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Let me try to give you a picture of what child psychiatry was like in the 50s when I entered it. The notion of evidence based psychiatry, which is in the air now - there wasn't even a ground for talking about that. Experienced people had seen many cases and they relied on what they remembered of their cases and they relied, unfortunately, on those of their cases that chose to stick with them to get an idea of longitudinal course. Follow-up studies were almost unknown. People were more stationary in America in academic psychiatry then, than they are now. So in the course of a life in Baltimore, even in my own stay of some 17 years, I would see a mother on the street with a child I had once seen and I would get informal follow ups that way. But it's obvious that the ones who thought I was an SOB and hadn't helped at all wouldn't walk up to me and say "Oh Dr Eisenberg....". Furthermore, I realised that I couldn't trust the follow ups because I was often complimented on a piece of advice I had given that I was sure was utterly incompatible with my own philosophy - but that's how it had been reconstructed by the parents.

There were simply no drugs that worked except strong sedation and anticonvulsants, when they were appropriate. Electric shock was used on children by Lorretta Bender at Bellevue - most of the rest of us were horrified by the idea, so that wasn't an available therapy. Systematic behaviour therapies were just in the process of being developed and weren't available. One did one or another kind of psychotherapy. Either more or less commonsense psychotherapy - practical down to earth as Leo Kanner, my teacher, would have done or very analytically informed psychotherapy. That was the field.

I recently had an occasion to think through some of this. The first published randomised control trial was the Medical Council Randomised Control Trial of Streptomycin in tuberculosis, which was published in 47. Now I started only a few years later. I think it's to the credit of psychiatry that when the new psychotropic drugs were discovered, by accident, randomised trials were introduced at once into psychiatry. They'd already appeared in antibiotic therapy. There was one drug that had been used for quite some time with reported clinical success and that was Dexedrine. Dexedrine had been used by a paediatrician, named Bradley, who was in Oregon. Somebody discovered ephedrine in the Chinese pharmacopoeia, as a stimulant, and then Dexedrine came out shortly thereafter. I can't quite tell you its pharmacology. Bradley applied it across the board to a set of disturbed children and made the keen clinical observation that it slowed the children down, although it was being recommended as a stimulant in adults. It was to be another 25 years before Judy Rapoport had the courage to give Dexedrine to normal children. She used a couple of her children and...

About 1962 ?

No later. Judy didn't do that until several years later. When I was writing about Dexedrine I thought it was a paradoxical effect in hyperkinetic children. So that's all I knew. It never occurred to me to be courageous enough to try it with normal children. Judy gathered a dozen youngsters I guess from NIMH staffers - if you do it on your kids you can hardly be accused of being unethical. Anyway they slowed down and so then we realised it was a

paediatric effect not a disease age effect. In any event so Bradley had discovered this.

He has this marvellous phrase in the article from 37 which says that the results were so good that one would have thought that the other members of staff would have been extremely hostile to the therapy for fear that they would lose their jobs except for the fact that the effects on the kids were so wonderful.

Its an interesting phrase but I would say I think Bradley is under estimating something else. An overactive difficult child is such a pain in the ass that, even if you make your living taking care of them, seeing them calm down so that they're more practicable must have been a big offset against the loss of business part of things. Well anyway Loretta Bender was using Dexedrine. Loretta Bender and I participated in an early psychopharmacology conference just as the first studies were being done. It was sponsored by the psychopharmacology branch of the NIMH and there's a book edited by Seymour Fisher (REF to this?). In any event Loretta Bender was a clinician of the old school, who saw no need to do randomised trials she could tell the difference between a drug that worked and a drug that didn't. She also said something else that I don't know that anybody has ever looked at. Working at Bellevue, she had experience long before most other people in the use of stimulants in children with sexual disorders – children who had been abused at home and then went on to grope, attack, feel up teachers and so on, and she claimed that Dexedrine would also lower that behavior. But that was on the basis of her experience, I don't know that anybody has ever looked at it in a systematic trial.

In any event, that was the state of the field. The field was completely dominated by psychoanalysis. Not that everybody was a psychoanalyst but it was the only sort of comprehensive interesting intriguing psychological theory and all we had was psychology - I mean in one sense or another. The notion that there were children with what we now call ADHD was reluctantly acknowledged by other people. Indeed one of the interesting aspects of that, which has I guess receded into the background, was the big US/UK difference. I remember going to a World Health Organisation conference on the Classification of Childhood Disorders. One of the first things the Division of Mental Health had done when it got a research budget was to set up a series of 10 meetings on the Classification of Psychoses, which were held yearly in different countries. And this was a meeting held in France. Mike Rutter was there, Michael Shepherd who was part of the Central Committee and there were a number of French Psychiatrists. I guess Michael who respected me, as much as I admired him decided that if I said it was there, it was, and they included it. But the British found much less of it than we did in the US. I think part of that was because the British paid much more attention to empirical data than Americans did and tended to call simple behavior disorder the kind of things that we would call hyperkinetic when a kid also was getting himself into anti-social acts of one sort or another.

Can I just hop at this point and put it to you that another way to read this might have been in terms of introversion and extraversion, ideas which Eysenck had put forward for adults in the late 50s and early 60s. You

could see hyperactive kids as falling on the extraverted end of the spectrum – could the Brits have been seeing things in this way?

It's an interesting and worthwhile question, which I haven't a clue how to answer, because Eysenck made no impression on our side of the water. So that was not a topic of discussion - whether it was in the UK at that time I don't know. I knew that he existed, I knew about introversion and extraversion but it had always seemed to me like a simplistic way to look at things. But I didn't say no that's not it or say no it's not it because here's what he says and here's what we find and they do or they don't match. No idea. That was completely out of it. Certainly, we left it open as to what extent hyperkinesis was a temperamental disorder - an inborn disorder of some kind versus a parentogenic disorder.

That idea was still there.

Oh yes. Oh absolutely. I mean the schizophrenogenic mother was rampant in adult psychiatry. Drop back a minute. Leo Kanner had described early infantile autism in 1943. It was the first description of a disorder, that he called - when he first wrote it up - inborn errors of affective contact. Lots of people were furious with him because everything was thought to be psychogenic. Our view of hyperkinesis was influenced by the fact that you saw overactivity in brain damaged children. I was not ready to decide that brain damage could be necessarily inferred from the existence of hyperkinesis but there was a feeling that there was something different physiologically about those children. I believed, because my boss believed it and because I'd never seen anything to contradict it, that you outgrew it with age. It was really quite uniformly believed that it was gone by adolescence. I remember reassuring parents that you just had to have the stick-to-it-tiveness to survive for a while. This was before the drugs were available - because they'll outgrow it, or some phrase like that. And it wasn't until people like Gaby Weiss and her colleagues working in Montreal began to do the long-term follow-ups that we saw that there were some problems in some of these children later on.

Where did the phrase minimal brain damage come from – it was a big phrase in the early 60s - MBD.

Well let's drop back historically, I can actually probably give you a reference or two that I wrote at the time that would maybe give more accurately than I can remember the historical roots. The notion that brain damage was associated with specific behavioral disorders began with a description of the post encephalitic behavioural disorders that occurred in the early 1920s after a big epidemic of encephalitis. There are people who described grossly disturbed overactive difficult kids. They were obviously much more severely affected than the people we're taking about. Then a chap named Strauss wrote a book that may have had the title Minimal Brain Damage and MBD became a source of controversy in the field. The psychogenically oriented psychiatrists didn't want to acknowledge any of this stuff. People like me thought absolutely there had to be something. I wrote a couple of pieces on Minimal Brain Damage but I felt it was limited and would not have applied it widely. Then the fight became as to whether all of these hyperkinetic children really didn't have minimal brain damage and whether all dyslexic children didn't have minimal brain damage. It became a fight between the organicists and the psychogenicists of the time.

These are amazing fights to think about retrospectively. They have reappeared now in the genes are our destiny arguments. I mean Freud said biology was destiny. Now we know its genes that are destiny versus the rest of us who say genes determine the probabilities and then the environment selects among them. So here we had a field, that was predominantly in the US psychogenically oriented. It was influenced by psychoanalysis, even though there never were that many psychoanalysts and child psychoanalysts in particular were a minority in the profession - 10/20% not more - but there was no other really competing idea. So that people like myself, who could not subscribe for a whole set of reasons to psychoanalysis, none the less found themselves using concepts that were borrowed from it. And, therefore, the use of drugs to treat psychogenic disorders became somewhat controversial.

But the discovery of drugs, absolutely, were sensational experiences for the people who lived through it. I remember one case I had when I was working for Leo Kanner for the Paediatric Department of the John's Hopkins Medical School. I got a 9 year old black girl, who in the first year of the integration of schools in Baltimore, went to school apprehensively because she thought the Whites would be looking at Blacks as stupid and she'd better know the answers in class. When the teacher would call on her, she felt paralysed and then she thought X-rays were influencing her mind and her parents weren't really her parents. She developed what I then called schizophrenia. I have a 40 year follow-up - she was a schizoaffective disorder which I didn't recognise at the time. But she came in the hospital absolutely delusional. I was smoking a pipe and she wondered whether that was an instrument to put into her vagina and so on. Reserpine had just come out. I gave her Reserpine and within 24 hours it was gone. I'd never seen that type of thing in my experience before - so it was really quite sensational. I continued to follow her as an out-patient. I tried some psychotherapy but the family wasn't very interested in psychotherapy.

So we started then to try drugs on children. We were "astute" enough to realise that we ought to look at some differentiation of disorder. There was no good classification scheme. We had what we called anxious or neurotic children verses overactive children - that was the dichotomy we used. Using this we tried out a series of minor tranquillisers. In the first studies we did, we employed meprobamate and perphenazine and we had a placebo. We did it very carefully and we found that the placebo was by far the best drug of the three.

For all the kids

Yes. In kids the placebo effect was the only thing you could count on. The other crap didn't work at all and the side-effects were somewhat higher from the drugs than from the placebos though not very much higher. We did two or three studies with great disappointment because we really were not anti-drug - we wanted to find something that could reliably work. Then we decided to do an experiment with Dexedrine because there was every reason to think on clinical grounds if that didn't show up we should get out of the business altogether.

The first study we did was in the training school for delinquents. It was a convenience sample, that is here we had a study you could begin on a given date because all of the patients were in there; it would end on a certain date and you could within a reasonable period of time find something. By and large, while there could be elopements, most of the kids were there from the beginning to the end of the study. We found there was no question about the effects with dexedrine. Methylphenidate had just about come out at that time and we did some out-patient studies with it and in every case the active stimulant was more powerful than the placebo in its effects both on the reports of parents, the reports of teachers and on the kind of tests that Keith Connors, the Psychologist who was working with me, devised. He used continuous performance tests of attention and so on.

Why did you pick methylphenidate because it was being used in adult psychiatry at the time with no great linkage to hyperactivity. As late as 1972 in the Archives you can see advertisements for it being used for people who were depressed. So it isn't as though it was the hyperactive drug that we now think it as being.

But it's also in the same pharmacologic classification analogous to dextroamphetamine. It was in the same drug category. I think by that time stimulants already had a somewhat unsavoury reputation among adolescents as a drug of abuse. Indeed the use of both Ritalin and Dexedrine was ruled out of order in Japan. Japan was quite concerned about drug abuse so you couldn't get it to treat children because they just wouldn't allow it to be manufactured. In the methylphenidate paper we say "we undertook the present study of methylphenidate an agent which has been recorded to be effective in clinical trials". The two references are to Mauricio Nobell who in the Diseases of the Nervous System and the Archives of General Psychiatry had reported uncontrolled trials. Damned if I know why he first used it. But I think we saw no problem in using it because we thought of it as a Dexedrine-like drug. I think everybody all along was worried about Dexedrine's effect on appetite, on sleep on possible stunting of growth. This was said to be a milder drug. So that's why it was tried.

We did one other study which I think is one of the more interesting things we ever did. It was published in a reasonable journal but we didn't make of it what we should have. It's a marvellous placebo effects study, whose full value we didn't appreciate. The first study we did in the same place where we did the study of perphenazine versus placebo in delinquent kids. The design of the study, we realised after we'd done it was extraordinarily naive. We had Cottage A in which they got either perphenazine or placebo - that part was standard. We took Cottage B where nobody got anything as a control cottage mostly to look at the ratings of the House Parents, which were the main index we were using. As the study proceeded we became aware of the fact that the ratings in the control cottage were getting higher and higher and it ended with a mass elopement of 13 kids, not everybody but a big chunk of kids ran the hell away from the place. We tried to figure out what happened. In the placebo and the perphenazine groups - in the treatment cottage - which did much better than the one that exploded, there were no drug effects at all. What I only realised later on was we'd done an experiment on the placebo effect of not getting a medicine that's defined as good for you. The kids were in a delinquency place. You got out based on good behaviour. This was a

drug that was going to make you behave well. People in the control cottage felt cheated. The kids felt cheated and the House Parents felt cheated because these kids were no pleasure to take care. The bonus is coming to the other cottage. So there was something about the chemistry of the place we didn't control for it so. What we should have had was a cottage in which everybody got placebo without knowing it if we were interested in the ratings. But this was really very interesting. The status of not getting a drug. Not just because it isn't being given but they're getting it and I'm not getting it. A negative placebo effect if you like. That's when we then did the Dexedrine study and we did a number of others with Methylphenidate.

Can I just hop back on this issue of the no pill control group. It is as you say extraordinarily rare to have done it. The only other person that I know who has actually done that is Louis Lasagna for a sleeping pill somewhere around the early 1960s.

Was it a no pill control group in Lou's case or the people who were not getting the pill saw the pill as something they would rather have had.

I don't think so. I can't remember the study fully but I know it's the only other piece of work that I can think of where there's an actual control group for the placebo.

It may be that he employed it to show that placebo was associated with side-effects. You know one of the interesting things that goes on all the time is that the side-effects you get from anti-depressant drugs, which are effective, often mimic depression itself. So is it the depression, is it the drug. Every placebo study shows that people report side-effects associated with placebo presumably coincidental things. And one way to test the power of placebos to give you side-effects would be to have people all running along and you just measure how frequently rashes and stuffy noses and dry mouth are reported by non-treated people.

It's absolutely fascinating isn't it? In your articles you refer to the work of people like Beecher and Lasagna. How much did placebo become an issue for you because all the way through to your most recent articles you are still saying well you know the placebo effect is important....

We certainly were influenced. I was at Hopkins when Lou Lasagna was at Hopkins. I knew about Lou's work. Lou had done this work earlier with Beecher at the MGH. We found all that quite fascinating. Beecher later became primarily known for the work on ethics and he caused an explosion in the United States with his articles saying that informed consent was often obtained. Lasagna and the people who worked with him were friends of ours and there was lots of discussion. They were very ingenious. Take, for example, a study which I just think is a gem. Meprobamate – Milltown - was a sensation in the United States. The Comedian, Milton Berle who was a big feature on TV in those days took to calling himself Milltown Berle and people would pop pills on television as part of the joke. Well when they wanted to test the effect of meprobamate on anxiety they got as volunteers college students who agreed to ride the Ferris Wheel. They took people who were frightened of the Ferris Wheel and for a monetary consideration agreed to become subjects and then you took your Milltown or a placebo while riding this thing. It would have scared the hell out of me. I was not asked to volunteer so I

didn't have to uncourageously refuse. It was a marvellous idea for generating real anxiety as opposed to lots of the fancy experiments that were done.

Anyway, I think we made one terrible mistake that was fashionable then that I think was a disservice to the field. Like everyone we wanted to do a study that showed a clean result with minimum patient loss. Obviously in a 6 - 12 week study you still lost people but you didn't lose as many as you would in the 52 week study. Furthermore you could get the study done in an incredible piece of time even if you had to keep accumulating out-patients. Finally, the drug was lovely in that the symptomatic change was quite striking. This isn't a vague effect. There were real differences that showed up on the continuous performance test. It showed up on Poteus Mazes. It showed up on things where the kids had to draw a line and not hit a wall. The teachers saw it, the parents saw it. It's a deadbeat effect, it isn't like an antidepressant where you have to wait three weeks. So everything was in favour of saying if you got a symptomatic result you had really taken care of the kid's problem. Because everybody thought, myself included, that the kids problem in failing to learn was an inability to attend and once you had overcome this distractibility the kids would learn - said we on faith. Now nobody would attempt to give reading achievement tests at the beginning and end of the 12 week period. We didn't measure it because it wouldn't have shown up. And if we didn't find it, we wouldn't have thought it meant anything because the test would be insensitive. But we never looked a year later to see whether the kids who were on medication were in fact learning. In fact they don't do well. They don't do well at all because whatever the hell the condition is, it's a complex of more things.

We did try to provide guidance to the parents but we didn't stick with it. And if I were now as a clinician recommending care to parents, sure with a very grossly overactive child I would use drugs but I would insist that the parents come in with the child. I would try to teach the parents some elements of behaviour therapy for management of this. I would want educational specialists to confer with the teachers to help these kids with their learning problem. And I think there are a few studies now that show combined treatment tends to show substantial gains at the end of a two year period. I think now what's happening in the United States is that drugs are being prescribed wholesale. First place, for kids that don't have a reasonable ADHD. They are being prescribed without any kind monitoring at all. School teachers call the paediatrician, the paediatrician finds it the easiest way to deal with it. I think it's dreadful. I think the combination of a drug that produces a symptomatic effect, in an era when cost control is the goal of all medical care, is a terrible explosive mixture. So I think that parts awful, even though I think the drug could be effective for these children as part of a total package of treatment.

Now I'm trying to give you a picture of the times. Somewhere in the early 60s, when we were doing these first few studies, before we got on to the stimulants, I was invited out to give a talk at Washington University in St Louis. Now Washington University is a special place in American Psychiatric history. It really reintroduced research to psychiatry. It made classification a respectable topic. I think classification has moved down a primrose path since, but it wasn't their fault. I felt like a triumphant hero. They said to me a

psychiatrist who can count. It was very crude counting but that's a measure of how things were then. They weren't hailing a biostatistician. I didn't and don't know any biostatistics to speak of. I counted - there was some quantitative data in the study and that was very rare at the time. It's some measure of the distance we've come. Of course the trouble with it is that now everything is quantitative. Diagnosis goes out to four decimal points. It's become reified. And the human interaction part of it is disappearing from the face of psychiatry. That's terrible. But what I'm trying to convey is how different the times were. The people of Washington University were out of step for a long time.

At that point in time though given the work that you did would you have almost been seen as going into the biological psychiatry camp rather than the social psychiatry camp you've ended up in.

Absolutely. I'd written a couple of papers on brain damage in children - more speculative than data based. I treated hyperkinesia as a genuine disorder. I recognised how terrific the work of Stella Chess and Alex Thomas was on temperamental differences. I think I was the first person to give credit to Lee Robins.

Let me just tell you about that. Here we had this complete psychoanalytic intellectual hegemony. It wasn't a dirty trick. It wasn't a conspiracy. They weren't out to screw anybody. They had the truth, they were happy to share it with you and they had a way for you to learn how to be one of them too. A woman, who was a social worker, named Helen Whitmore? enlisted a brilliant psychologist, named Hanns Lucas Teuber, to do a follow-up study on delinquent children who had been put into a special programme of social work therapy. Lucas set it up so there was a comparison group of children. I don't think it was double-blinded but there were kids who got ordinary care and whatever the hell the community did for delinquents. Then you had the kids with once a week contacts with the social workers. At the end of two years he did a systematic study of the court records on recidivism. Now before they revealed the results of the court record studies, they had asked both the social workers and the kids who were the patients of the social workers to say what they thought about therapy. Both the clients and the providers were highly favourable. When they looked at the outcome, there was absolutely no difference. It was a psychological blow. Now the psychoanalysts immediately said "well it was social worker therapy". But still it was upsetting. So that was number one.

A second blow was the notion that Chess and Thomas had introduced that children were different from the beginning. An idea which hardly seems heretical now but the notion of indefinite plasticity was attractive. Why? Because we had just come through the War, we'd just come through defeating Nazi racist geneticism and so here environmentalism was triumphant.

Jerome Kagan hadn't begun to come into this area yet then.

No Jerry was just beginning to do his longitudinal studies. Then a third thing happened - the Lee Robins study. Lee Robins had come to St Louis with her husband and discovered that there was a St Louis Child Guidance Clinic, which had operated from about 1925 - 1929 and then for reasons of economics, to do with the depression, had had to shut down as an agency.

Unlike what usually happens, the records had been put into a storage room in the City Hall or some place. Lee discovered that and decided to follow-up these kids 25 years later. She had two big advantages. One was an intelligence and a tenaciousness that are really unparalleled and secondly that this court associated clinic had the most remarkable set of records. They not only recorded child, mother, father, siblings but uncles, aunts and grandparents. So there were lots of clues in this record to try to track somebody. They got access to drivers licence records. This was before Social Security so I don't think they had Social Security records but they got access to a number of public records you'd have trouble getting into today. And they didn't stop. Lee called the mother to ask her whether she could contact her daughter because they had the mother's address and the mother said you stay away from her she's too busy raising her collies. "Oh", said Lee, so then they called the Dog Fancier Associations and they found out who was raising collies and here was this woman's name and right age. They called her and she said "I'd be delighted to talk to you". They didn't let anything drop. In any event, they found that neurotic children had almost as good a likelihood of a fine outcome 25 years later as kids who were classroom controls. But the delinquent kids and the disturbed kids were in big trouble and they were the very people Child Guidance Clinics were ruling out – we didn't treat delinquents.

So that was another blow to the psychogenic position. And then drugs came along and drugs shook up adult psychiatry in a way that really was quite remarkable. It shook people who should have known better. Because the drugs worked, the assumption was the cause must be organic. Hell that doesn't follow at all. Phenobarbitone stops seizures but not because epileptics suffer from hypo-phenobarbitolemia. Every kind of organic thing was given an impetus. A psychoanalyst, who was a forceful and not too subtle researcher, named Robert Heath working in Tulane in New Orleans did an experiment based on the idea that there must be something in the blood of schizophrenics. He did extract something from the blood and injected it into "prisoner" volunteers. And the prisoner volunteers displayed waxy flexibility and other symptoms of catatonic schizophrenia. Well of course the prisoners knew what the experimenters wanted. And they produced it. Now you would think a psychoanalyst would have a psychologically sophisticated experiment. Furthermore you would think other psychoanalysts, even if he didn't know it, would have raised questions. But when Heath came to Boston, the home of orthodox psychoanalysis, and gave the story, everybody remembered that Freud once said that underneath schizophrenia may be a chemical imbalance. You know they all fell for this crap. Taraxein was some kind of toxic mix that had nothing to do with schizophrenia - it has disappeared except as a historical curiosity of the time.

Now, I think, from a field that was overly psychologically deterministic we've moved dreadfully in the other direction. The perspective of time makes that terribly clear. What was marvellous about the field in the days when psychoanalytic theory was dominant was that psychiatrists thought listening to their patients was important, helping patients to try to make sense of what was happening to them. The better psychoanalysts really were not so different from an ordinarily intelligent person and wouldn't have given you all this fancy stuff. Freud was not a Freudian. His interactions with his patients were much

more active than what he wrote about. I think that was always true of the analysts who mostly were afraid to acknowledge it because they would have been thrown out of court had they said it. But now we've moved.

A number of forces have been operative in the United States. In the first place in the US there are now more psychologists than psychiatrists and more social workers than psychologists. First, psychologists got the right to be paid for psychotherapy under insurance schemes and at least from the beginning charged less than psychiatrists so they became competitors in the market. And then social workers undercut them both. Social Work changed from social work to psychotherapy full-time. It pays better and we don't have to deal with miserable, unhappy, poor people. It's a prestige operation being a therapist. Well, now this is a broad generalisation, but I think that psychiatrists began to embrace drugs and the biological explanations of behaviour in order to emphasise the one difference they could claim from psychologists and social workers. We've never been able to prove that our psychotherapy is any better than theirs but hell we can write prescriptions and so far at least they can't - although the psychologists are fighting for that right in the United States. So that made the new drug era more congenial.

Under managed care, that movement was given further impetus because in the United States, unlike say Australia or a number of other countries, mental health benefits are restricted. A diabetic can get life long care, as he should, but a mental health patient gets only a certain number of visits per year and they're cutting down and restricting those. Some of the insurance schemes, that are now under way under managed care, provide what is known as carve outs. So if you're the General Practitioner and you think the patient needs care you can't send him to the psychiatrist or psychologist or whatever of your choice instead you refer him to a sub-company. A mental health carve out company, which then provides the care, sometimes by a counsellor who has very little in the way of a psychological educational background. Psychiatrists because they're considered high price in this field, although the differences are maybe not that great, are used for diagnosis because that's still legally a responsibility, for prescribing and for monitoring drugs. I think that belittles the field. I would not have become a psychiatrist if that's what I had understood to be a psychiatrist's job.

I find myself looking at the field with mixed feelings. From the research point of view, it's more exciting than it ever was before. We have real data with cognitive behaviour therapy, with interpersonal psychotherapy are effective and as effective as drugs for the treatment of depression. Gerry Hogarty, at Pittsburgh, has lots of data on a cognitive enhancement treatment for chronic schizophrenics. It doesn't cure them but it maintains them better in the community. I find it enormously exciting to see research correlating brain states with mental states - it doesn't tell you which is causing which but it's really quite fascinating. There was a piece in a recent issue of Nature, which was a marvellously ingenious use of modern technology. They used goggles which produce virtual scenes in which the volunteer is shown a piece of city landscape and it moves you along as you're asked to find your way from place A to place B. At the same time they have a PET Scanner on your head looking for which parts of the brain line up. It turns out that, just as in mice, the hippocampus is very active when you are learning spatial localisation and

when you're recalling where it was. Then they asked the subject who has learned this to find a new place in the city and you find the frontal lobes get more active than they would. That's terrific. That's very exciting. I would never have expected from the way I had been trained that obsessive compulsive disorder would be so consistently associated with abnormalities in metabolism in the basal ganglia and other places. I find it even more fascinating, and I use this with students all the time, that when the patient who shows these abnormalities then gets treatment to some extent the abnormalities resolve. And that occurs whether you give them drugs or psychotherapy. Isn't that marvellous is the way I feel in terms of all the things that are going on. Unfortunately, these changes can be interpreted the other way around. That is fundamentally what we're doing is using drugs to change these abnormal circuits and that's why their mental states improve and the effects with psychotherapy are dismissed as a placebo effect or chance or whatever. Its dismissed because it doesn't fit the model of what the hell is going on.

Or as you say the model of what's going to lead to my reimbursement..

Yes although people are not as crude as that. The education of physicians has changed markedly as the result in changes in patterns of delivering care that have resulted from the emphasis on costs. Patients are in hospital much less than they were. For the most part that's been good. Hospital isn't a great place to be, although you can cut short the stay for psychosis to the point where an enormous burden is put back on the family because the patient still has more recovery to do before he becomes possible to live with. But the opportunity I had as a resident to follow somebody over one or two months of a severely psychotic disorder is simply not available to the young people now. I'm not suggesting we keep patients in hospital, who shouldn't be there, to give the medical student a chance to learn. But the fact is its different. The logistics of arranging a programme so that you now see that patient in day care and then you see him in out-patient department is much more difficult. There are many more disconnects. Patients disappear, the students move on to something else. Educating students in out-patients departments is basically much more expensive than it was in hospital. For a patient in hospital in the old days, there was lots of down time and this is whether it was a cardiac or a mental patient. Being examined by a medical student was often in fact constructive. Welcome in an otherwise boring day. In the out-patient department, since someone has to check the examination out, you're slowed down. So the productivity of the clinic, from a standpoint of reimbursement is reduced, and the length of time the patient has to put in the clinic is increased and patients are not so happy to hang around the place. Again this applies to all of medicine, not just psychiatry. No one has figured quite out quite how to do it. If we have to go back more and more to an apprenticeship type of model for clinical training, the problem of monitoring the quality of the education becomes so much worse. Whatever meaning, an activity like a ward round had where you interviewed the patient in front of 12 students or you went from bed to bed or you did an examination while the students watched and then you watched one or two students doing an examination, was visible in public. If you were a fumbler or a fuddy duddy that was apparent to everybody. If it goes on in your office, there's you and a student and a student gets an incompetent that's that. And it will take a while before you get enough feedback from the students to wash this one out. So there

are real problems in the educational system that came about as a result of the change and a very serious problem about the education of psychiatrists. If it is true that in my time a disproportion of the training time for psychiatrists was devoted to psychoanalytic seminars and to long-term treatment, the opposite is true now where its psychopharmacology, diagnosis and much less to human interaction.

Let me bring you back to 62 and just ask you about a few of the other people who began to play a part then and how the ADHD thing began to grow. There was Magda Campbell.

I'd say Magda came onto the scene 5,8 10 years later. Magda inherited the mantle of Loretta Bender at Bellevue Hospital.

You'd better tell me who Loretta Bender was?

Loretta Bender was a Child Psychiatrist, one of the early ones in the United States. Trained, I would guess, in the 1920s and then worked at Bellevue Hospital in the New York State Hospital system. She was one of the few people at all who used Electric Shock. There are interesting conflicts in the literature about this, which I don't know that anybody has ever looked at systematically. Loretta would report very good results in treating what she called schizophrenic children in Bellevue with electric shock. But there was somebody named Clardy, who ran the State Hospital Childrens' Unit at Rockville, which was a backup hospital, and he would report getting Bender's patients in confused and disturbed states after electric shock. So there was this absolute disconnect between her results and others. She trained a number of good people. She trained Stella Chess, she trained Magna Campbell and Al Freedman, who became who did some of the early psychopharmacology work at Bellevue with her. But I don't know actually Loretta's detailed background or training.

The other interesting thing about Loretta Bender was that she was married to Paul Schilder, a distinguished analyst and neurologist. A very interesting man, who was killed as a pedestrian by an automobile, while leaving Bellevue Hospital obstetrical ward after she had delivered I think the second of their two children. I was always convinced Loretta had a reading disability. She wrote abysmally. Trying to decipher some of her papers was very difficult, I think because of a residue of that rather than an attempt to be obscure. So she was one of the leading figures, along with Leo Kanner. Hilda Bruch did the studies on anorexia and obesity. There was a woman analyst in New York ??? who became interested in autistic children and had a following of her own. She was quite different from Kanner.

When did Magda Campbell begin to come in... she became quite a big figure in the pharmacotherapy area.

Magda Campbell has herself a very interesting history. She was a Jewish refugee out of some place like Romania. Her mother got out somehow and then she worked as a maid for some American Ambassador in Vienna who managed to get them a visa to get to the United States. So its a story of coming up under very difficult circumstances. Magna began treating severely disturbed children with antipsychotic drugs, because that's what she saw on the wards of Bellevue. So it was often a somewhat different direction. We never had an in-patient unit for children. That's why I never got into that at all.

Somewhere around the late 1970s, early 80s, what had been MBD and had become ADD, was still relatively rare compared with now. While the pills had been shown to work and you guys over here picked it up in a way that in the UK people weren't happy to, all of a sudden in the early 80s there's this explosion. There seem to be two or three names linked to this Paul Wender, Rachel Klein and one or two others.

You're absolutely right. By that time, I had moved out of Hopkins and Child Psychiatry to the MGH - that move was 67. So I was not as close an observer of the scene by that time. Paul Wender was, and is, an overenthusiast for drugs. Paul was a resident of mine in Hopkins. Paul's important contribution was to put the persistence of attention deficit disorder into adolescence and young adulthood on the map. I think I'm right that he was the first person in the States and may be the first person anywhere to start giving Dexedrine or Ritalin to adults who allegedly have the last remnants of ADHD. Whether that's always true or not is a moot point but he recorded good results. Most of us were treating pre-adolescent children with drugs. I believed it washed out by adolescence. Furthermore everybody knew that adolescents could readily become addicted to the recreational use so we never wanted to consider it. I would think if I'd seen a 15 year old who had a clear history of ADHD, I would have been very fearful of starting the drugs because of the way that everybody had allowed the field to be shaped. So it took a certain amount of courage on Paul's part to start treating adults. He reported good results, as I've said, and a number of people now claim this is a legitimate syndrome and do treat the adults.

Rachel, who probably began in the late 60s early 70s, did systematic placebo control trials. She tried a variety of drugs on more than just ADHD but she's surely one of the key people who put ADHD on the map. She's a very bright and impressive woman. I have been an admirer of hers. So that would be the other name.

On the other hand she gives a very forceful line on the drugs working, the condition exists, what we should be doing is we should pick people up and put them on the drugs. She isn't particularly advocating adding in the extra non-drug elements.

No she isn't. And I don't know that Rachel has ever done long range studies. The other group I should have mentioned was Gaby Weiss and her colleagues in Montreal. They were in this during the 60s and continued to work on it systematically and followed up their children for long periods of time. I don't know what Gaby would say if you asked her about whether the adult survivor would profit from stimulant drug treatment.

Let me ask you about Keith Connors.

Well Keith Connors was and is a very bright psychologist who I'd succeeded in recruiting from Denver to come to work with me. I took Keith with me from Baltimore to Boston. We didn't have a tenure line so Keith had to look for some permanency. Was offered what looked like a splendid opportunity in Pittsburgh. He went to Pittsburgh for several years but found himself unhappy with the relationships with other people there. I can't assess the legitimacy of his complaint but he left and went to George Washington University at

Childrens Hospital, Washington DC. Now he has moved to the South somewhere.

Keith, for a while, worked in an interesting sub-variant of this syndrome. There were a number of paediatric allergists who got involved. There were two schools of thought – hypoglycemia was one argument and the other argument was a sensitivity to diet and additives. This was the early 70s I'd say. By and large, the people who pushed this were clinicians, allergists who thought everything was allergy. They had no scientific sophistication at all and never did a study worthwhile. Keith started to pursue this in a more serious way. Now you know its highly possible that if you have a 100 kids with ADHD, two or three might have some allergy on them. I mean there has got to be some but certainly it's not reliably present. As for hypoglycaemia, this was one of the things you see in both adult and child psychiatry as a generic answer to whatever the hell is wrong. But it never shows up on tests and people have to invoke it - well you didn't find it because you weren't looking at the right time. But again it certainly is not substantially supported as a cause of overactivity.

When all of these things began, it takes some work to dispel them. A number of the people who were allergy-centred reported good results from treatment based on eliminating the offending allergen from the food. One of them was a dye that's widely used in food. So the therapeutic challenge was to modify the whole family's diet in quite an extraordinary way. I would think that might have all kinds of psychological effects on the life a family leads when everybody has to worry about this. So maybe they got some results but none of it ever stood up. Some people who were in the brain damage field began to think it might be the residue of lead poisoning. Well you know there are lots of homes with old lead pipes but none of it in any event was ever solidly shown to be present in the majority of children with this symptom complex.

Before we leave the child treatment area, can I pick up another issue, that is beginning to come on stream more, which is the treatment of children who are depressed with antidepressants. This is interesting in that it's not readily supportable by either clinical trials or symptomatic evidence in a way that methylphenidate for instance had been. This links in with the fact that there didn't really seem to be any children who were depressed before Kim Puig-Antich put the syndrome on the map in the late 80s.

You know that's really a very interesting question which I once posed to Mike Rutter. Mike did the first reliable epidemiological studies in child psychiatry. That was an enormous contribution. In his Isle of Wight study, in which I think every 8-10 year old was actually examined and records obtained, I think he found something like a rate of depressed children in 2 in 1000. Now the claims of the prevalence of depression now are many fold that. I wrote Mike a letter asking him where the hell were we then. Is it possible these people are calling depression something that isn't depression. Mike didn't give me a concise answer. You might try putting it to him. He's extraordinary. The world's best child psychiatrist there's no question about that. On the record, he found 1 or 2 in a 1000 rate and people now find 10 in a 100. What the hell's going on. Is there an epidemic of depression, are we redefining

something? Now I was taught by my mentor Leo Kanner that depression didn't exist in children. I don't remember why he said that but that's

Well in the UK until reasonably recently you would here this and get the view from child psychiatrists, who had a Freudian approach towards things, that children couldn't be depressed because of the absence of a superego.

That wouldn't have been Kanner's explanation. He just hadn't see it. He'd seen unhappy children but it was considered situational unhappiness rather than depression. So you know it's a real puzzle. I mean there's a report from that time by the best set of data we have, using different criteria, and now you have respectable people out there who claim to find a great deal of it. So that's puzzle 1. Puzzle 2 is that the prescribing of tricyclic antidepressants for children is, in this country, out of proportion to what I think the frequency of the disorder is but in addition they're being prescribed in the absence of any controlled study which shows efficacy either with children or adolescents. Now I have no doubt, at least that there's depression in adolescence, and I don't disbelieve that there could be in children, I'd just have to see what the data are. But why should depression in adolescents, for example, not respond to a drug I'm convinced works with adults. I mean it really is quite striking. There's one recent study that I've been told about I haven't seen, with fluoxetine, but its not overwhelming.

You're right, they had to do a washout to take out all the placebo responders and even they were just about able to show a response.

Right. So it's really quite remarkable I don't know how to explain it. There are lots of things we find to be true that we don't have an explanation for but it's really quite remarkable. I wrote an editorial on this, if you want to pursue this topic, which appeared in the Journal of Nervous and Mental Diseases a couple of years ago. I had some sympathy for the clinician - the title was In The Absence Of Evidence What Is A Clinician Supposed To Do? I mean the point is the data doesn't support prescribing. But the parents come to see you with a depressed adolescent and there is also data that depressed adolescents are more likely to commit suicide than others so it's a serious disorder. There's no evidence that psychotherapy works. There may be some now but at one point you didn't have any control trials of either psychotherapy or antidepressants that made a difference. Do you say come back when the biochemist has solved the problem or do you use what you have even though you don't have controlled trials?

Isn't the issue partly that its so hard to show that things work over and above simply seeing the person. So you are doing something by simply seeing them.

That's right. But on the other hand proof that seeing them versus letting them be on a waiting list isn't terribly persuasive either in this condition. So it's a big problem when you're confronted with the patient. My conclusion was that the National Institute of Mental Health had a major responsibility to call together a group of competent people and say look we've got to explore this issue of what are treatments that work for adolescent depression. Now to come back to childhood depression, I am impressed by what I've heard Myrna describe. Myrna got hold of Puig-Antich's sample and she has followed them and they are likely to have had subsequent episodes of depression. So whatever the

hell it was in childhood those kids had, he picked up something which predicts depressive difficulty later.

My colleague Bill Beardsley has worked with depression the other way around. He began studying the children of depressed parents. The depressed parents sample came from a large study that other people were doing which included a control group. Depressed patients were asked to nominate their friends who the investigators then asked to co-operate in the study. So you had more or less normals and the depressed and then they looked at the children. There was much more prevalence of psychopathology in the children but in childhood this was not clearly depression. There was a variety of troubles. Now we know from Myrna's population studies that the offspring of depressed parents when they reach the age of risk are much more likely to show depression. So there's something there that we evidently didn't pay attention to.

Is there a risk though simply saying to these kinds you're depressed and putting them on pills where it isn't clear that the pills resolve the situation that we may make things worse.

Oh no, I understand that but I wasn't arguing that. What I'm arguing for is what Bill Beardsly does. Given the evidence that the children of depressed parents are at risk, and he thinks at risk because they have both more genetic loading and they have unhappy childhoods, with lots of marital difficulties often going along with the depression, he has what he hopes is a prophylactic intervention with the whole family. This involves clueing the kid in that mother or father is depressed and their angry attitudes are not because they don't love you but because they're sick and they're not sick because of you – this is important because lots of these kids blame themselves. He also clues the parents in that what this kid needs is to build up his positive features. If he's a good soccer player, make sure he gets in the soccer league. Or if he likes his uncle, have his uncle take him out once a week. Its looking for positives but he also tells them, if he gets depressed bring him in. Make sure somebody sees him early. I'm not suggesting that pills are the answer but I am suggesting that the offspring of depressed parents clearly are at greater risk and the question is what can we do to pick it up early and protect them against it.

I had one sad experience in my own family. I have a brother in law who now is 70. He's had a long cycle of recurring depressions. We had my nephew, who is now a doctor, staying with us when he was a medical student. We live in different cities, so I had seen very little of him growing up. My sister never asked for advice and I don't think I would have been smart enough to offer the advice. But he told me with bitterness how he hated his father and he never understood his father was sick. He remembers his father pounding on him because he wasn't practising the piano. Complaining if his lessons weren't done. All sorts of irritable unpleasant behaviours. Never available, never affectionate and he thinks it would have been much easier to bear had he understood his father was sick and these were symptoms of the sickness. But my sister, like most wives under those circumstances, they feign ignorance and they make up excuses of some other kind rather than dealing with it directly. Bill has these family psycho-education sessions in which they meet as a group to talk openly and honestly about the illness. The mother will say

you know I'm sorry Billy but sometimes I just get to feeling this way etc. Apparently it's quite useful. He's accumulating data. His idea of a controlled study with this population was to have a comparison group that got two lectures in which the same content was imparted versus six clinical sessions on a one to one and then family basis with an attempt to individualise it. Instead of talking about symptoms of depression in general, these are the symptoms this family has. The people who had the family intervention report much greater satisfaction and keep in contact with the interviewers. But he still has to wait until they're in their 20s to satisfy the risk question.

Can I bring the US-UK comparison at this point. The adult ADHD story helps bring this out clearly. During the 80s and the 90s, all of a sudden you have this explosion in the US of "you know I had three wives and 4 jobs and things like that which was all because I've had adult ADHD and if this had been picked up I would have been okay. I'd have remained with the same woman and had a successful career. You've had books like John Rately's book Driven to Distraction on the top of the best-seller list. We haven't had any of that in the UK. It may be beginning to creep across the Atlantic. But I convened a working party for the British Association for Psychopharmacology recently to see what could be said about the use of drugs for childhood ADHD. The reason to do this was because in the UK still there was great conservatism about the diagnosis of ADHD even in children. Up until about a year ago very very few children were being put on methylphenidate and you could still hear that a lot of child psychiatrists say they haven't ever prescribed a single drug to any child. Its an extraordinary contrast.

The contrast is extraordinary. What did the working party conclude.

Well I can post you the statement. We had Paul Leber and Rachel Kline there and a range of people from the British Psychological Society, who were hostile to the drug treatment through to industry representatives. From my point of view, the most important bit was to write in what I think was the lesson of the Osheroff case, which is you must revisit how the person's doing and if the outcomes aren't good enough you should be prepared to change what you're doing. This was a means to try and get over peoples' "ethical" prejudices about only doing psychotherapy or only doing drug therapy or whatever. To put the onus on all parties to agree that the well-being of the child should come first.

Very sensible. In the first place, that's a generic difference between the US and the UK. Fads come in much more rapidly here. In the piece I wrote about Michael Shepherd I talked about the values of British empiricism where you pay attention to facts rather than being carried away by theoretical fancies. Second, American psychiatrists have been much more active in private practice than British psychiatrists. Forgetting recent times and managed care, look at the heyday of psychological psychiatry in the 50s and the 60s, it's my impression that the number of psychiatrists per 1000 population was higher in the US than the UK. Then in a place like the Maudsley, which really has been a model for research and theoretical discussion and all the rest, there was much less interest in treatment than there was in diagnosis. This does seem to go with a research outlook. You look at Washington University, all their research was in diagnosis and they paid very little attention to treatment. Admittedly we didn't have a hell of a lot

of evidence in the 50s or 60s but you've got to do something with your patients. But they paid very little attention to this. So the working life of the psychiatrist was much more involved with out-patient treatment in the US than the UK. I think, until recently, the Royal College didn't have a specified requirement for training in psychotherapy. They introduced that recently but that's what we were training everybody for. For better or for worse, we trained everyone in treatment. So that's the difference in climate, into which something comes that has a therapeutic cast to it.

When the British were so reluctant to acknowledge ADHD, few people had trouble saying it was true in the 60s and 70s here. One of the arguments was that the British only accepted it as a diagnosis when the adjective pervasive applied. The kid had to be on the go in the classroom, at home and in the doctors office. Whereas you could get the diagnosis in the States, if in one of the three places the kid looked like a whirling dervish – or even much less than a whirling dervish. I think the furor therapeuticus is American, we think everything is curable.

Why?

It goes back historically. When Alexander Spurzheim, who was the chief disciple of Gall, the phrenologist, came to the United States, phrenology had a phenomenal reception here. This was because what phrenology said was that if you felt the bumps on the skull you could detect the defects as well as the assets of an individual and in principle you could then design a training scheme to suit them. So it was to be useful in prisons. You could find what was wrong with the prisoner to help him do better. There was never any data that it worked but the impulse was there. Its an amelioristic society. So I think that's part of it. Its messianic. You see that all through medicine. While I'm sure there are British chemotherapists who will treat the last metastasis of a cancer with a cocktail that will do the patient in, there are fewer such chemotherapists in the UK than there are in the US. British doctors lived with a system which denied renal dialysis to patients with end-state renal failure, who were above the age of 60, for reasons of economy in the NHS, where we were doing it to demented Alzheimer's patients. I can't explain the medical culture but it's an activist, doing one. I don't know whether we have more than our share of snake oil salesmen, phoneys, in this society but they're surely around. It's an interesting cross cultural difference that applies to all sorts of other things.

I lived through another interesting experiment. One of my friends was Mort Kramer was a biometrician in the NIMH. He helped bring a group of British and American psychiatrists together around the phenomenon which became evident in the post-war period that there was apparently much more schizophrenia in the US. Mort started that branch of the NIMH. He began collecting data on hospitalisations systematically. Doing international comparisons, he found apparently much more schizophrenia in the United States based on hospital records than in the UK which had much more depression. Now since there's a lot of similarity between the two populations, what the hell was going on. So they had a US/UK meeting. I was at one of those early meetings which occurred in an office in Whitehall. Sir Aubrey Lewis chaired the British delegation. Benjamin Pasamanick was one of the people from our side. I was there as a hanger-on. Nobody could explain this,

the question was was it real. So they set up a US/UK comparison in which patients were videotaped. Videotaping as a technique had just come in by that time – the early 60s. So they showed patients interviewed in similar ways all speaking English - we didn't bring in any Hispanic American patients. And we found that American psychiatrists were calling schizophrenic what British psychiatrists were calling depression. It really drew in the horns of American psychiatrists who became much more cautious about the diagnosis of schizophrenia. And then the big difference disappeared.

62 was pre-Washington University's input to all this

Well Washington University was beginning to work independently in this area. I mean these papers are all occurring simultaneously. I went to a meeting that was marvellous for me as a young person in Geneva for the WHO Mental Health Programme, which I date to about 1964. The Advisory Council for Medical Research of the WHO had been persuaded that it was time to have an expert committee look at the justifications for a research programme in mental health in WHO. It was a really remarkable meeting, co-chaired by Bob Felix, who for many years was the head of the NIMH and Sir Aubrey Lewis with a number of people from various countries. Neil Miller, the psychologist from the States, was there – a lovely man. We all agreed there should be a research programme and we proposed an agenda of 10 items, one of which was an international comparison of diagnosis. They set up a series of meetings in which Michael Shepherd, Sir Aubrey Lewis and American, British and French representatives were involved to look at common criteria for psychiatric diagnosis. These were employed in setting up the international classification of disease mental health section that followed. So they had a meeting on schizophrenia, a meeting on manic-depressive disorder etc. And one of them was on child psychiatry. One of them was on mental deficiency, this occurred in Washington.

All of these things were happening simultaneously, while the Washington University group was setting up their first exercise in research diagnostic criteria, the RDC criteria. They used the technique in which they were perfectly happy to have 50% of the patients unclassifiable because they were looking for pure incidences of something or other. That obviously became unfeasible if you wanted to use this as a scheme for classifying patients who came to a clinic. In that setting you had to have a small residual bin rather than a large one. This went through various forms and the British form was much more closely in tune with the World Health Organisation ICD, whereas we went off on our own and our Diagnostic and Statistical Manual took on a life of its own. A life of its own, which is not unrelated to its financial value. In the first six months after the latest version came out, it made 10 million dollars for the American Psychiatric Association. Everybody bought it. In the United States you need to use the codes for reimbursement. And some people think that one of the rushes to go from DSM-III-R to 4 was to make money. The revisions have occurred too fast but I don't know that that was the reason. It certainly makes it easier to decide to go ahead when you know its profitable. Had it been a money loser, since the organisation is in financial trouble, it surely wouldn't have gone ahead. Now that might have been the wrong decision to make.

So, in the early years after the war, the lack of international comparison had become evident. Mort Kramer was one of the people who certainly helped to initiate this look. The British had always been more careful about diagnosis. Michael Shepherd did two important things, although in an era of neuroscience nobody gives a damn about them and in an era where people are looking for big claims, Michael who was always a sceptic irritated everybody. Michael actually looked for his MD thesis at the discharge rate from a large British State hospital from the years 40 - 45 and then again for the years 55 - 60 roughly and found that, before the drugs emerged, there had already been a big change as the result of the community mental health open hospital movement. That was lost entirely in the States; all of the change was credited to the drugs. That was one of the big things Michael did. But he did something else which we're still trying to figure out. Michael actually did was the first person to look at mental disorders as they present in General Practice rather than the psychiatrist's office. That's a theme that David Goldberg has carried forward and pointed out how few of the people that the GP thinks might have a mental condition actually get to a psychiatrist or psychologist. Therefore the sample we work from is unrepresentative compared to the world out there. The really crazy people finally get into the system but we don't see all sorts of intermediate disorders.

So there was much more of an empiric grounding in the UK and there was a much less efflorescent psychotherapy trait in the UK. I mean you want to look at the numbers in Royal College of Psychiatrists, or the Royal Medical Psychological Association in the 1960s, there was an order of magnitude difference between the US and the UK. We went through an enormous efflorescence. I don't know what happened in the UK, but during the Second World War there was such a recognition of the hugeness of the psychiatric problem in combat, that the US Army took doctors who evinced some interest and gave them 60 day courses in psychiatry so they could work meet the need. The so-called 60 day wonders. Of those people, a fair number came back at the end of the war and then underwent regular psychiatric training because they'd become interested in what they were doing. I'm making up the numbers but I think there were maybe 3,000 - 4,000 at the beginning of World War II - not very much larger at the end of World War II - but there are now in the 30,000s. There was no comparable big expansion in the UK. The few people I've met in the UK who are psychotherapists were either MD's who got interested and took some special course or psychologists and psychologists weren't reimbursed. So it was like psychoanalysis, a private practice affair. In Australia, the health insurance scheme had no restrictions on psychiatric service. I think that was true because it only paid for a time for psychiatrists and if you have finite number of psychiatrists, with just so many hours in the week they could work, you control expenditure by having a limited number of providers not by restricting their ability. If they want to serve 5 people all year long instead of 50 people, so be it.

In the United States, we have always been bothered once insurance came along by the number of people out there providing. The insurance companies were happy to certify psychologists because in the beginning they were cheaper, only to find that the net aggregate was more expensive - even if they were cheaper you were getting more people doing it. Then they began to limit the number of sessions you could have. They still do and one of the big fights

in mental health in the United States is for parity - I don't think that has any meaning in the UK.

Social psychiatry needs to be reinvented?

Yes social psychiatry needs to be reinvented and social medicine needs to be reinvented. My argument is that all of medicine is deeply embedded in a social context and, while psychiatry has suffered more than the rest of medicine in the United States, psychiatry is emblematic of what's happening all along. You go to your General Practitioner and he's under pressure to get you out of that office faster. He spends less time listening to your story, explaining the problem if you're diabetic. It's really awful. I think the system is destroying the essence of medical care. Both internists and orthopedists have been doing social medicine without knowing it, like Moliere's bourgeois gentilhomme, but they didn't know it. The better ones get obsessed with the technical part of it.

Is there a problem in that you can't make money out of social psychiatry. The state if it were to run the system, perhaps could show some benefits.

Well I think the system in the United States is on a collision course. There's increasing public dissatisfaction with this truncated pattern of care. It has moderated costs while it took out a lot of profits for itself. Now it's reached the squeeze point where they're raising premiums again, so they don't look quite as heroic as they were. But this country is so obsessed with the idea that the free market is the answer to everything and that government is the ogre - it's such bullshit. Five or eight years ago, we had the so called savings and loan association scandal in the United States where bankruptcies totalled 180 billion dollars because Government regulation had been removed. These guys just went around throwing the money and now the tax payer is making it up because the banking system required it. That's what we do each time. These suckers in Indonesia take out all that money in the system and the IMF loan is not for the poor in Indonesia, it is to replace the banks because we need an international banking system. That's what we did in the United States. But it's all seen as the private sector is all good and the government sector is all bad. You have no idea the kind of bad press the NHS has had in the US since its inception. But we're the only God damn country left in the world without a scheme for covering ordinary citizens.

So do I think there will be National Health Insurance in the United States? I will tell you. I thought there would be since 1948 and the Wagner-Murray-Dingell bill and Franklin Delano Roosevelt and the like. There's a marvellous story about an American Senator named Claude Pepper who remained a New-Dealer until he died 5 or 8 years ago. When Pepper died he, of course, went right to heaven and when he arrived at heaven he was told by St Peter that all new arrivals have the right to ask the Lord a question and would he like to exercise the right. He said he certainly would. So he was ushered into her presence and he asked her "Oh Lord will there ever be National Health Insurance in the United States?" She said "Son, there will be but not in my life time". So I don't know. There was a marvellous opportunity, which Clinton missed out on. I was furious with the way they did every thing wrong. But that opportunity was lost and I don't know how it's going to be recaptured because this belief that Government is evil and business is good is

widespread. I don't believe it but that the majority of Americans do. The notion that Big Brother can do better for you is one so many people don't believe.

So all social aspects of care, as I said, are at risk at the current time. The way the scheme works in the United States, in many of these managed care organisations, is that the provider, the doctor, the psychologist is put at risk if he makes referrals. There's an account kept of the referrals he makes, the hospitalisations, the tests he orders etc and if you exceed some predetermined average or norm it comes out of your next year's reimbursement. If you're a conscientious doctor, you don't let that stop you but it worries you because you're always worried when you deny a treatment on medical grounds that you might be denying it on economic grounds. It's dreadful; it undermines the doctors confidence in himself. It makes patients doubt their doctors. Whose interest does the doctor have in mind - not that all doctors were great before but this is now putting everybody at risk.

You mentioned Genetics earlier, which is a hot issue at the moment. The genes people say they're not determinists any more but you have to wonder that if underneath the surface they still are. Then on the opposite side you have people like Richard Lewontin and Leon Kamin. How do you see all that at the moment? There was the meeting a few years ago that was supposed to be held by the NIMH - the one about genes and violence which ran into so much controversy.

Yes, well that gets of course into political questions too. A misapplied genetics has all sorts of evil and terrible effects and the example in the United States is the Herrnstein and Murray bell-curve. Lots of geneticists are closet believers in the bell curve position. There are genetic definitions, which are just false. Heritability is supposed to be the percentage of a trait that is attributable to genetic factors. Now heritability is reported at the end of a study in which they attempt to partition this out, as if it's a quality of the gene instead of the quality of the gene in a given environment or range of environments. The geneticists will admit when you beat them over the head with it yes of course you might get a different heritability estimate in a different environment. But, if you do an R A Fisher kind of experiment and you had a dry soil and a soaked soil, the seeds wouldn't grow in some places so there'd be no heritability. Now that's the extreme but it is one of the things that happens the marvellous plants behind the green revolution - they require a lot of nitrogen and fertiliser in the soil so it's a big problem if you can't supply the nitrogen and the fertiliser. There's always an interaction effect. From a stand point of discovering as many pieces as we can of the human genome, just think of the excitement when we have all that. When you come into me as a patient and you have an infection and I can say because I've done the requisite test that you're a rapid metaboliser of this antibiotic, so I'd better give you three times the dose. Or somebody else needs 1/3rd the dose or whatever.

But that isn't the way it's being looked. Look at all the garbage about cloning. People know about the ethics and assume that the clone of the person would be a replica of the person. Not so. Things can go wrong in the uterus, things can go wrong in the birth process. It depends on the environment you have, the food you eat. I wrote something about this 20 years ago, when the first

frog had been cloned, and it's equally true now. To get Mozart, you would not only need the DNA from the genome, you would need his father, his mother, the Vienna of the time, Papa Haydn etc etc. You wouldn't get it out but all of the big ethical debates are as if you'd have a clone. It's the pervasiveness of this belief that's the issue. The people who do twin studies celebrate the fact that there is a correlation of 0.5 in some traits between identical twins versus what you see in fraternal twins. Well why the hell isn't it 1.0 - they've got the same genes. All sorts of things can slip in the process. They are more similar there's no question about that. I don't want to be a Luddite, I don't believe we should deny ourselves the knowledge that we might get. But the hazards are just enormous. Suppose insurance companies can get access. If you want to make money and you're running a life insurance business, you want to get just healthy people who will pay you a premium enough so that the death benefit would be less than what you've earned over the years. If you're thinking of insurance as a social scheme you want everybody to share in the business the sick as well as the well. If they can exclude the people who are at risk from this that or the other thing, that's fine in business terms but what happens to the people who really need the insurance. It's not the people who outlive their longevity who need life insurance it's the survivors of the people who won't get to that point.

So how to control the information. We haven't done very well up to now. No one will any longer deny an insurance company the right when it enrolls an individual subscriber to get a blood pressure. Well that's like doing the genes; it's a little less informative than a whole other set of things. But should I be barred from working in a plant if I have a 10% greater susceptibility, I don't think so. But there's a condition known as xerodermatitis pigmentosa, where when you are subject to radiation, chemicals, everything else, you need to be kept out of that plant. There's no way you can make a plant so safe that I wouldn't be at somewhat greater risk. So there are some places where knowledge of something ought to lead to a common-sense approach. I think Lewontin is terrific. I think he sometimes gets carried away. He and Kamin fight because they recognise what the end of it is. Leon Kamin is not only a guy who objects to genetics because of the way it can be misused but Kamin was himself a terrible victim of McCarthyism because of his left wing views - he had to get out of the country. Couldn't get a job. He's back in the country now and this world is different now to what it was then. So he has all the reasons for not trusting big powers that can manipulate this information and there's still some terrible things that occur.

There is an article of unbelievable outrageousness that appeared of all things in *Social Science and Medicine* when McEwen was still its editor. There are two biologists at the University of British Columbia, who wrote a piece which claimed that the reason HIV infection is so much more prominent in blacks is that because blacks employ a reproductive strategy that's more primitive than that applied by whites. There are two strategies people who work on reproduction put forward, one is where you make a few children and you invest a lot in raising them versus the other which is scattering your seed widely. Now the problem is when you reify this and ignore culture and all of the other factors. He published it and when people attacked him for publishing it his response was freedom of speech. Does freedom of speech mean an astronomy journal should take an article saying that the earth is flat

or that the sun rotates around the earth? So it's a vicious terrible threatening possibility but I think what we have to do is we have to fight it every step.

Is there a kind of cycle which says that when politics have failed, when social inputs have failed there is a turn to biological solutions of which drug treatments for kids who are depressed are one option and genes are another?

I suppose so. In the United States, the collapse of the Soviet Union is widely interpreted as a triumph of the free market rather than seeing it as a collapse of a corrupt totalitarian state. So this then meets other things that makes social interventions look less promising.....

NEW QUESTION

Many liberal efforts seem to have failed though. Joan McCord reported some years ago on a scheme for delinquents put in place in the Boston area where some got extensive input from social services and others didn't. Following both groups up 40 years later, intervention had clearly made a difference but in the wrong direction – the treated group who had had the social benefits had higher death rates, higher crime rates, worse health and greater relationship breakdown. It doesn't encourage non-biological solutions. Its interesting, I think that many of the eugenicists, like Shaw and the Huxleys had previously been liberals interested in social engineering. Your career almost seems to have gone in the opposite direction?

NEW QUESTION

You are unique in having the ultimate insiders perspective on both Michael Shepherd and Gerry Klerman, having delivered eulogies or written obituaries on both. In my Antidepressant Era, I have given them both key roles in the drama that has been psychopharmacology, based on I think an analysis of how history will ultimately see them, given their achievements. At the moment many others would probably think I overestimate their contributions. How did you see them both?