THE NEO-KRAEPELINIAN REVOLUTION
SAMUEL GUZE

Can we begin with when you actually began to work in Washington University?

Before I went into psychiatry, I was in internal medicine. I was in medical school at Washington University World War II, after which I had an internship in medicine at Barnes Hospital, followed by two years on active duty with the army. I spent most of these years overseas, in the Philippines and in Japan. I was the public health officer for the Northern part of the Nagasaki military government team for about a year. When I came back, I had a year of internal medicine at Barnes and a further year in medicine at one of the hospitals affiliated with Yale. But the first year I came back from the army, because there were so many returning medical officers, the rotations of many of us were rearranged to accommodate the needs of all of us. As a result I and others in my cohort spent a lot more time in the outpatient clinic than was usual.

I found that experience very frustrating. I remember going to talk to Dr Barry Wood who was then the head of medicine. I said “Dr Wood I don’t know what’s wrong but I’m working hard in the medicine clinic and I see a lot of patients, trying conscientiously to do good work. I do a full history and a complete physical and I get appropriate X-rays and laboratory tests but, when it’s all over, I don’t feel that I’ve learnt very much. What’s even worse I don’t feel as though the patients have gotten very much out of it”. He looked as though he fully understood what I was saying but he didn’t commit himself to that. “Well”, he said, “Sam that’s a problem in medicine. We just recruited a new man to the Department of Psychiatry whose job is to build links between the department of psychiatry and internal medicine. His name is George Saslow, I think you should go and talk to him”. So I did and he listened to what I had to say and then he said “if Dr Wood will release you from some of your medical commitments why don’t you come and work with me?” Dr Wood agreed.

George Saslow was a most remarkable teacher. He was also a most skillful clinical interviewer. He had a clinic where he would see two new patients per day, three afternoons per week. Generally, the clinic was staffed by a medical resident like me, a psychiatry resident, and some social work and psychology students. He would interview the new patient to bring out what he thought was important. Then he would assign the patient to one of us and follow up by regular supervision of what went on.

He had a PhD in physiology followed by a psychiatry residency at the Massachusetts General Hospital. Despite the latter training, he was very anti-psychoanalysis. He had a remarkable ability to interview patients so that we learned about the place of the illness and its effect on their lives. I recognised early that the best thing I could get out of that experience was to emulate him as much as I could. So I tried to practice what he did with patients and found that I got similar results. After what was a very good year, I went to New Haven, as a third year medical resident, expecting to come back after that to a fellowship in haematology in St Louis.

Half way through this year, I had a call from George Saslow, who said "Sam, we've just received a 5-year grant from the Commonwealth fund, including money for fellowships, and I think you'd be a natural to come back and be our first fellow". At that time, the Commonwealth Fund was very interested in medical education and in trying to inculcate an awareness of social and personal factors in health and in illness. I pointed out that I had left St Louis with an understanding that I would return to be a fellow in hematology in Carl Moore's laboratory. Saslow gave me a week to think about it. I talked to my wife and then I called Carl Moore, who said: "Sam I've got a lot of people who want to work with me, so it won't cause me any disruption. I'll miss having you working with me but if this is what you really want to do, do it". I learnt later on that he really didn't understand this interest of mine but he had confidence in me, which was very lucky, because after Barry Wood went to Hopkins in 1955, Carl Moore became head of medicine and later Dean and Vice-Chancellor. This was important for the rest of my career. So I called George back and accepted his offer.

The fellowship, because of George Saslow's faculty appointment, was in the Department of Psychiatry. In retrospect, I recognise that I had a slight misgiving about this. I thought of myself as an internist and I wasn't sure that I wanted to have anything to do with a psychiatric identification. I was skeptical about psychiatrists. When I was a medical student, our psychiatric training was simply appalling. But the fellowship year working with George was marvelous. He must have thought I was doing pretty well because toward the end of the year, he began to have me substitute for him when he was away. To cut a long story short, I ended up with 3 years of training in the psychiatry department so that I was eligible for examination by the American Board of Internal Medicine.

In 1953, I was appointed Assistant Professor in Internal Medicine with a joint appointment in psychiatry. At the same time, David Graham, who had been trained at Cornell with Stewart Wolf and Harold Wolf, was brought in to the department of medicine with a strong interest in psychosomatic medicine. David Graham and I worked together – our job was to make medical students and residents more interested in the psychosocial aspects of medicine. We worked together with no difficulty. In 1955, Barry Wood went to Hopkins, his alma mater, Carl Moore became head of the department of medicine, and George Saslow was offered a position back at the MGH, in the department of psychiatry at Harvard. He offered to have me come along – to do at the MGH what we had been doing in St Louis. He sent me to meet Eric Lindemann, who was the new head of psychiatry at the MGH. George and he had been friends in the past. After visiting Harvard, I decided not to move. When Saslow left St Louis, much to my amazement, I was offered his position, which was very flattering. But it was stipulated that I would have to have psychiatry as my primary appointment. After some internal debate about this, I accepted the offer.

During this time Ed Gildea was head of psychiatry. He had been recruited from Yale in 1942. During the War years, he essentially had to do everything because everyone else was off in military service. When they came back, Ed gave many of them appointments and they were almost all psychoanalysts. Ed himself was not a psychoanalyst. He was a pioneer in biological psychiatry. He had done some very early work on thyroid function. His wife Margaret, who was very bright and a very good clinician was intrigued by dynamic psychiatry and had actually been psychoanalyzed by Jung at one point. She came from a very wealthy Chicago family, who had been in the plumbing business. Her father, Lille, was a Professor of Biology at the University of Chicago. Her mother, who was a physician, had never practiced.

Margaret also had links with F Scott Fitzgerald.
I understand that she dated him. She was a very bright, handsome, self-confident, charming woman. I have only enthusiastic praise for her. She had a remarkable ability to compartmentalize. She could discuss Jungian or Freudian ideas but she also had the ability to take care of very sick patients. It is fair to say that in the early 50s, the department was predominantly analytic.

Well by a curious quirk of history, our medical school, going back to the 30s, was a strict full-time faculty, with the department billing and collecting patient fees and using the money as part of the departmental budget. Many of the returning medical people felt that they had lost several years of their careers and opportunities to make money. At the same time many had a Freudian rationalisation about why it was important to charge money, collect, and keep their patients' fees. So they went to Ed Gildea to complain about the system. But, there was no changing the full-time system at that time, so Ed offered them a place on the part-time faculty. They could go into private practice and volunteer to help us with our teaching, they nearly all accepted that arrangement. That left in the department primarily, Eli Robins, George Winokur, and me.

Was George Ulett there?
He was there, a pioneer biological psychiatrist, but he was working down at Malcolm Bliss, which was the public hospital. He was doing that because he wanted access to patients for his research.

Eli Robins was from Rosenberg Texas. He had gone to Harvard Medical School. He was a resident in psychiatry there and he had just come out to St Louis to work with Ollie Lowry, who was head of pharmacology and who was developing all these marvelous techniques for measuring microscopic quantities of chemicals and molecules. Eli had been greatly influenced by Mandell Cohen in Massachusetts General Hospital. He was very anti-psychoanalysis and Eli, even though he had had some personal analysis with Hanns Sachs, concluded that psychoanalysis was not the right approach to psychiatry.

Was this when he had his first episode of MS and the analyst thought it was hysteria?

That happened when Eli went into the army. The neurologist and the psychiatrist who were called in thought it was hysteria. Eli went to see Raymond Adams who was the head of neurology at the MGH, who said that idea was ridiculous – he decided that Eli had probably had an episode of polio but, in retrospect, we nearly all think that it was the first episode of his MS. Eli, while working with Ollie Lowry, was a very effective proponent of psychiatry as a medical discipline and was committed to the importance of neurobiological research. I shared and fully accepted these views.

George Winokur had come from the University of Maryland. He was planning to go back to go into personal analysis in Baltimore and then into practice but Eli influenced and the whole atmosphere in the department influenced him too. This was true of Ed Gildea also. When you got into small groups with him, it was quite clear where he thought the future of the field lay. Ed was a big man, bald, very classical Irish looks, very smart, very well-read in everything but surprisingly inarticulate as a lecturer and so he was not a very effective leader in that sense. But he knew what was important and acted accordingly.

Against this background Eli, George, and I set out to develop a plan for the future of the department, which we would present to Ed Gildea. We actually developed one with nearly total unanimity on all points because we understood each other's thinking so well. We worked out what we would do for 2nd year, 3rd year and 4th year medical students, each of the years of residency training, how research would be cultivated in the department. When we made a presentation to Ed Gildea, he was taken aback. He said he didn't know if we were going to be able to succeed, pointed out that we were going against the tide, but concluded that, if that's what we wanted to do, go ahead. And I must say, he held the line. Almost immediately, he got an enormous amount of criticism and many complaints about us from people all over the country, including some from St Louis. He brought us in every so often to be sure that we knew what so and so said about us. He suggested that we might tone some of the things that were most upsetting to people. In response we insisted that we did not agree with what was being said, that we were trying to be polite and reasonable, but we were saying things we believed.

We insisted that psychiatry would make the greatest progress by being viewed as a medical discipline. This viewpoint has all kinds of implications as to how patients are thought about and approached. In addition, as in the rest of medicine, research offered the only hope for the future of our field and it had to include clinical studies, basic laboratory work, and basic epidemiologic work. Our basic outlook on psychoanalysis was that there appeared to be no way to test it and there were such limited data to support it. We were young and probably "full of ourselves" at that point, but when someone would challenge us, invariably reply: "well give us the data, show us the evidence". Of course there weren't very many systematic and controlled data. Thus we made enemies who complained to Ed Gildea.

What you've outlined there was what was later termed the neo-Kraepelinian position. The first printed reference to this credo I have is from an article by Gerry Klerman in the 1970s. Where these goals as explicitly articulated as you've just outlined them as early as the mid-1950s or is there anything earlier than Klerman's version actually in print to this effect?
A series of papers in the 1960s described specific studies that were discussed in terms of our overall philosophy. One was an article of mine in the American Journal of Psychiatry in 1967. There was also a paper in 1970 with Eli. Perhaps the first "philosophical" paper was "The Need for Toughmindedness in Psychiatric Thinking" in the Southern Medical Journal in 1970.

In 1963, Ed retired and we were delighted when Eli became head of department. Ollie Lowry, I think, played an important role in this. That was of course a very important political moment for us because we knew then that we could proceed without ambivalence. We started going forward at a rapid rate. We put a lot more emphasis on encouraging residents to do research. We generated more space from the school for academic purposes. From the mid-50s to the late 60s, we bucked a national trend – all of our clinical research grant submissions to the NIMH were turned down. If the grant was for basic pharmacology or biochemistry, we did very well but we couldn't get a single clinical study through NIH. In retrospect, I think we were lucky because it meant that we didn't have the money to hire all kinds of technicians and assistants, so we and our residents did all the work. This led to very close relationships with the residents, who were also our research collaborators.

I've also heard that at this point in time when people from St Louis were doing their boards that they were often failed by hostile examiners?
Well no they didn't fail because we recognised that we were up against a problem so we did very careful, detailed coaching. I remember Eli and George taking me aside when it came to doing my boards in psychiatry spending hours with me. They raised the kind of tough questions I would be faced with. I said then and encouraged all our people afterwards to approach this board business with a "covert, hostile, competitive attitude". It had to be covert, but "you are there to beat the examiner". "Take every opportunity to be persuasive that your thinking is solid". For example, I saw a patient during my board examination who had some kind of puzzling clinical picture. We were convinced that this patient might be in an early stage of some dementing condition. I suggested some kind of psychological tests. My examiner wasn't too happy about that. But she wasn't as sophisticated as she should have been about the whole gamut of psychological tests and as soon as she began challenging me I suggested that these tests could be viewed as comparable to projective tests. We got going on that and she got caught up in it. That's what I meant by covert hostile competitive – I was determined that I was going to outwit her. So we did fairly

well with these exams. Those who failed almost always did so because they didn't take our advice and got into arguments. They forgot the insistence on being "covert".

As I've heard it this was a time at St Louis when psychoanalysis consisted of one lecture, given by you, which essentially came down to a set of definitions – this is all you need to know about this stuff!
No. I gave lectures to the medical students and the residents and one introductory lecture concerning psychoanalysis had a handout, which had something like 60 defined terms on it. I said that if you understand these terms, what they mean and how to use them, there's no reason why you can't hold your own in any context. Actually this got so popular that I made a videotape, so I wouldn't have to repeat it over and over.

Now at this time, the psychoanalytic movement in St Louis got stronger. At first, they were an affiliate of the Chicago Psychoanalytic Institute. Those who were training used to spend the weekend to Chicago, but, after a while, they raised enough money to start an independent institute in St Louis. It was clear that there was going to be a long protracted "war" with our department.

Things were going along relatively well when Eli became head of department in 1963. But the last 6 months before that he was having some very funny involuntary movements of his left arm. At first a minor twitch, then a more obvious jerk and then sometimes his arm would end up over his head. Bill Landau, who later became head of the department of neurology, said he had never seen anything like it before. In fact, no neurologist came to St Louis that Bill Landau didn't drag around to see Eli and none of them was able to make a confident diagnosis. In November of 63, two days after President Kennedy was assassinated, Eli called me. We were living just two blocks from each other and he asked if I could pick him up because he didn't feel like driving. When I picked him up I asked what was wrong and he said, "I don't know, I just don't feel right and I didn't have enough confidence to drive the car". Within about 24-36 hours he developed manifestations of transverse myelitis and then everybody concluded that the entire illness was some atypical demyelinating illness, probably MS.

Now that was a serious problem, because for almost a year after that he was at hospital or at home. George Winokur and I divided up the work. I would stop off on the way home every day and bring him his mail and review things with him. We did that for a year. The sad thing is that he never had a full remission. It was a just steady, inexorable progression and he became more and more incapacitated. He went from a cane to a wheelchair to an electric wheelchair. It became clear that he couldn't continue as Head of the department much longer.

In the early 70s as our department's reputation became more widespread, George and I used to get invited to give talks about what we were doing. We'd present research results and we were publishing a lot because we had all the residents working with us. I began to get some nibbles about whether I might want to move elsewhere and in 1974, I was offered the Chair at Hopkins. It was a very tough decision. Hopkins was still viewed as our number 1 academic medical center even though the psychiatry department had drifted down. It was clear though that it had just awakened from a slumber of about a decade and they started developing plans for the future. I spent 4 or 5 very trying months but I finally decided not to go, primarily because of Eli's illness. I felt that if I left, Eli would not be able to carry on - George had left at this point. I went to see Ollie Lowry, who was dean at that point and I put my cards on the table. I said "look, I don't want to go, although this is a wonderful opportunity for me personally but I think it will be very bad for the department". The Dean agreed. I said, "well what I need is a proposal that will protect the department". So he said that he would recommend to the Executive Faculty that the school give me a unique commitment to the effect that if and when Eli has to step down, I would automatically replace him as department Head. On that basis I turned Hopkins down. The following year, Eli got much worse and he resigned and I became head of the department. Eli continued to work but steadily less and less. Even so he was a very important figure in the department because he was a symbol of the importance of diagnosis, research, and neuroscience.

In 1971, Carl Moore, who was our first Vice-Chancellor for medical affairs decided that he didn't want to be Vice-Chancellor anymore. M Kenton King, the Dean in 1965, had asked me to be "minister without portfolio", as he put it, in his administration. He called me "Assistant to the Dean". I worked only on things he assigned to me. It was understood that when he was out of town, people would turn to me. In 71, when Carl Moore stepped down, the search committee recommended me to be the next Vice-Chancellor. George Winokur was offered the Chair in Iowa at this point and took it. I think he did the right thing but he was a big loss to us. From 71 to 74, therefore, I was working very hard between helping run the department and being Vice-Chancellor for medical affairs. We had other people who rallied around at this point and helped out – Paula Clayton, Mark Stewart, and others but they were still relatively junior. When Eli stepped down, the school agreed that I would continue as Vice-Chancellor in addition to being Head of the Department. I said that for several years I had been straddling both positions and I thought I could do it but if the school had any misgivings about it I would step down as Vice-Chancellor. Anyway, they decided they didn't want me to make a choice, so from 1975 until 1989 I did both jobs. I learned that I had a talent I did not understand until then – I am a ferocious delegator. I could delegate anything to anyone and let him or her report back to me at a comfortable interval. .

In this period, a lot of things happened. We built more buildings, we got more space, our research budget began to grown and, beginning around 1975, we became successful at getting clinical grants for epidemiology and other work.

Can I go back to 62, for a moment, when the Kraepelinian program began to come into view. Where did the notion of operational criteria come from?
Actually, George Winokur and I were introduced to those ideas by Eli, who in turn had been introduced to them by Mandell Cohen. I'm not certain where Mandell got some of those ideas. He came up from Alabama. He had a rich Alabama accent. He was trained in both neurology and psychiatry. He was very much influenced by Paul Dudley White, a cardiologist. They did many studies together on anxiety neurosis as it was called at that time, and I think some of the Mandell's ideas came from Paul Dudley White. Eli then brought them to St Louis. George and I were immediately persuaded that this was the way to go because it seemed entirely consistent with our understanding of how medicine generally worked. So these ideas were there from the early 50s and once Eli persuaded us of their validity, we began to use them. Our early studies were crude and unsophisticated but we quickly learned. My early studies were on hysteria and criminality and George started doing studies on depression early on.

But you weren't calling them operational criteria at this point?
No. Now that came later. I think George Winokur was the one who adopted that term. I think Percy Bridgeman, a professor of physics at Harvard and a Nobel laureate, in the 40s or 50s, was writing about operational criteria as a philosophical concept in physics and I think George or Eli picked the term up from there. There wasn't a day that we didn't meet together, sometimes have lunch together and talk about our reading and the residents and so on, so it can be hard to pinpoint where our ideas actually started.

Now in 1962, Morton Kramer organised on meeting in the UK on why there was so much schizophrenia in the US and comparatively little in the UK. Had he any links with you – because ultimately what he began there fitted in so well with what you were doing?
He was at Hopkins in the School of Public Health. There were only very slight links at the start. We were familiar with his work, I'm not sure he was as familiar with us.

Well would he pay any heed to anything that wasn't on the East Coast?
Well, Kramer was not a physician. He had a PhD. And Washington University psychiatry at that point was not very well known. The Medical School was well known in medical circles but Kramer wasn't in those circles.

But I should now mention Lee Robins. Lee had a PhD in sociology in Harvard, under a teacher whose concepts were exactly the opposite of operational criteria. But, she was influenced by Eli and through him by Mandell Cohen. All of us began to realise that a very important vehicle for learning more was to do follow-up studies. St Louis in the 20s had a child-guidance clinic, sponsored by the city or the state. Lee heard that all the records were available and she started studying them. A private foundation helped her get started and after that she had no trouble getting money from NIH. I think it was at that time that Kramer's thinking began to influence the rest of us because Lee got interested in his work in epidemiology and we all had a lot of admiration and respect for her.

The rest of us, in addition to Lee, began to do more traditional epidemiologic studies. Lee did some very interesting studies of the long-term outcome of the children seen in the child guidance clinic. The whole department was kept informed about what everyone was doing because the department wasn't that big at that time. As the data got churned out from Lee's study, everyone knew what was coming out and other people starting thinking about applying this approach to other areas. Then we had Kramer come out to talk to us and that's where the linkage was established.

Now out of the Kramer initiative came the famous videotape that Bob Kendall took around psychiatric departments in the US and the UK, which led to the International Pilot Study of Schizophrenia.

I can't remember how we first got to know about Bob Kendall's work but early on we became aware of his thinking and we felt we had a kindred soul. In fact we felt that in British psychiatry, we had a kindred group. I'm sure that more people in our department faithfully read and studied the British Journal of Psychiatry than any other psychiatric journal. We believed there was a lot of common interest and thinking between us and Lewis and Slater.

I can't remember when we got interested in genetics but it was early – in the 50s. I remember very early giving a talk before a medical school audience about the importance of heredity in psychiatric illness. After it a Professor of Psychology came up and said "Sam, why are you so interested in genetics"? He was a little behind. He hadn't heard about the Watson/Crick findings and didn't see their full implications. I remember taking about 15 minutes to explain how exciting and important we thought that work was and how we thought that modern genetics was ultimately going to play a very important role in psychiatry. Furthermore we had already begun studies to test familiality in different psychiatric illnesses. We, therefore, became very interested in Slater's work. We didn't have a lot of extra funds but whenever we could we would have people from the Maudsley and from Edinburgh, after Kendall went there, visit our department and all of us would look for every opportunity to visit British colleagues.

We also had links with Stengel who was in Sheffield and was interested in suicide. Eli got interested in suicide early on and because of this we all became interested in suicide thus leading to our becoming familiar with Stengel's work. We read all of his papers and discussed them and wrote letters to him asking him about further details. We were a bit uncertain about Stengel because people told us he had a strong psychoanalytic streak and we couldn't understand, if this was the case, how he kept it from distorting his work on suicide, which we thought was very good.

We then went from affiliations of every kind we could generate with UK psychiatrists to Scandinavians and we began doing collaborative studies, first with the Danes and then the Norwegians and the Swedes.

Can I ask you about the 1969 Williamsburg Conference, which had been set up by the NIMH to get biological research on mood disorders off the ground. The biology of all that wasn't ultimately worth a toss – the MHPG work etc but out of that program came the recognition of a need for what ended up as the Research Diagnostic Criteria the RDC. Gerry Klerman was involved in getting this rolling. When did the links with him begin?
I wasn't actually at Williamsburg. Gerry had been at Yale. We had some informal connections with him and we learned that even though his background would have suggested a different tradition, there were many points of contact where we really communicated. I remember he visited us in St Louis once – this may have been after he went down to NIMH – but I decided to unburden myself about our troubles with NIMH. He was astonished to learn about how one proposal after another had been turned down. I had kept all those files so I said "Gerry, here take this and read it, you'll see". Some weeks later, I got a note from him or a telephone call indicating that it was an eye-opener for him. He said it was going to be different now and it was. And then there was Bob Spitzer from Columbia.

Did Bob Spitzer come in through Klerman or Lee Robins?
It may have been Lee. I do remember he came to St Louis and gave a talk to our department. We had a couple of days to get to know one another and from that point on we worked hand-in-glove. Because Bob shared our philosophy, not 100%, but shared the foundational views. What was then very lucky for us was that he was appointed Chair of the APA Committee that was commissioned to develop DSM-III. At that point the APA was dominated by the psychoanalysts and the only reason that they let Bob Spitzer do this was that they didn't think diagnosis was important. No-one wanted to give it the time.

They set a budget for the whole thing – about a million dollars. Maybe a third to a half of all the members of the committees had been trained at least partly by us. Why was that? Well as Bob Spitzer defended himself later, it was just that when he looked for people who had some demonstrated interest in diagnosis a very high percentage of them came from St Louis or had been trained in St Louis. When word began to leak out about what was going on, the psychoanalysts wanted to stop the process and start all over but by that time most of the money had been spent. Then when DSM-III turned out to be such a business success, as is always the case in our country, it turned out to be the deciding factor.

One of the reasons that's offered for Spitzer's involvement in DSM-III was his role in the debate on homosexuality. Now you guys had also worked on that – the Saglir and Robins monograph. That seems to have been a key event because when homosexuality got dropped, it did give you the

ground to say that if we've been so wrong on this, don't we need to go back and look at the rest?
Marcel Saglir came to us from the Lebanon for psychiatric training. He was and remains a very able individual. He approached Eli about guiding him and Eli agreed to do it and then he did the studies that ended up as monographs on homosexuality in men and women. Until that time, most of us really hadn't given much thought to the issue. I don't think we had even considered the issue of whether there is a medical model way of thinking about homosexuality. But I remember we were all very skeptical about the Freudian idea about it. Later on Spitzer got involved in the issue.

Where did the term neo-Kraepelinism actually come from? Was it Gerry?
I think Gerry probably. We would never have used the term ourselves. We didn't like it for a long time. I think we were afraid it would seem too old-fashioned an idea, even though we insisted that all our residents read Kraepelin's monograph and emphasized his work with the medical students. But we were worried that the label didn't point in the right direction.

But it worked
Yes I think it did. Kraepelin was a very interesting fellow. Over the years I've read whatever of his has been translated into English. And I think we shared basic ideas.

Well mentioning Kraepelin brings in the notion of following people up, which you said you'd become convinced from early on was important. Now was that from Lee's work or were there other inputs?
Going back to Paul Dudley White and Mandell Cohen, they did follow up studies with people with anxiety disorders. It seemed to be something that we all simultaneously understood. A strategy of doing family studies, epidemiologic studies and follow-up studies in parallel was a way to get clues about etiology, prevalence and incidence, and it helped validate diagnosis. It was just a basic part of the medical model.

As I understand it when it came to follow-up work and your work on sociopathy, that you followed people up to the point that you tracked down people who were on the FBI's most wanted list, even when the FBI hadn't been able to get them.
Yes we found a few. That was just kind of a cute thing. There were two big studies that I was personally involved in and directed. One was about psychiatric aspects of criminality and the other was what we called The Clinic 500 study. I wanted to do both but this was at a time when we were still finding grants hard to get for clinical work. So I approached the residents and outlined that there were two projects that I wanted to do. One was to look at psychiatric aspects of criminality. This came about because the man who was the local officer of the Missouri Office of Probation and Parole came to see me. He said that his officers frequently had questions about their clients and would I, as Director of

the Medical Center Psychiatry Clinic, be willing to let them refer those individuals for consultation. I said, "sure but let me tell you something, we really don't know very much about this kind of thing. We will do the best we can but I think it would be wonderful if we could do a systematic study". Now this man who didn't know anything about science, an interesting man, immediately saw the value of this and said that they would do it. So I presented to our residents the two possibilities – a systematic study of an unselected group of convicted felons and the other was to start from scratch and do a study of a random sample of the patients we saw in the psychiatry clinic. The intention in both cases was to carry out cross-sectional, follow-up, and family studies. The residents were unanimous – they wanted to study the criminals first. I thought this was a mistake but since I'd offered them the choice we went ahead.

Many of our residents had their first contact with research on one or other of those studies, which between them took 15-20 years of my life. In the criminals we first studied the males and then females followed by studies of their spouses and first degree-relatives. We later did the same thing with the psychiatry clinic sample. We discovered that only a very small percentage of the criminals and their families had any psychotic disorder other than linked to substance abuse. We learned that there were several ways in which individual criminals could be diverted from the criminal justice system and most of our schizophrenics or bipolars ended up in this group which was only about 1 – 1.5% of the total sample. That's what got me interested in alcoholism because I didn't know that there was so much alcoholism in that population. When we did the women 8 to 10 years later substance abuse, including alcoholism had become widespread and we could see it in our data.

In the mid-60s, you guys were using lithium when very few other people were doing so, how did this come about?

Early on we agreed that George would be responsible for the inpatient service, I would do the outpatient and the consultation services and Eli would take responsibility for medical student education. George began to get interested in the medical approach to in-patients. Now I don't know when that report from Australia came through but when it did, George was never one to hold back, so we got into it very early.

What about hysteria then and Briquet's syndrome?

Well Mandell Cohen, Purtell and Eli Robins had done a study at the Mass General, which we became very familiar with on the consultation service. We'd frequently get a note from neurology or medicine saying "diagnosis hysteria, please see". Well after we got into it, it was clear that the diagnosis of hysteria as used by our colleagues in medicine and neurology didn't mean a thing. So we went back and re-read the Robins publications and we decided to start studying these patients in a systematic way. That's when I really had to focus on operational criteria much more. We began with the criteria they had used in Boston and enlarged the range of issues. To this day, I think we were on the right track but there were a lot of other confusing things going on and then the whole area got muddied by “borderline personality”. Carol North is now the consultant to the general hospital and she’s continuing studies with hysteria and borderline personality and I think with time we’ll understand it much better. Basically I think it’s a disorder of personality and temperament.

Well you’ve raised an interesting point here. You guys were the neo-Kraepelinians, which was a very categorical approach to nervous disorders. But while you only had 10-12 clearcut illnesses, the neo-Kraepelinian approach as embodied in DSM-IV has hundreds and the ultimate out-on-the-streets version of this is shadow syndromes, the idea that there are a host of subclinical neuropsychiatric disorders, the adult ADHDs and things like that. The other way to conceptualise the problems of community nervousness is in dimensional terms and especially as disorders of personality and temperament. Now it seems to me somewhat ironical that the best known personality theory at the moment also comes out of St Louis and from one of your own proteges, Bob Cloninger. How does all this look now to you?

In June of 1975, there was a meeting in St Louis of the soon to be released DSM-III. I remember getting up and proposing that the group recommend very strongly to the APA that we thought it would be helpful if the APA could agree on certain criteria that must be met before a condition could be included in DSM-III. I also suggested that perhaps we should urge that until there had been at least two long-term follow-up studies from different institutions with similar results, we shouldn’t give the entity a status in DSM-III. The alternative was to have a lot of undiagnosed cases. We could have a way of sub-categorising undiagnosed patients in which the label would indicate what the diagnostic problem was. That would put us on a stronger scientific basis and it would constantly remind psychiatrists of our ignorance and what kinds of questions needed to be studied.

I couldn’t get that group to vote in favor of my suggestions. The answer that I was given was that they said we have enough trouble getting the legitimacy of psychiatric problems accepted by our colleagues, insurance companies, and other agencies. “If we do what you’re proposing, which makes sense to us scientifically, we think that not only will we weaken what we are trying to do but we will give the insurance companies an excuse not to pay us”. At first, I thought this was just a rationalisation but they really meant it. I bring the story up because you have to recognise that any classification system is always of the moment. If you look at the rest of medicine, diagnostic systems are always been sharpened, certain things are being lumped together, certain things are being pulled apart and I think the same thing is true in psychiatry. I have always taken the position that when we understand everything about them, some psychiatric disorders will end up as distinct qualitatively different conditions and others will be on some sort of continuum. That doesn’t trouble me. We have exactly that in the rest of medicine. One of the advantages in my background in internal medicine is that I can always bring up examples like high blood pressure that illustrate the point beautifully. I've never seen that as threatening and I've always been a very strong supporter of Bob Cloninger and his work.

But interestingly his view is almost an opposite to the DSM-IV view in a sense
Well I think what he would say is that it enriches the DSM-IV view and it points up certain places where we might want to pursue both paths simultaneously. Bob Cloninger was a medical student with us and somehow or other I gave a lecture to medical students and he came up afterwards and said he thought it was very interesting and he would like to work with me the coming summer. He then spent 3 summers working with me and then later on he began to get interested in personality and temperament. I can't remember what triggered his particular interest in that. He had done a lot of work with people in Sweden and we had become interested in studying alcoholism.

The operational criteria were lampooned as I understand it on the East Coast with references to The Chinese Menu approach to psychiatry
Actually that was Gerry Klerman who used that terminology. He was sympathetic but that was his terminology and I think there's something to it. It really did resemble a Chinese Menu approach to things but coming from Gerry, we didn't take offence at it.

The criteria that ultimately came out first are referenced as the Feighner criteria
Well John Feighner was one of our residents. He was chief resident when we decided it was time to prepare a paper for either the American Journal of Psychiatry or the Archives, setting forth the criteria we were using for our research. John agreed to write the first draft of the paper and get all the references. We had a policy that whoever did the first draft, whether a first year medical student, resident, or faculty member, would be the first author. That paper turned out to be cited among the top handful of publications for a number of years and all it was was a description of the criteria we were using in our studies at the time. But John Feighner did a fine job preparing the manuscripts.

Yes but at the point it came out, there was nothing else like it had ever been published, at least not in psychiatry. It has to have been greeted with a certain amount of incredulity – this isn't a psychiatric paper!
The people who had any kind of sympathy for the importance of diagnosis welcomed it. We got lots of very thoughtful letters – some critical but in a constructive way. I think it was incomprehensible to the people who didn't think diagnosis was important. I remember once at an APA meeting, Karl Menninger gave a talk. He didn't single us out but he talked about his dissatisfaction and unhappiness with the way psychiatry was moving toward these diagnoses. I remember him saying that what we really want is a description paragraph in which you note the essential features of that case. I raised my hand – I was only a junior assistant professor at the time and I said "Dr Menninger, you know those

of us who are interested in the importance of diagnosis want a label that could substitute for just that paragraph. What we want that paragraph to include are the key items that research will have shown us are important for classifying that person".

This wasn't covert hostile competition, was it?
Well maybe it was but I was trying to build a bridge with him because I thought that he doesn't understand that that's what we are trying to do. I don't know whether he got it or not. He smiled and took another question.

After the 72 article, the 75 book Psychiatric Diagnosis seems to have been the next key step, how did that come about?
Bob Woodruff was the first author of that. He was a young member of our faculty, who had come from Harvard Medical School, joined our residency and then the faculty. Actually he was the one who kept bugging me that it was time to have a textbook. "Why don't you have a textbook" and I would say – "in time". Well, he said, "if I'm willing to do the work, would you work with me". I said yes. So he also got Don Goodwin because Don was a very skillful writer; he had been a newspaper writer before coming into medicine. So he started it off. Unfortunately between the first and second editions, Bob Woodruff committed suicide. I didn't know it at the time but he was suffering from a fairly severe depressive illness. The first 2 or 3 editions sold like hotcakes. By the time we got to the 4th and 5th editions, I felt that what we had published the book to do had already been done. Our Editor at Oxford University Press doesn't agree and he has made some suggestions for future editions.

Well you can see by a review of the 5th edition in the American Journal (1988) by James Eaton that not everybody's completely on board yet.
That's probably a good idea because no matter how good you think you have it, progress in medicine comes about by critique and new work – new ideas. We have a new text book, "The Washington University Adult Psychiatry", which I think is a marked advance over the 5th edition because in it we recognise what we were reluctant to do before which was to face up to the fact that DSMIII, IIIR & IV are now out there. We don't try to cover every diagnosis but we do more than we did before and we have had a lot of people writing complementary things but much to my disappointment the sales have not been spectacular. We talked to someone in Mosby who said that they are working on a plan to change this.

Now can I raise the issue of therapeutic skepticism. Places like St Louis and The Maudsley that have been keen on diagnosis sometimes argue that we have to get the diagnosis right in order to give the right treatment but both you and The Maudsley then turn out to be therapeutic skeptics to some extent. Neither of your institutions are associated with heavy backing for particular treatment lines.
We used to be more skeptical but we aren't so much now. You know I used to say something that would puzzle people, which is that "basically, you know, I

consider myself a psychotherapist". The reason is that I felt that the most important thing in psychiatry and in medicine is that one needs a physician who is going to understand the impact of the condition on the person's life and relationships etc. That is what psychiatrists need to be able to do. They can always prescribe medicines but there's always got to be a healthy skepticism about medicines until lots of patients have been studied and followed up. Even in our own department, the more senior people still tend to be more skeptical about available medications even though all of us prescribe them very widely. I tell the residents that they help some people – that it's still mysterious that they help one person and not the next. Managed care is working devastatingly against us in these areas. Managed care is unwilling to pay psychiatrists for talking to patients.

As I understand it Eli Robins would say that the barbiturates worked as well as the antidepressants for depression and George Winokur would say that ECT was the only thing that reduced suicide rates.
That was in the 1950s and 1960s. Later when Eli got sick he did very little clinical work. But if you follow George Winokur when he moved to Iowa in the 1970s, you learn that his department did a lot of modern psychopharmacology. So I think there was a time when there was justification for saying we were therapeutic nihilists but I don't think that's true for the past twenty years. As reports have come out about pharmaceutical efficacy, we have used them.

The process that you kicked off that led to DSMIII and IV has been good for the APA but it's also been good for the pharmaceutical industry. It's given them a series of targets to aim magic bullets at. This drug for that category.
Yes the APA got their building in Washington out of DSM III. As regards the use of DSM III by the industry, I'm not happy with any of that.

Can I introduce the 1970 paper citing a 15% lifetime risk of suicide in affective disorders? It's of the most cited figures in the psychiatric literature. Retrospectively it looks as though it may have applied to hospitalised depressives but does not apply to primary care depression. My point however is that the finding is used by a company like Lilly, who for instance will say that look we have to pick up and treat all these people who are depressed because of their high suicide risk. So there's an issue about how data get picked up and used by the industry.
Yes I agree with that. We didn't actually appreciate the selective nature of the samples we were dealing with. I'd love to get a good population study to look at that now.

The pharmaceutical industry has discovered psychiatric illness in a big way. I'm shocked at how much of an annual APA meeting is now supported by the pharmaceutical companies. Without knowing the precise figures, I am not happy with it. Their motive is to exploit every possible indication for which their preparation has efficacy – now all that means is that patients seem to be improved or whatever 6 weeks after they start the drug. I think NIMH is now going to get in on the act. Steve Hyman is keen to improve the clinical sophistication of studies. The studies that are used to persuade the FDA that there is efficacy require such highly selected samples that most of the people we treat clinically would be excluded. I also recognise that there are many people who have been taught diagnosis as a cookbook procedure and that makes many of us unhappy. I even have misgivings about the large epidemiological studies where lay interviewers go out and interview subjects. You take someone who may have a masters degree in psychology, sociology or even something completely unrelated, and they're taught how to do one of these interviews and they go out and not only are they not encouraged to do a proper clinical interview, they're discouraged because the sponsors want the questions asked absolutely consistently. For big epidemiological studies I don't see any alternative.

But is there another irony here in that you guys more or less developed standardised diagnostic interviewing.
Yes but what we told people was that this is only a guide to the interview. You interview the people the way you usually do but before you're finished make sure all the questions have been covered. So it's a way of keeping the score but it's not a formula for conducting the interview.

How did the Diagnostic Interview Schedule come about?
Well it came about primarily because it was going to be used as an epidemiological tool for people like Lee Robins and Linda Catter. The DIS permitted large-scale epidemiological studies using lay interviewers. I understand the need for such large-scale studies but it's not the same as conducting a clinical interview with a patient.

By these means though you then generate the figures and justification for the pharmaceutical industry.
Sure, but the same thing is true in the rest of medicine. Why do we have a dozen different remedies for high blood pressure or arthritis? The drug companies have learned that they can make money on the basis of whatever distinction they can claim goes with their product. That's the capitalist system. I support the system but I think it has some serious costs to it. I don't know that they are worse than the costs that go with a socialist system. It's an imperfect world!

Our system produces well-trained and thoughtful clinicians in many disciplines. It used to be that in Britain you had many fewer places where people were trained and so there was greater homogeneity. Today, at least in the USA, we have so many places where residents are trained that many have very different training experiences. The variability across the country is very large, which opens up opportunities for the pharmaceutical industry.

On the other hand I think modern pharmacology represents a big step forward. I give Roy Vagelos, who used to be a colleague at Washington University, great credit for this. He was one of the pioneers in the industry who said we must develop drugs in a totally different way – from a rational understanding of the mechanism of the pathology matched to the mechanism of the drug action. We do have more effective drugs but modern marketing is what sells the various pharmaceuticals. I don't know what to say beyond being skeptical.

There's too much money going the other way
Absolutely. I have never participated in any study supported by the industry. Others in the Department have and they have done so in a way that has been perfectly acceptable.

We have talked about the importance of marketing but you've been a successful marketer yourself. Titles like "Biological Psychiatry – Is There Any Other Kind" or "What Makes Psychiatry a Branch of Medicine" catch the imagination very effectively and define positions in an almost slogan-like way that any copywriter would be proud of.
A defender of marketing would say that's just a way of educating people. It wouldn't be easy to come up with a crisp distinction between marketing and education. So I do think in that sense you're right. Some of the titles like "Biological Psychiatry: Is There Any Other Kind?" were just to be provocative. And the responses I've had from that have justified the time I spent thinking it out. There always were people who felt that biological factors were important but I was trying to get their attention by pointing out that – biology is the fundamental science for all living things, including humans. Its possible to study the biological basis of creativity and certainly personality and temperament and I think the time is fast approaching when we are going to be thinking about humans not just sociologically but biologically in the broadest sense.

Do you recognise a Washington University School formed from those who trained with you?
By and large those who came through the department have all continued to carry forward the basic concepts we had and have tried to extend them in their own ways and make them more useful. I like to think that its not too difficult to tell when you're dealing with someone who spent some time with us.

Sure but what the outsiders would still say is that there is still something of a draw up the wagons mentality in former St Louis trainees – still a feeling of its us or them
Maybe. I think it's less defensive now and more positive. There is a feeling now that certain things can be discussed and everybody understands what we are talking about. But it's been very gratifying. I've been at this now for 40+ years. From my point of view we have succeeded to a degree that I never expected in my lifetime and it came more quickly than I expected. I thought we would be

fighting even today battles that we stopped fighting many years ago. Once DSM III got through the battle was won to some significant extent.

You know our department was the first to establish an electron microscopy lab and that was after John Olney had learned about electron microscopy working with one of the scientists in ophthalmology. So I went to a wealthy patient of mine and said "I need a gift and you're one of the few people I know who could afford it". I told him why. I said we're talking about something that may not pay off for 25 years. He gave us $150,000 and we bought the electron microscope. Now I think it would have been very hard for many other department chairs to be thinking that way. I also did the first experiments using Positron Emission Tomography in psychiatric patients. Michael Ter-Pogossian, who was a radiophysicist, was telling me at lunch one day about this technology. I immediately said "that's a wonderful technique for studying psychiatric patients" and we started using it. We saw the potential early on, not because we have been so smart but because we were receptive to the idea that we should keep up with the rest of medicine, applying ideas and techniques to psychiatry.

The other thing you've done is to get heavily involved in the Research Center idea. You've gone out on a limb to get people and projects. Was that a deliberate policy?

It was but there I have to give credit to the various Deans involved and the Institution. I knew that if I wanted the Deans and fellow department heads to be responsive to us, they had to feel that we were committed to the research emphasis of the school. This led to our having laboratory space and support. This also made psychiatry a respected part of medicine.

From nowhere in the 1960s with only residents to do the research, there were points during the 1980s when you were close to being a mini-NIMH in terms of funded projects and senior scientists.

I don't have the figures to hand but there was a point where as a share of the total operating budget psychiatry had the highest amount in the medical school. That was because we didn't do too much private clinical practice. In psychiatry, certainly since managed care, clinical practice does not generate financial surpluses for academic purposes. It was never a big possibility. Now its zero. We always told our people that if they could get a grant, everything would be better.

I interviewed Leon Eisenberg yesterday, who spent sometime saying Biology counts and I interview you today and you say you are a psychotherapist. So this is something of a turn up. He seems to end up in very much the same position as you in the sense of What's the importance of the illness in this person's life.

Well I think those of us of our generation didn't have very much in the way of pharmacologic interventions, and so what we were offering was support, encouragement and guidance. I'm old-fashioned enough to think that's still an important part of medicine.

Is it being lost under Managed Care?
No question about it – because of the question of time. Time becomes money. I have a patient in the hospital right now. He's a lawyer in his 80s, who has got Parkinson's Disease, a severe spine problem, and severe pains. He has a neurologist, an internist, a psychiatrist, as well as other consultants. He saw me initially a few years ago when he had a spell of depression. It didn't respond to medication but it responded to time and support. The family turns to me now for general discussion and information. They don't call the neurologist or internist, except for very specific questions. Everything else to do with his life, his feelings, what the future holds, they want to talk to me about. I think that's a very important role. I can be independent of Managed Care, most colleagues can't. I think the calibre of medical practice may very well be falling in the United States today as a result.

Will the analysts not ultimately blame you the Kraepelinians for bringing this about? They would say they were in the business of getting these things to make sense to the patient's life?
They might but I don't think it would be fair. I've always said to my analyst friends "Look I've no quarrel with anything you do with patients except if you fool yourself and fool them about what you think is the Etiology of their illnesses". That's the only place I had trouble with them. If they would stop talking about Etiology, they would find they had very little trouble with me.

That answer prompts this question. With the eclipse of analysts and the rise of DSM-III/IV which is so obvious here at an APA meeting, while we think of analysis being gone in one sense, the other really big thing here – between workshops, books etc - is Recovered Memories, which in essence is a major claim about Etiology.
I agree but my view of Recovered Memories is that most of this is iatrogenic. I really think that people in our field and in adjacent fields like psychology and social work don't understand how hard it is to establish Etiology. Their range of ideas as to what might be relevant is too narrow.

The thing that seems to fit your career very well, it seems to me is the Missouri State Motto "Show Me".

References

Guze SB (1967). "The diagnosis of hysteria - what are we trying to do?" American Journal Psychiatry 124, 491-498.

Robins E & Guze SB (1970). Establishment of diagnostic validity in psychiatric illness: its application to schizophrenia. American Journal Psychiatry 126, 983-987.

Guze SB (1970). The need for toughmindedness in psychiatric thinking”. Southern Medical Journal 63, 662-671.

Feighner J, Robins E, Guze SB, Woodruffe R, Winokur G, Munoz R (1972). Diagnostic criteria for use in psychiatric research. Archives of General Psychiatry 26, 57-63.

Woodruffe R, Goodwin D, Guze SB (1975). Psychiatric Diagnosis. Oxford University Press.

Guze SB (1992). What Makes Psychiatry a Branch of Medicine. Oxford University Press.

Guze SB (1989). Biological Psychiatry, is there any other kind? Psychological Medicine 19, 315-323.