ann our daughter, looking very intent and Ann said "mother, what are you doing". She said eaves-dropping. Sometime later that conversation would be in a story. I had to take her to a baseball game once when she wanted to do a story, because she had never been to one. As her editor told her, this was one she needed to be sure about – is it on first base or at first base. If she got it wrong they'd get a thousand letters from baseball fans.

Can you tell me about your falling out with Mass General?
Well as long as I kept saying the things that I was saying, and the analysts advised Dr Dr Cobb that they didn't like what I was doing, one of my options was just to change over to neurology and to medicine. Cobb by this stage had been so taken in by the analysts that he wanted to quit some of the research programs that we had started. So I changed over at first to medicine and then to neurology. I still used to come along to some his neurology clinics and offer criticisms. Later he said that he agreed with most of what I said. He was an interesting man. He had been Head of a neurology clinic and a professor in neuropathology. At that time he had an interest in psychiatry. But he was master of none of these areas. In the end, he said he was wrong about letting the psychoanalysts take over. He said that publicly.

At one time, in the neurology clinic he presented a case of a woman that folk wanted to operate on for a brain abscess. Anyway Cobb asked me to see her. Now I had learnt from Dr Neilson out in Los Angeles how to examine for this kind of complication and when I did she had nothing like a brain abscess. She was kind of stuporous. She didn't respond much and her language sounded as if she was intoxicated. I said it seemed to me that she must be taking some drug but she had been in the hospital about 3 or 4 months at this point and they weren't giving her any drugs and she seemed under a life sentence to stay. A medical student came by and said that they had a found a pill in her bed, so we searched her bed and she had full bottle of phenobarbitone. It had been brought by her children. Dr Cobb's response was that here was old Mandel coming back and setting us all straight again as he used to do. He remained more or less a friendly person but the psychoanalysts disliked me intensely.

Was that because you were so forthright in your views?

Not as far as I know. I was against psychoanalysis. I just felt they shouldn’t teach it in medical schools except in the history of psychiatry. I felt that we had no place for the analysts but on the other hand all our teachers had become psychoanalysts and all of the residents became psychoanalysts. We had psychiatric rounds once a week. It used to be that we would have a discussion and Dr Cobb would summarize but it changed so that he would turn to Dr Helene Deutsch and say Dr Deutsch what do you think?

There was one case conference that I told a story about at my 90th birthday party. This was when I was still in the psychiatry department. I was taking rounds and we presented this case. I didn't know what was wrong with this person whether they had schizophrenia or whatever. So we had a discussion. At the end, Dr Cobb said now Dr Deutsch, tell us the answer. This is a true story! She said usually we had a good discussion on something dynamic but when Dr Cohen is in charge of the case conference we talk about diagnosis. She asked “Dr Cohen, why don't you talk about psychodynamics?” She felt she couldn't discuss diagnosis. I said I would bring psychodynamics up if I knew what the word meant. I meant this honestly.

So Dr Cobb said I'll tell you what it means. He told me roughly that the patient has problems, the problems lead to symptoms and that was it. Dr Feinsinger who was a psychoanalyst said “oh no Stanley, you’re not right. The patient has certain early psychosexual problems and some problem stirs that up and then they have symptoms”. Then Helene Deutsch said you are both wrong. It is like an electric lightbulb. You are in the dark, you turn on the switch and there is light. She said “that is psychodynamics, do you understand Dr Cohen?” I said so few of my patients are electric lightbulbs that I found it difficult to accept this summary. Justin Hope, who was a good psychiatrist, fell off his seat laughing. Years later when we met, he said “Seen any electric lightbulbs lately?”

Can I take you back to Eli Robins? As I understand it, he was psychoanalysed and during this time he had the first episode of his later illness. I was told that his analyst said it was psychosomatic.
No that wasn't it. He went to analysis and I think his analyst would tell him things, which he knew could not be true but she would insist on. He decided to quit. And he was very rich, which made a difference. Anyhow, Eli had this problem when he was in the army in Texas. He was due to go to Germany and he came through Boston beforehand. He developed severe pains in his left shoulder. They put him Berkeley General Hospital. The intern out there made a diagnosis of hysteria. They asked me to see him. It was obvious he didn't have hysteria by any criteria. Also his shoulder on one side showed atrophy. So we gave him a weekend pass and took him down to the General, where Ray Adams saw him. He had been exposed to a polio epidemic in Texas and there really was no question what the diagnosis was. He was discharged from the Army.

So he had polio did he?
This was as far as I was concerned typical polio. Then years later, things went wrong again. I never agreed with the diagnosis of multiple sclerosis. He started having symptoms, where his left arm would fling up and down. They didn't have an explanation for this. And then he developed a progressive paralysis of his legs, which finally confined him to a wheelchair. Now multiple sclerosis is not usually a progressive disease in this sense. Secondly shaking of your arm up and down is not a symptom of it. But he saw some good people and they were pretty satisfied with the diagnosis of multiple sclerosis.

They named a lecture after him, while he was still alive and I went out to give the first lecture. One of the things I may have helped with was to care for Lee Robbins, who was a very smart person and a good researcher. She used to wheel him around every place. I said they should get a man to do it - she shouldn't have to. She should get on with research. I don't think Eli accepted this 100% but I gave him no choice. Later he got one of the first electric wheelchairs.

When I gave this talk he was there, and discussed it. At one of the lectures I gave there I asked the audience some questions. One of the questions was is DSM a 100% accurate? 50% accurate or 10% accurate? Most of the people decided on 30%. I asked Eli, who said 0 -30% accurate, probably closer to zero. I said, "that's my boy". This was a very friendly atmosphere.

Do you think DSM III, while helping convert US psychiatry away from analysis, has caused as many problems as its solved?
Well I think telling people that you should know how you diagnose a disease is useful. But there are many things, which I consider wrong. Some things can get out of hand attention deficit disorder. ADD, as its called now. Once you get initials then that solidifies the condition. The few cases I've seen of it were all adults and they all had depressions, without any question. My daughter works in a children's hospital and she was telling me that half the patients in hospital at that time have that diagnosis. This can't be right.

You moved from psychiatry to neurology?
Yes, I was in charge at the Pratt Diagnostic Clinic for a while after the War. I went over there with the idea that Ray Adams was in charge of Neurology and this might make some research possible. I wanted to continue with my research but they never gave me space. I had kept my labs at the General so I was there every morning and went to the Pratt in the afternoon to see the patients. It became clear that they didn't want me to do any research, they just wanted me to see a lot of patients and make a lot of money. And they kept the money. So after being there for 3 or 4 years, I came back full time to the General to neurology and medicine but not psychiatry.

In 1980 or thereabouts, anxiety neurosis was reborn with yet another name - panic disorder. Now I understand that David Sheehan organised a meeting at which the then experts on panic disorder met in Mass General - Don Klein, Isaac Marks and others and you were asked to give a talk.
Yes I gave two talks. But actually the way it came about was this. The first person to organise a meeting was a young psychoanalyst whose name was ---. He was very belligerent. When we presented a paper to the AMA on depression he went to the head of the Mass General, Dean ??, who was a good personal friend. He said that I was going to present this paper and it did represent the views of the Mass General. He thought that Dean C?? should say that I couldn't present this paper. I gave the paper anyway. But I was amazed that he could have that much nerve that he could say that since I disagreed with their views, I therefore could not present the paper. It was quite extraordinary – for the United States

You said he was involved in trying to get a meeting together?
Yes, getting the speakers. The story I heard was that Upjohn said there is a Dr named Mandel Cohen who has worked in this field. We ought to get him to give a paper, where can we find him? Anyway, they found me and asked me if I would give a paper and then if you published the paper they would give you a $1000 for it - they would decide whether you could publish it or not. Then there was a second paper on our 20-year follow-up data on anxiety neurosis. Ed Wheeler, who was a cardiologist,

a personal friend, had his name on this but he said it was so far back that he just didn't recall it all and he didn't want to give it so they asked me to give that too.

At this meeting, the new young professor of psychiatry at Mass General, Hackett, gave a paper there. He said there for the first time that his department was 20 years behind the times. He was an Irish patriot. I used to give him ties. We had a meeting one Friday, talking about some study and I gave him a tie. The next day he went riding, fell off his horse and died. He was getting closer and closer to announcing publicly that psychoanalysis had been a mistake. What made it worse was that he was seen as tolerant of all points of view.

In 1985 there was meeting in Mass General celebrating the 50th anniversary of the department of psychiatry. You weren't there as I understand it?
I was invited to give a scientific talk at some meeting in New Orleans. I just flipped a coin. I suppose sensibly I should have appeared at the MGH. I hear that a couple of people said decent things about me.

Gerald Klerman
Yes. You might get the feeling that I was fighting every day I was there but I had very little personal trouble or fighting in the end. I'd say more or less what I wanted to say. Some people said I was awkward because I asked questions but if a person can't or won't answer I never asked them a second time. I don't ask them to show them up, which is very easy to do. But once I found them unfriendly or not scientific I never asked them a second time.

At the 1999 APA meeting, at the age of 92, you were given a Lifetime Achievement Award. What led to them doing this, why did it take them so long and when did you hear about it?
Oh about 6 months before the start of the meeting. I suppose I was not considered by the APA for this kind of award because I never went to their meetings. I was not even a member anymore, although I had been at a younger age. As a matter of fact, they wrote and said they wanted to give me this and then they wrote me a second letter and said they noticed that I was not a member anymore. Would I be willing to join? So I joined but I didn't have to pay the dues. The president of the APA can chose these things and Munoz who was the President last year was a young disciple of mine. He trained at Washington University and believes that the future of psychiatry is really to do with Washington University. So he has a very simple view of me. He is also a very good politician.

How many of the people behind DSM III other than the people at Washington University did you know? Did you know Bob Spitzer for instance?
No I didn't. As a matter of fact, he read my memorial to Eli at Eli's funeral. I couldn't get there. And he asked to say a few words and he said something about himself – he was very proud of the fact that although he was an analyst he was willing to be interested in diagnostic criteria. But you know the problem is they don't get to their criteria now by research - they get a group of good people to sit around the table and discuss it all before they finally decide to accept whatever.

Is there a problem doing it this way?
I think there is. You end up taking what your group believes rather than trying to do something or other to find out more. In the case of our depression criteria, they were there to get into the study from which we hoped to discover more.

Where did you get those criteria from? Did you not just sit around as a group?

No it was just me. There were patients who fulfilled primary criteria who were the sickest. These were admitted to hospital often for electrotherapy. We ended up with about 8 or 10 of these. Then we had a second set of criteria which was any good psychiatrist we thought we could call on and they would agree with us clinically had a depression. It happened there were about 20%, which we felt that the psychiatrist would not agree with us. There was a last group we kept for cases where we felt couldn't make a diagnosis. Fortunately we didn't have many of those.

The reason we had to have these criteria starts with the question who do you put into the study. In the case of hysteria this was not very clear. The first step was if in a diagnostic clinic some people thought there was a case for psychiatric examination. Then they had to have at least one or two symptoms, that would traditionally be assigned to hysteria. Then there had to be no other diagnosis. Our final criterion was that follow up studies showed they did not develop some other illness. They still fitted the criterion for hysteria. Finally, also they fulfilled the criteria that Briquet and Savill had – seeing that they were the only ones that seemed to have any data.

Sam Guze went a step further. Our method was a better method for saying who hasn't got hysteria but Sam's went on to identify positively those who had. He worked out a system which I think they later called the somatisation syndrome which has 6 categories. Hysteria was located in this group but you had to have 25 symptoms to have hysteria – or Briquet's syndrome as he called it. I counted the symptoms ours had and I think we had 23 on average. This was true not only in St Louis. In Atlanta where one of my students was working he said there was a saying down there that hysteria patients averaged four major operations for every one that controls had.

Coming back to Stanley Cobb at some point he got involved with the psychosurgery story. Can you comment on that – it was a very 1940s story?
I don't know whether Cobb was in it or not. But a neurosurgeon named Tom Ballantine was one of my good friends. He was also from Hopkins. He first did a series of cases at the Veterans hospital and he wanted me to work with him. I wanted to work together also because we were good friends. But I wanted to work in controls and this and that and he wanted me to pick out the ones that would be useful and evaluate them afterward.

I'd seen all Freeman's cases when I was down at Saint Elizabeth's in Washington. I actually reviewed Freeman and Watts' book on Psychosurgery. It was beautiful neurosurgery. I think what I said in my review was that they had shown that you could do the surgery safely - for the most part, they had very few surgical complications. But the thing you couldn't tell was exactly what was wrong with the patients, exactly what they did in the surgery because they didn't have any autopsies and finally how the patients came out.

Much later on, Tom started doing another series of cingulotomies at the General and he asked me to work on it but again I didn't want to. The hospital tried to ban this. They had a guy called Leon Eisenberg, who was a professor in child psychiatry who had come from Hopkins. He was later kicked out of the General and went to the medical school where he became a professor of social medicine or something like that. But anyhow he opposed this work completely. Finally, the people from the surgery department came to me and asked whether these operations were helpful or not. I said I didn't know whether they were helpful or not, I was sure it didn't hurt very much but I said it was a dammed shame if the department of surgery couldn't do operations that were done in many other countries because the psychoanalysts didn't like it. You might as well close up the department of surgery. So they agreed good or not with certain protocols in place the operation in principle should be let go ahead.

What did you make of Walter Freeman, he's usually portrayed as being a zealot.
He was portrayed as worse that - as a sadist. At one point, he wanted some good pictures for his book or something and instead of using air he used some radio-opaque substance, which gave just beautiful ventriculograms. It didn't hurt the patients. But I think most people think this was almost murder. He was a very tough character. He was a neurologist rather than a psychiatrist.

He used to take his whole family, pack them into the car and drive West. Drive all night sometimes to go to hospitals where he used to do operations. On one of these trips, one of his kids went swimming in the river and got drowned. I don't think he was ever the same after that.

But he was also the organiser for the Board exams in neurology and psychiatry. I never studied for those exams - I had an impression that nobody ever failed or that actually there was a lot of regard to where you were trained and who you knew. It said on my card I was from Harvard and trained with Dr Cobb. Dr Cobb came down for the examination meeting the year that I was doing it and he stayed in St Elizabeth's with me the previous night. About 9 o'clock, I excused myself and said I'm going to study for my exams. I had made up my mind I was going to look up the brachial complex. He said don't waste your time. You'll know more than most of your examiners. This in fact was true. Walter said I'd got the highest marks of anybody.

I used to enjoy training people in psychiatry. I used to tell them that they would be up against a lot of psychoanalysts and that you couldn't say the wrong thing in public. This is what they want you to stay and these are the facts. What you tell them is your own business. I also used to enjoy examining. These young people all had the party line at the time. Neurology was the hard part of the exam. If you worked in a State hospital, even for twenty years, you might never have seen a case of Wilson's disease and maybe never heard of the disease. It was possible you didn't know enough to look for the ring in the cornea. If I had somebody who obviously was experienced and competent, I would try to make sure the questions kept to things like how did you diagnose bromide poisoning, what does the spinal fluid show in Paretics, how would you manage suicide risk. In other words I was asking to see if they had some practical knowledge that would justify passing them.

The way it was organised you had three young examiners and the chief examiner and only the chief examiner could flunk you. Let me tell you this story. One of my boys at one point was given a case of hysteria, so he gave a Freudian explanation. He talked about defence mechanisms and such like and then said that some people saw things this way. They said to him, don't you accept it? He said no, he thought it was nonsense, not scientific. He had been warned not to do this. They said well what do you believe? So he told them about our stuff and the stuff from Washington University. Then it was up to the Chief Examiner, who was a professor in neurology and psychiatry at NYU – Sam Wortis. Now Sam had worked down at Hopkins in the lab with me for a year and we were good friends. So he was listening to all of this and he said to the candidate are you from Boston? He said well I've heard about that system but I haven't met anybody who actually used it so tell us all about it. In the end after the candidate had explained it all, he said it sounded pretty good to him and me. Anyway the candidate had been told not to say any of this but as he told me afterwards he was dammed if he was going to say all those things he didn't believe in.

So don't get the feeling that we were all fighting each other all day long. I woke up in the morning and had three good meals a day and worked in my lab and gave talks. I never did have much of a feeling that I should try to do something to change it all. Maybe I should have.

But it did mean that you did mean you didn't get your life time achievement award until you were 92.
Yes it also meant that I didn't get to be the head of a department. I actually had an offer to go back to Hopkins to be on the staff, and I guess I might have made professor there, but I had to turn it down because the war had started and I'd been made director of the study on Effort syndrome. Besides the fact that I wanted to do the study, I was glad of the opportunity to avoid the front lines. The OSRD was like being in the Army. I think I could have got out of it but then they might have drafted me into the army as a doctor. Although the draft age was 38, doctors up to the age of 50 were made to go into the Armed Forces. They made it very clear that there was a war and we had to have doctors. The medical schools and hospitals had a list of those who they thought were indispensable and had to stay and the rest of the people all had to go into the Armed Services.

You knew Aubrey Lewis.
Yes I met Lewis several times. At one point he was being considered as a possible Chair for the Dept of Psychiatry at the Mass General but apparently he was rejected on the basis that he was a "cold fish and Jewish". I found him extremely warm when I visited him in the UK. We had a great deal in common. He was also anti-Freudian. He said to me that there was not a single Freudian in charge of an academic department in the United Kingdom and that he would see to it that the Freudians never did have any say in academic psychiatry in Britain. In my opinion though he was a considerable scholar of psychiatry rather than a researcher.

I used to advise trainees to go to St Louis or to the Maudsley. This was advice I gave to Paul McHugh, who went to the Maudsley for two years before coming back to Columbia and later ending up as Professor at John's Hopkins. Paul also believes that there are a number of diseases in DSM III that don't exist. He has lectured on multiple personality and written on it in the American Spectator for instance saying that most multiple personality cases were probably created by the doctor. I think I have seen one in my life.

Boston one of the places where they used to happen – with Morton Prince.
Yes and it always impressed me once he retired the disease retired with him.

Did you ever meet Prince?
No. When I came here there was a C McVeigh Campbell who was professor of psychiatry. He was very concentrated on money and costs. We were at a meeting and I said that most people with manic-depressive disease did well if they didn't commit suicide. I thought we should put quite a lot of emphasis on preventing suicide. He said if you put everybody in a hospital you thought might be suicidal you would put too many people in a hospital and that would waste money. I felt that depression was like appendicitis – you couldn't tell which one would pop and which wouldn't. So I recommended an admission in all cases and thus far I have never had a patient who committed suicide.

What can you tell me about Adolf Meyer?
I think when I was in Hopkins, I was the only student interested in psychiatry. Most of them didn't know what Meyer was talking about. If you knew what he was talking

about then you could understand what he was saying. He would come around the lab every day and ask you questions, but I actually never accomplished anything in his lab except making a brain model. It was a necessary part of training with Meyer at that point to make a brain model. I spent a lot of time getting the corpus callosum right. At that time we didn't know much about the thalamus or basal ganglia. I'm sure some people did but we didn't.

He is said to have been against diagnosis, theoretically. He had a system in which diseases were called ergasias. Thymergasia was mood disturbance, parergasia was schizophrenia, ??ergasia was neurosis. There were five neuroses - anxiety, hysteria, obsessional, hypochondriasis. Parergasias came in four types, paranoid, simple, hebephrenic and catatonic. When I went down to Washington I used to stop in and see him. He had retired, he actually had a stroke. Anyway years later, when I called in to see him, I said to him Dr Meyer schizophrenia has four types paranoia etc, parergasia has four types paranoid etc, what really is the difference between them. I had asked some of the disciples this and they couldn't answer this. Do you know what his answer was? He said Dr Cohen if anybody but you asked that question I would have gotten real angry. So I still don't have an answer. Some of his disciples said that it was more appropriate to talk about reactions rather than diseases in the case of disorders where the cause wasn't known. On the other hand Esther Richards who actually ran the hospital used the terms schizophrenia and manic depressive disease.

Dr Meyer while professing not to believe in diagnosis was the best psychiatric diagnostician I have ever ran into. He taught us things like looking at body posture. Mood disturbances sat around more, schizophrenics were more angular in their posture. Preoccupation with death was found in manic depressive disease and not in other diseases. He had a lot of rules like this while at the same time saying that there was no such entity. You found also that he got up at almost every neurological or psychiatric meeting and made some comment or criticism. I think his general feeling was we didn't know the answers, which I think is still true.

When the diagnosis of schizophrenia began to become very common in the United States, Meyer argued that many of these patients actually had manic-depressive disorders. In contrast to the views at the time, he argued that manic depressive disorders could have delusions and auditory hallucinations. In his opinion it was mis-diagnosed cases of manic-depressive disorders that accounted for many of the so-called cures of schizophrenia.

He visited my lab near the end of the War. He looked over the charts, the blood lime table, rates of respiration and all the different things. One of his theories was that anxiety neurosis was a form of hypothyroidism. Others saw it as a form of convalescence. We asked these people the same questions that we asked thyroid patients but whereas the thyroid patients had about 4 overriding symptoms, the anxiety neuroses had up to 15 symptoms. We had a nice chart of this and he wanted a copy of it. Now in the laboratory we had a lot of equipment for spirometry and all sorts of other things, along with a technician. As he was leaving, his final summary was "its absolutely wonderful Dr Cohen that you have been able to fit so much equipment into such a small room".

He went to all the meetings and always had something critical to say. But everybody acknowledged that he was our mentor. And he also controlled who got to be a professor, until he retired.

After Meyer retired his mantle was taken over by Karl Meninger, Francis Braceland and Bob Felix and things went very psychoanalytical,
Yes, let me tell you how they got into power. Menninger was in the Surgeon Generals office. There was an Irish doctor from here in Boston who used to work at the State Hospital in neurology and psychiatry, who was the Psychiatric Surgeon General. He actually didn't do anything in it but anyway he died. And they brought in one of the Menningers. At the end of the war the government gave a funds to have 200 patients and 200 doctors psychoanalysed. They all went to the Menninger clinic - I forget how many $1000 of custom it was.

Now as regards Felix, I'd known him because he was a doctor with the Marines at the Coastguard Academy. He sent me patients and I'd see them. I thought he was a fool. When they were gonna pick who was to run the NIMH, I was on the committee and I said I didn't think he was the right person for the job. In fact I think I said a quarantine station in the Gulf was about right for him. Anyhow he naturally heard about this. He had gone out to St Louis and become professor of psychiatry at the Catholic medical school there before he went to NIMH.

He was very much interested in preventative psychiatry. They had a meeting in Chicago to which doctors from all States were invited. I went out with George Ewing. He was representing Missouri and I was representing Massachusetts. This was the meeting where they decided to set up the new psychiatric clinics. Mass and Missouri voted not to set them up. But anyhow they had one session there on preventative psychiatry and he gave a talk. He told us how to prevent schizophrenia. Finally, I asked him exactly how do you do that. He mentioned the work of Eric Lindemann. Of course Lindemann's stuff here in Boston was flawed. At the end of the meeting several of the doctors as well as psychologists and social workers stopped me and said they were glad I asked that question. They knew they were supposed to prevent schizophrenia but they didn't know how to do it and they felt he hadn't answered the question.

Felix was one of the few people that I knew about who actually didn't like me because he had heard of course that I had not recommended him for this job. He once got up and opposed something that I did and said if people had followed my advice that they wouldn't have made the great advances that they did. Great advances included appointing him to the NIMH. But he was really quite second rate.

Francis Braceland was a psychiatrist and a psychoanalyst. He had been involved in the Naval Service during the War. He later ran the Institute of Living in Hartford Connecticuit I think. They did a good job on patients so I think at a practical level he was very good. At the theoretical level, he just talked the same nonsense as all the rest. He was a nice fellow though. Felix was not a nice fellow or so people said. These are all impressions and memories I may be wrong.

You were saying that it was feasible to do research because no one had done any before.
Well in those days anyhow it was really worthwhile to do research in obstetrics and psychiatry - both of which I did. There were a lot of important questions to be answered and nobody had ever done the work, so anything you found out was a contribution. This was before antibiotics, before sulpha drugs, before the birth control pill. Take bacterial endocarditis, everybody who had this, including some of my friends - 100%. When penicillin came on stream, the Armed Forces got all of it. Under Chester Keefer, they gave it to 110 patients to try it on all diseases. They gave it to TB and found it didn't work. They gave it to syphilis and found did work. Chester was in charge of several cases of bacterial endocarditis and gave it to them and

wrote a paper saying it didn't work. We're depending on my memory here but a pediatrician stole some and gave it to a child and cured him and then gave it to a second child and cured him and this was how the cure for bacterial endocarditis was discovered. To have a disease which everybody including a couple of your friends died and then to get something where 80% got well was amazing. Jim Thomson who worked with me on the pregnancy papers and some of the psychiatric studies said the truth of the matter was 80% of the patients didn't get well. If you got young healthy patients into hospital with bacterial endocarditis you had a high cure rate but if you took the kind of patients they had in Bellevue, picked up in the gutter, and if they had bacterial endocarditis they didn't get better no matter what you gave them because they were in very bad shape.

I don't say that the psychiatric medicines are the same as the antibiotics. The evidence is that they help some but I'm not sure how much. As a matter of fact I am not convinced that lithium is all that wonderful.

I remember a case at Mass General when they presented this manic person they had given lithium to. The argument was that it would cure the manic episode and prevent another attack. But this girl was sitting there and waved to the people in the crowd. She kept talking on the way in and on the way out and answered questions in a very manic way. She was not delirious anymore but apart from that she didn't seem to me to be much better. Of course the reply to all this is if you're so critical why don't you do it yourself. Well some of these things I don't know how to do. I'm also lazy. I don't see the point planning a study to do something if at the end of the study you still won't know the answer to the question - there is no use doing such a study. Unless, of course, you do it to get money from a grant.

You brought a medical model into psychiatry but you also ended up doing some very medical research toward the end of your career.
Yes, another line of research began when we coincidentally had two patients in whom the glosso-pharyngeal nerves were cut intracranially. We also did this in four monkeys. Now if somebody asked you what does the glosso-pharyngeal nerve do, how would you answer?

It innervates the tongue and pharynx.
What we found was that in the patients and the monkeys, it didn't seem to have any impact on sensory function whatsoever. They still had sensation in their mouth and taste. Their gag reflexes were normal. But if you know the anatomy of it, the glosso-pharyngeal nerve has a big branch that runs down to the carotid sinus. We found out that when you cut the nerves, if you give cyanide gas to the patient, their blood pressure doesn't drop. The response to CO_2 is questionable – we couldn't do enough to find out. Now the bifurcation of the carotid artery corresponds to the fourth branchial arch in the fish and this gives rise to the chemoreceptor sense. We proposed therefore that this nerve going down to these carotid arteries has a chemoreceptor function and does not have the other traditionally described functions at all. Now if you ask the surgeons well what about the glossopharyngeal neuralgia, how do you treat this - you cut the glossopharyngeal nerve don't you. If you talk to the surgeons doing this, they say its better in addition to that to cut a few twigs of the vagus nerve. So they are not cured by 9th nerve resection but by a combination of 9th and 10th nerve. We found that amusing - something that was in all the books but you couldn't find any evidence for it.

Another thing we looked at was Cheyne-Stokes breathing. This was believed to be a form of heart failure. But what we found was that it had nothing to do with heart - it had to do with patients who had strokes or tumours were that involved the bilateral part of the mid brain. We had 62 autopsies from 200 patients and it became clear most of them did not have heart disease or heart failure. Well now some people, like Haldane, argued because you could relieve it with oxygen that this seemed to prove that it's caused by anoxia, which is probably caused by heart failure. We just put a mouth-piece on the patients mouth and found that also made the apnea go away. If you had them simply breathe air rather than oxygen that also made the phenomenon go away. If you took a pen and simply stimulated around the lips - that also made the apnoea go away. So we think that putting the mouth piece which was what Haldane did produced the effects, it was not the oxygen.

We showed this in 40 cases. Another thing that we found about Cheyne-Stokes breathing was that it occurs in men much more than women. There was no explanation for that. Although people like Dr Meads, professor of medicine at Harvard, a good friend also knew this. I used to work for his thyroid clinic. We found out there that the patients didn't know there was anything wrong with their breathing. The family knew when they stopped breathing because they thought they were going to die. But if you asked the patient was there anything wrong with them, they said no.

Another thing was that they had apraxia of breathing. If you told them to take a breath during their apneac period, they could not do it but they could do it otherwise. So we thought Cheyne Stokes breathing was related to bilateral lesions in the brain stem and secondly that they had symptoms without knowing that the symptom was there. Now that was quite interesting. But we didn't do enough with it. We published the paper on the apraxia and 2 or 3 papers on the rest but we never published a paper on the whole 200 patients we had.

Professor Mandell Cohen – Telephone Interview 2nd Sept 1999.

Mandell Cohen was born in Mobile Alabama 8th March 1907.

He trained in medicine at John's Hopkins University in Baltimore and later at Harvard Medical School. While at John's Hopkins he studied psychiatry with Adolf Meyer whom he regarded as a fine person. He feels that Meyer's contribution was often not understood. Meyer is portrayed as a man who was against psychiatric diagnosis and disease but yet was one of the best diagnosticians that MC ever met.

Meyer's writings and formulations were very complicated. Rather than speak of diseases he talked of Ergasias. Rather than speak of schizophrenia for example he talked of Parergastic reactions. Just as there were four types of schizophrenia simple, hebephrenic, paranoid and catatonic, there were four types of Parergastic reactions in Meyer's schema. MC Adolf Meyer at home on one occasion, after he retired, asked him what was the difference between a Parergastic reaction and schizophrenia. Meyer's reply was "Mandell if anyone else but you had asked that question, I would have become very angry". Others of Meyer's disciples/pupils argued that it was appropriate to talk about reactions rather than diseases in the case of disorders where the cause was not known.

MC had great respect for Meyer's diagnostic and clinical skills. He points out that Meyer often argued that, at a time when the diagnosis of schizophrenia had become particularly common, many patients in the United States attracting the diagnosis of schizophrenia were actually Manic Depressive Disorders. In contrast to the prevailing views at the time, Meyer argued that Manic Depressive Disorders could have delusions and auditory hallucinations. In Meyer's opinion it was mis-diagnosed Manic Depressive Disorders that accounted for many of the so called cures of schizophrenia.

After working/training at Hopkins with Adolf Meyer MC moved to medicine and physiology in Boston at Mass General. In 1935 when a department of psychiatry opened up at Mass General he joined this. Stanley Cobb was the First Professor there. Cobb moved from Boston City Hospital where he had been professor of neuropathology. While biologically based in the first instance, Cobb later became psychodynamically oriented and recruited to the department Eric Lindemann, Feinsinger, Hanns Sachs, Helene Deutsch and her husband Felix Deutsch. Gradually the analysts took over conferences etc.

MC became alienated from this new department and left it moving back into medicine at Mass General in 1942 and later into neurology. During this period he got a Rockefellow Fellowship, travelled to Los Angeles where he worked with J M Neilson who was doing work on aphasia, and spent time in Washington at St Elizabeth's Hospital, which provided him with his first exposure to a large hospital with a large population of chronic patients as well as to forensic psychiatry. This was at a time when psychiatrists were still alienists and schizophrenia was still dementia praecox.

From 1942 onwards, while based in medicine in Mass General, he collaborated with Paul Dudley White, an eminent cardiologist, through to White's death in 1970. They collaborated on a series of papers starting first with a condition called neurocirculatory asthenia by the cardiologists, also called Da Costa Syndrome, Effort Syndrome or either Anxiety Neurosis or neurasthenia by psychiatrists then and very often these days called Panic Disorder. Because of the large series of names and ideas about this condition, they felt early on that it was necessary to define the patients that they were studying. This led to the notion of diagnostic criteria. In essence diagnostic criteria were used according to MC because at the time he didn't know enough to do any differently.

There were no explicit ideas in their mind at the time that operational criteria as such were important. There were no philosophical precedents for taking this point of view. It was simply common practice in the rest of medicine, particularly in cardio-respiratory conditions to define the patient samples being studied. This seemed particularly indicated in the case of anxiety neuroses of this type, given the profusion of names and of ideas regarding the condition.

Working on this group of patients, they noticed that there were a lot of abnormalities in response to challenges of various sorts. These patients produced high blood lactate responses for example. No abnormalities were present under basal conditions.

This work led to the first production of anxiety attacks experimentally using carbon dioxide inhalations.

Cohen, White and colleagues also went on to produce operational criteria for Manic Depressive disease when Cohen had moved to Tufts University as a Professor of Research Psychiatry. These early criteria map onto those later produced in DSM III.

They also produced diagnostic criteria for hysteria. Work on hysteria was done on patients drawn from Pratt (?) Diagnostic Hospital. They produced five papers on hysteria looking at a group of patients with over two hundred controls. They described a condition that bore similarities to the condition described by Briquet a century earlier and they noted these similarities. Where others working on hysteria were describing single cases and were describing cross-sectional aspects of the case, under an analytic influence, Cohen and colleagues were describing common factors in large samples and discussing the longitudinal history as well as family backgrounds of the patient set. They noted that hysteria often ran in families.

This work, in particular the work on the anxiety neurosis, was sponsored by the OSRD, an off-shoot of the National Research Council as part of War related research. A large number of soldiers during the course of the First World War in particular had effort syndrome and the Americans and the British had up to two million soldiers affected by this – a greater number of individuals affected than by any other condition.   The hope was that research would indicate the cause of the disorder and possible treatments. Cohen and colleagues were able to show that this disorder ran in families, that it had often been there before the war although it may have become worse during conditions of war.

One of the early papers on hysteria was written with Stanley Cobb as co-author and in this they explicitly mention that researchers shouldn't talk about the mind and the body as though these were distinct entities with the mind being some incorporeal thing. Their explicit understanding was that everything was physical and therefore should be ultimatEli localisable in physiological terms. This underpinned the notion that the disorders should be studied medically and as such that operational criteria were appropriate.

Eli Robins was a resident training with Cohen who became a personal friend. Cohen and his wife knew both Eli and Lee Robins and were invited to their wedding. Eli Robins worked on hysteria in men. In order to recruit patients he went to Army Hospitals and recruited a different population to that that was available in the Boston Hospitals to Cohen and Dudley White. Eli Robins demonstrated that the clinical picture of male hysteria was different to that in females.

Robins later moved to St Louis and established a department with Sam Guze which was the only department in the country where there were no Freudians. They concentrated on factual research. William Sergant writing about the US Psychiatric scene in the early 1960s commented that there were only two

Chairs of Psychiatry in the US at the time that were held by non-Freudians, one being Robins in St Louis and the other being Cohen at Tufts.

MC sees the relationship with Eli Robins very much as a Moses and Joshua relationship, with Moses setting the children free to some extent but Joshua being the person who organised the homecoming and established a State and Government. The success of Eli Robins he puts down to the fact that Robins and Sam Guze were able to take this approach in a department where there was no internal opposition from analysts.

In 1985 on the occasion of the 50th Anniversary of the establishment of the Mass General Hospital Department of Psychiatry the alumni of the department got together for a meeting. MC could not be present as he had to attend a meeting elsewhere. He had been completely detached from the department for the preceding 40 years. At the meeting in 1985 however Gerald Klerman addressed MC's role as an originator of much of the modern work on anxiety neurosis as well as an originator of the concept of diagnostic criteria.

MC had contact with Aubrey Lewis in the United Kingdom who at one point was being considered as a possible Chair of the Department of Psychiatry in Mass General. He was rejected on the basis that he was "cold fish and Jewish". MC later visited Aubrey Lewis in the UK and found him an extremely warm individual. They had a great deal in common and talked the same language. Lewis appeared to be also anti-Freudian mentioning to MC that there was not a single Freudian in charge of an academic department in the United Kingdom and that he Aubrey Lewis would see to it that the Freudians never did have any say in academic psychiatry in the United Kingdom. MC's view of Aubrey Lewis was that he was a considerable scholar of psychiatry rather than a researcher.