PSYCHIATRY & THE MARCH OF FOLLY?
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A number of people I've interviewed mentioned the trouble you had in Holland during the 60s when it was not fashionable - almost not possible to be a biological psychiatrist. Do you want to tell me something about it?

To clarify that I need to bring up some historical data. I started my residency in psychiatry in the late 50s. I had always been interested in research. I did research in Neurology as a student at the university of Leyden and later in the army, and then I started training in psychiatry as a future neurologist. At that time, in Holland, you had to do one and a half years in the "sister" discipline. I started out with psychiatry. I had left the army 30 March and I started April 1st 1958 - at the very time that the first MAO inhibitor and the first tricylcic was introduced. Well, the first monoamine inhibitors were immediately fascinating to use because they exerted antidepressant effects and it was already known that they had an effect on monoamines. So from that time on, I was captivated, amongst other things, by the question whether there is any relation between antidepressant action and changes in brain monoamine function and metabolism, and whether there is anything amiss with brain monoamines in depressives responsive to those antidepressants.

So I started to work from the very first day of my residency with Bart Leynse who was a biochemist. At first he was somewhat hesitant. In the 50s, for a biochemist to work together with a psychiatrist! Could it harm your reputation... but in the end he accepted and then I worked on that topic and I wrote a thesis - a combined psychiatric/biochemical/ pharmacological discourse - about the significance of MAO inhibition as a therapeutic principle in depression.

I defended my thesis in 1962. I finished my residency in 1963 and subsequently I worked for 3 years in Rotterdam as chef de clinique (clinical director) of the Psychiatric University Clinic. In 1965 I was invited to come to Groningen to start a department of biological psychiatry. How come? That was the single idea of a very important man in my life. Sitting on the board of a University, at that time, was a more or less honorary position, but there was always one professional and fulltime board member and in the 60s that full time board member happened to be a former general practitioner, Dr. Dijkhuis. He had read my thesis and in one way or another he felt that the question of the link between brain and behaviour was novel and important.

So he asked me to come to Groningen and I got all the money I requested to set up a Department of Biological Psychiatry. At that time that was an enormous amount to invest in Psychiatry - I needed facilities for pharmacology, biochemistry, animal behaviour and neurophysiology and a clinical research unit. I accepted the position. I remember very well I discussed it thoroughly with my wife. It was the mid 60's, the heyday of psychoanalysis, of psycho-dynamic thinking. Assuming the official role of Head of a Department of Biological Psychiatry, the first of its kind in Europe, in that era would mark me for ever as Mr Biological Psychiatry, an idea I didn't like. I feel to I am a full blown, allround psychiatrist. I like biological psychiatry, but I'm more than that concept implies. Yet the challenge appealed to me and I went.

My presentiment came true. I was pinpointed and earmarked as Mr Biological Psychiatry in Holland. At the end of the 60s, a social revolution took place - in America politically oriented with the Vietnam-war as a focus, but in countries like Germany and Italy and Holland it was a anti-establishment movement - and biological psychiatry became a focus of the revolutionaries. Biological psychiatry was reactionary, was conservative, was anti-human, it was everything bad, politically, medically, therapeutically. And so anti-psychiatry developed. There exist no psychiatric diseases, according to the most

extreme protagonists; there is just labelling by a society that doesn't accept unusual behaviour.

Antipsychiatry became more and more influential and biological psychiatry more and more unacceptable and I was Mr. Biological Psychiatry. In 1977, I went from Groningen to Utrecht, at the heydays of anti-psychiatry, and anti-biological psychiatry. Groningen is located more eccentric than Utrecht and was less extreme, and so biological psychiatry had been more or less accepted. The most extreme revolutionaries were located on the Western coast - in Amsterdam and Utrecht. The Psychiatric University Clinic, after my arrival, became a focus of political turmoil and my very presence there became highly politicized. Frequently there was trouble in terms of disturbed symposia, disturbed lectures, protests against biological research, street riots against electroshock treatment, even physical threats.

I've heard from people like Alec Coppen that when they went over to talk to meetings in Holland, they found that there were demonstrators outside the lecture theatres.

Oh yes, we had police there. It reminded me of the 20s and 30s, of the clashes in Germany between the communists and the fascists. That time too, there existed a highly politicized environment that incorporated all kinds of extraneous domains. What has biological psychiatry to do with conservatism, reactionary convictions? - but all of a sudden it was, within medicine, the symbol of everything that at the time was unacceptable. Psychiatry it was claimed had nothing to do with the brain. It had even little to do, according to some anti-psychiatrists, with inner psychological workings - they were also, but not so vehemently, against psychodynamics. It was all caused by social factors, maybe by relational factors but in particular by social ills. So, those were stirring, but interesting times.

Did it ever get to the stage as being actually worrying to you or your wife in terms of serious threats to your personal safety?

Yes indeed, including the children. There were times when we had to have police protection. My family, more than I, suffered in consequence of that. I, to an extent, was concerned, but on the other hand I flourished; I am at my best, I think, when I have a cause in mind, which I can defend and fight for and I felt the biological approach had a lot to offer to psychiatry, that it was a cause worth fighting for. Scientifically and clinically - in terms of providing novel treatments to psychiatric patients. I felt I did progressive not conservative things, since I was working for the betterment of diagnosis and treatment. That is not reactionary. Rather it is a progressive attitude and I liked to defend it. So it never occured to me to resign. If you ask me: were you perfectly happy with the situation? I would say: no, but I didn't suffer. Also, something which was very important to me, was that the Dean, the President of the Hospital, the Faculty and the staff of the Department were always behind me, both in Groningen and Utrecht. They always thought the work was important. No-one in the University or the Hospital or the Department ever tried to belittle or discourage me or say: Van Praag, maybe you should shrink your activities a little bit, keep it more in the background.

Why do you think it happened. You've mentioned the whole 68 period but one of the other things I've heard you mention before is that there is a certain Calvinism within Holland. Do you want to elaborate on that?

Indeed, that trend exists. Even among the revolutionaries. Holland is in nature still a very Calvinistic country. The churches are empty, but yet it is a nation which has a strong Calvinistic mentality, in that principles are highly important; you can not and should not compromise on principles; the right thing to do is: implement your principle, whatever it costs. There is little tendency to relativize; to reason. Yes, there are principles, but there's also a practice, real life, and maybe we should discuss whether the principle should and can be fully implemented. That religious fervor inspired the antipsychiatric movement. Likewise it dictated the idea that the spiritual is a domain independent from the physical and that the former domain is of a higher order and of greater value than the physical. The idea also that something in the human psychological existence is immortal, independent from the body, from the brain; that there is something incorporeal that survives death. That belief system was being questionned, in the opinion of my opponents, by my work.

I replied, there can be no behaviour, no experience, without corresponding cerebral underpinnings. But the very idea that by ECT, by use of a machine or a drug, you can change the mood of a guild-ridden melancholic patient, ran counter to ingrained ideas of Christianity and particularly of Calvinism. I think, though I can't prove it, that an element of that philosophy gave antipsychiatry its fervor. The unwillingness to accept that the psychological realm, though of course different from the physical, can actually be influenced by physical means, that you can study the physical underpinnings of whatever psychological process, was anathema to antipsychiatrists. Listen, I'm not a theologian; if you believe that there is something in the human existence, that is immortal, by all means, think so, I do not deny it. But it is not the object of my studies. That's a subject for theology. I study phenomena which I consider to be dependent on a functioning brain. If there's more, it belongs to another discipline.

So the late 60s and the 70s represented a highly politicized era, that was highly revolutionary and anti-bourgeois. Any type of establishment, anything that was believed in by fathers and mothers was rejected. Biological psychiatry, moreover, had to struggle against the combined force of religious convictions regarding the immortal soul and the firmly Calvinistic inclination to adher to (antipsychiatric) principles, even if they turn out to be unpractical, not usefull or even wrong.

To the point now that ECT is still very difficult to use in Holland, isn't it.

Yes. I find it almost unbelievable. Almost the first phone call, I had when I came back to Holland two years ago, was the request to sit on a Comitttee to judge ECT. I said: has nothing changed in all of these years and they said "well you know the parliament has asked questions". I said: what questions - let them read. ECT is not a panacea but there is enough data to show them that it’s a legitimate method within certain diagnostic borders. Gradually, however, the resistence is easing up. When I was in Utrecht, we were the only department in Holland practising ECT. I contended: that is unacceptable - you prevent a treatment that has been shown to be effective for a substantial number of patients and for what reasons? You call it ethical reasons but you’re talking about your private ethics, not the patient's interests. Nowadays ECT is permitted, but only in certain centres, with all kinds of protective measures. I am not against protective measures, but if you have a therapeutic method available only in a few centres, you can be sure that many patients will not be treated. Distance is a therapeutic handicap.

I can give you another example of unethical political influence in psychiatry. For years in Holland there was no MAO inhibitor available. Even though there was clear evidence that

MAO inhibitors can be efficacious in major depression and in atypical depression. Many didn't "believe" in them, but in fact not to have those drugs available was unethical.

Remarkable isn't it. And this is despite the fact that there are two large pharmaceutical companies within Holland. One would have thought that the industry could do something more to ...

But the industry, in the 70s, and 80s, was very much in that same boat - it was considered devilish. The industries make money, make big money out of drugs and they were in the eyes of many as much detestable as biological psychiatry was. They failed to have a counterbalancing effect.

Frankly, to be looked at with suspicion is not a prerogative of biological psychiatry. The notion that people have to be protected against psychiatry is by to means rare. Recently, in Holland a new law was passed, that makes it extremely hard, much harder even than ever before, to admit psychiatric patients. Again - whose interests are we serving this way - certainly not those of the patients. It is as if psychiatrists are dangerous: Pied Pipers of Hamelin, who tend to admit people against their will, and against their best interests. It took 10 years to prepare the law and now it's implemented. I think this is terrible; absolutely terrible that you cannot admit patients who are floridly psychotic but not genuinely dangerous. Is this ethics? This is anti-ethics, ethics upside down.

Let me hop for a second - you left Utrecht and went to the US but before I pick up on that I want to pick up on the fact that you came back from the US to Maastricht to unify the Department. Now why was unification necessary? It seems an extraordinary need

Sure, but it had a direct relation to what we just discussed. Maastricht is Holland's youngest University. It is 20 years old and the department of psychiatry started from the beginning as a tripartite organisation. That was still true when I came two years ago - there were three independent Departments of Psychiatry. One was called Social Psychiatry; the second Clinical Psychiatry, that's hospital psychiatry; the third Psychobiology and Neuropsychology. Independent Chairs, independent budgets. The university felt perhaps it would be a better idea to unify it.

I know, it sounds a bit pathetic, but our profession is truly dear to my heart, so I thought unification was a very good cause. In addition, I have always liked management, apart from teaching and research. In New York, I performed a similar but much larger unification job. I had experienced how interesting this kind of scientific management can be and so I liked the idea. I had a secondary reason, which was to be near my children and grandchildren. Finally, after almost eleven years in New York, I was restless enough to say well let's climb one more mountain. A possible hidden motive was the wish to obscure the inevitable ageing process. I liked the idea of starting again and feeling younger than I really am.

You went from Utrecht to Einstein. Can I ask you how that came about - did you go partly because things were so hot in Utrecht.

I know that some people thought so, but no that was not the case. I liked my job in Utrecht. It included sometimes fierce fighting but it was for a good cause and, as I said, the Department and the Faculty and the Hospital were behind me, unconditionally.. No I was called, completely unexpected one day, by the Dean of the Albert Einstein College of

Medicine, Ephraim Friedman. The chairman of the search committee was Dom Purpura, the brain researcher - later he became the Dean. They called me and also the late Nate Kline. I knew him very well. They asked me would I be interested in coming to Einstein as Chairman with two special commissions. First of all, to unify psychiatry at Einstein and Montefiori and their affiliated hospitals and secondly to boost research. The last Chairman had been Ed Sachar and with his appointment, the Dean had tried to open the doors to research. For long the Einstein department had been highly psychoanalytic. Ed Sachar, however, left Einstein after less than 2 years to go to Columbia; an interim chair, Wagner Bridger, was appointed and subsequently they asked me. Well, I hadn't given it any thought. I had been many times to America but my wife was not so interested. The dean said come, look for yourself and let your wife join you and so we came. I found it, first of all, interesting from the management point of view, which at that time was not very much the responsibility of departmental chairmen in Holland. The American experience looked fascinating, to my wife as well. Also I felt a bit flattered at being invited to come to Einstein. So, after some negotiations I went.

What kind of situation did you find there. Was it still analytically controlled?

In many ways, yes; though Einstein more so than Montefiore. I had to unify those departments and I must say I was happy that the former Chairs, the acting Chair in Einstein and the regular Chair at Montefiori had already left. It would have been very difficult to work together. There was nothing about the unification in writing. In the more or less typical, classical American way, they said: Van Praag you have accepted, we are very interested in the outcome, but you do it any way you want. That's still one of the most inspiring aspects of the American society. Confidence in and high regard for initiative and improvisation. Just do it; you know, you're not governed by 3,000 or so rules like in Holland - do it.

So there were two Academic Psychiatric Departments each with their own affiliated hospitals. I tried to achieve a number of things. First of all the unification. What they had in mind was academic unification - combining the research, training and teaching. But I felt that it was a unique chance to bring about a clinical commonwealth as well. The two institutions and their affiliated hospitals together covered a large segment, 80%, of mental health delivery in the Bronx, a borough of 1.3 million people, so I felt that the Department could become a true comprehensive network of facilities.

The Dean once said to me, Van Praag you work so hard also in the clinical domain, but its not necessary. I replied: “But I like it, it's an interesting experiment to have an Academic Department so much involved in management of day to day clinical activity; having the chance to demonstrate that we are not living in ivory towers but down to earth; contributing to better organisation and innovation in mental health delivery.” So that was one task - unification, academically and clinically. I promoted research. I recruited a considerable number of research oriented people, not only in biology. For instance: when I arrived the payroll mentioned one half time behaviour therapist. We were responsible for more than one million people, among whom many chronic patients and there was one half time behaviourist! That I felt to be absolutely unacceptable.

I've tried to implement what I have always preached: a harmonious development of psychiatry, with balanced attention for biological, psychological and social determinants of abnormal behavior and for the corresponding treatment methods. Yes, I do believe in psychological interventions. There is little proof for the efficiency of psychodynamic psychotherapy but for cognitive and behavioural approaches there is. It would a priori be

silly to doubt that you can change behaviour by learning; normal behaviour changes - why then not abnormal behaviour? Psychodynamic psychotherapy, I'm willing to give the benefit of the doubt. It is hard to believe that one can understand present behavior without taking the past into account. If that is so, the assumption that one can lighten the present, by clarifying the past, seems reasonable. So, I tried to build up a multi-dimensional, research-orientated department. I also established a stronghold in the biological sciences; there existed one basic lab when I came and there were five when I left. Certainly there were still many things to be desired, but the basic model of an academic psychiatric department had been built up, at least the model I prefer.

I regret the tendency to make psychiatry mono-dimensional, whatever that dimension might be: psychoanalytic, social or biological. You know, the idea that schizophrenia is a brain disease, that is a terrible simplication, originating in the 19th century, but presently revitalised. Of course, it is a brain disease, but it is much more than that. There are many more variables involved. The brain dysfunctions do not drop from the skies. They are a product of a host of noxious variables, that can be psychological, relational and environmental in nature. This applies to the entire field of abnormal behaviour; to deny that would be unscientific, even if presently "politically correct". Tunnelvision is not to the benefit of our patients and not to the benefit of our profession as a science.

So I have been viewed in Holland as a pur sang biological psychiatrist, but in fact, I am and have always been a generalist. My first inaugural lecture (1968) was entitled The Complementary Relation between Biological and Psychodynamic Psychiatry. It's broad basis is what determines the charm, the importance of the profession. It is a three dimensional structure and our professional identity is in keeping. We are no more than amateur psychologists, amateur sociologists, amateur neurobiologists but we are not amateurs when it comes to bridging these divides. Synthesizers, that's what we are and should remain.

Let me switch to a few of your ideas about what we've learnt from the use of drugs and research on drugs. From the start you've been associated with two ideas - the 5HT hypothesis and a dimensional approach towards things. You can pick up either of these. Alec Coppen says that the 5HT idea was a North Sea idea between yourself, himself and Ashcroft up in Aberdeen.

Yes, I could agree with that. But I have been more than that, I have been a monoamine man. I have worked in Groningen with Jacob Korf in monoamines, not only with serotonin. Serotonin was neglected in America, where gradually it was only catecholamines that counted. But Korf and I, we always have tried to study, for instance in the CSF, both serotonin, noradrenaline and dopamine metabolism. I have worked together with neurologists in Parkinson's disease as well as with psychiatrists in depression to investigate what monoamines steer. Serotonin was only one of the variables we were interested in. In the USA, for many years it was all catecholamines; so the few people that remained true to serotonin, became the "serotonin-people". Yes, we were, but we had more irons in the fire.

Now the dimensional ideas. From the beginning I had difficulties with nosology. One of my teachers, Rümke, was a convinced nosologist but I didn't see the light. I saw so many patients of which I felt "yes, they are depressed; yes, they are psychotic, but to claim that they fulfil strict criteria for any particular diagnosis, no". In depression you see so many anxious people; people with panic disorder, with obsessions and so I ...

But you introduced the notion of vital depression in the 60's. you were one of the people associated with trying to pick out a form of depression that might respond to drug treatment.

Yes indeed, very early on, in the late 50s, we tried to operationalise the various forms of depression: vital depression, personal depression and mixed forms. Those are, however, strictly syndromal concepts without any further nosological connotations. If you want to study for instance vital depression, we reasoned, you have to define the concept. You have to describe in detail what you're dealing with. The term endogenous I didn't like because it was so much linked to absence of precipitating events and to heredity. We reasoned we need terms that, etiologically, are totally neutral, that are purely symptomatological and descriptive, without any connotation as to severity, duration, or causation. Moreover we developed, and we were the first to do so, a structured, standardised interview for the diagnosis of vital depression. Okay, of course, nothing as elaborate as the present days standardised interviews, but it was the first of its kind. The Hamilton had just been published. But the Hamilton is ...

Just a checklist ..

A checklist yes and we wanted to study and compare the various syndromal depression types. What are antidepressants doing in which syndromal type of depression; are monoamine systems disturbed in certain syndromal types of depression. So it was necessary to operationalise types of depression and to develop an instrument to assess those. All that, by the way, was, of course, anathema to the that time ruling class of psychoanalytic psychiatrists. Operationalisation of diagnoses and standardisation of interviews was robotic; it was, as one said to me, destroying the very fabric of psychiatry. This approach was hardly acceptable in the 50s and 60s.

We focussed our studies on a well-defined and assessable syndrome, i.e. the syndrom of vital depression, so that it would be as clear as possible what we were talking about. We studied “pure” cases, but at the same time it was clear, that relatively few patients were “pure”, that many showed also features of other syndromes. So in order to be able to give a precise diagnostic evaluation of a certain patient, a strategy additional to precise syndrome definition had to be pursued, one I have designated as “functional psychopathology.”

We reasoned as follows. The basic units of disease in psychiatry are the psychological dysfunctions, such as deviations in mood-, anxiety- and aggression-regulation; motoricity; level of initiative; perception; cognition; memory; concentration, and many others. Hence, to obtain a clear picture of what in a given patient is dysfunctioning and what is still normally functioning, one has to dissect the psychopathological syndrome(s) into its component parts, i.e. the psychological dysfunctions. They should be assessed and measured, and if no adequate psychometric instruments are available, those should be developed. In this way, a, what you might call “psychiatric physiology” will be developed, elevating psychopathology to a truly scientific level. Diagnosing in psychiatry, I maintained, is a 3-tier process: the disorder should be established, the syndrome characterized and finally the functional composition of the syndrome(s) determined.

I consider myself as a nosological sceptic. The third diagnostic tier is closest to my heart. I realise, however, that we need a common, simple language to communicate. The disease-based-system meets these terms, though it is a primitive and imprecise language. I do not want to do away with categories, I want to have the system functionally

complemented. For my teacher Rümke, convinced nosologist as he was, those ideas were anathema, but he was a great man and tolerated dissident residents.

In the review you wrote of my book Make Believes in Psychiatry, you mentioned that it seems I have prophesized several developments in psychiatry already in the 50s and the 60s. The remark was ironicallly meant, I think, but in all modesty, it can be said that many ideas I have professed during my academic life, occured to me at an early stage in my carreer. It sounds pendantic, but I cannot help it.

How much does the dimensional approach that you've taken, where you've been interested in concepts like irritability, how much does that link or otherwise to Fritz Freyhan's ideas of target symptoms.

Not the same. People always raise this issue. One example, I don't study acoustic hallucinations. Marius Romme in my Department in Maastricht has done interesting studies with people who hear voices but who are not psychotic. What I'm interested in is not in hearing voices as such but in the underlying psychological, i.e. perceptual disturbances. Thát is what functional psychopathology is all about. It is not the symptom as it is being characterised by the doctor or formulated by the patient but the underlying neuropsychological disturbances, that are being studied and related to neurobiology. Symptoms are not what I have in mind. It's really a psychiatric physiology I hope to establish. The term dimensional, in fact, is not adequate. I don't follow so much a dimensional approach, but rather a functional approach.

Well now could I ask you how that links then to the work of Eysenck during the late 50s and early 60s because he had this idea that people fall on different points of a set of physiological dimensions and that people could be picked out by drug challenge tests. Arguably it would be awfully useful if we were to do that nowadays, because so as far as I know the work is still valid.

No doubt. I am not a connoisseur of Eysenck's work, but what I am unhappy with is the small number of dimensions he uses - for example introversion-extraversion. Dichotomies make me uneasy. Its too simple to divide people into two groups even if you link them dimensionally. Eysenck is a dimensionalist but his system is to meagre. Defining personality or psychopathology on too few dimensions, means an unacceptable simplification of mental pathology. This doesn't detract from the fact that Eysenk's system is still a useful starting point.

One of the problems though for the Eysenck work appeared to be that the models worked beautifully, while we only had stimulants and sedatives, but once we began to get the tricyclic antidepressants which were neither stimulant nor sedative they didn't fit into the framework.

But what is the framework. Again, it is too much a reductionistic hat rack. In the psychological apparatus, there is an innumerable number of functional abilities and the dimensions, or better still the dysfunctions, to be distinghuished, should reflect the complexity of the psyche. The human mind is much too rich for simple dichotomies such as: extraversion/introversion, or type A/ type B personalities. Once more, functional psychopathology I define as the product of systematic dissection of psychopathological syndromes in their component parts, i.e. the psychological dysfunctions and of a personality structure in the component personality traits and subsequent measurement of dysfunctions and traits in a, hopefully, sophisticated way. Each psychological (dys-)

function and each personality trait can be taken as a dimension, that reaches from zero to maximally present.

If we, eventually, hope to understand brain and behaviour relationships, the functional approach is, I think, the way to go. I find it hard to believe that one day we will have unravelled the biology of, for instance type A-behavior or of the concept of extraversion or find specific drugs to influence them. They are too broad. I find it much more likely that one day that will be the case for behavioral components like, increased aggressiveness, impulsivity, lability of mood, to give only a few examples.

You have introduced in your book, the idea of a stem and branch - that it's maybe worth chasing 5HT the whole way through a range of behaviours. Do you want to comment on that.

Yes and no. Chasing 5-HT through a range of behaviors, but searching for relations between 5-HT and components of those behaviors. The same is useful for noradrenaline and dopamine. In fact in a paper in '75 we reported that the dopamine disturbance found in Parkinson's disease, i.e. lowering of CSF HVA, is not restricted to that disease, but is also found in retarded depression and in inert psychotics. Apparently, we postulated, it relates to lack of iniatitive rather than to a particular diagnosis. Serotonin disturbances in the brain, we have hypothesized, are related to instability of agression- and anxiety regulation and noradrenergic dysfunctions to hedonic disturbances. Biological dysfunctions in psychiatric disorders seem, thus, to correlate with psychological dysfunctions across diagnoses, rather than to categorical constructs.

Another example demonstrated by Renee Kahn and me is that the augmented cortisol response to MCPP found in panic disorder turned out to be very strongly correlated with pre-test anxiety, rather than with panic disorder. Apparently it is not specific for panic disorder, it is rather related to anxiety, irrespective of diagnosis. I think there are many more examples to support this idea that brain dysfunctions correlate better with disturbed psychological domains than with present day's categorical diagnostic constructs.

Let me chase this a bit further - what you seem to be saying is that there's a close link between brain function and personality. If so what do you make of the public health experiment going on at the moment with so many Americans taking Prozac?

The medical profession is never helped by perceived miracle drugs. Miracle drugs are myths. On the other hand, the Prozac uproar is probably telling us something. Prozac, and I think it holds for SSRI's in general, are not merely axis I drugs, active in depression, in OCD, in panic disorder, but in addition they seem to influence personality traits, personality dysfunctions, like impulsivity, irritability and lability of mood. The SSRI's might be the first generation of drugs that indeed affect personality traits.

I have seen several patients in whom that seem to have happened. The spouse said, with Prozac my partner became a different person. Why? Before, in the morning he or she was explosive; an unpleasant, irritable person, I had always to be very careful not to get into conflict and now he/she is very different, much more tolerant, much more mellow. The reason for that is, I think, not a miracle, but the fact that personality traits are being influenced. I hope and I think that the more specific we will be in impacting on particular neuronal systems, the better the prospect of developing drugs that will benefit certain personality disorders. There is a genetics of personality traits, there is a biology of personality traits, there will be a psychopharmacology of personality disorders/dysfunctions in the future.

One could even argue that we're likely to get to the genes of personality traits and the biology of personality traits quicker than we were getting to the genes or biology of schizophrenia.

Maybe. Personality biology would not be without danger. Who wants to have his personality “mechanically” changed? Personality pharmacology should be handled with great care and caution. Beware of cosmetic personality psychopharmacology. On the other hand, there are personality traits that are harmful for an individual as well as for his surroundings. If well-controled, there is room for pharmacological personality reconstruction. The emerging opportunities to interfere in personality structure, I think, could be an explanation for the remarkable upheaval Prozac has caused in the media.

There is a history here actually. I went back and read some of Kuhn's early case histories and he describes exactly the same kind of phenomena that are described in the book Listening to Prozac - of people who are sexual perverts, getting an antidepressant and their sexual perversion going away. This history has been lost. Do we just go around the circle some times?

Well, I didn't know that observation of Kuhn, but we have been focusing for so long mainly on Axis I diagnoses and failed to systematically study the effect of psychotropic drugs in pure personality disorders or in co-morbid personality disorder. Maybe there are more drugs than Prozac that have an impact on personality traits. Also you have to take into account that the authorities prevent this kind of research - the drug is marketed for depression or anxiety or some other diagnosis and to use it in another diagnostic category is generally not permitted.

With DSM III, the psychiatric profession in the US went biological. With this book Listening to Prozac its as though at street level they are going biological.

You are right. I regret this monomania. Let me make you a complement. It seems different in the U.K. British psychiatry, over the years has been much more eclectic, more level-headed, less theory-driven. In the States official psychiatry is presently biological. You don't get any important Chair without being a researcher, with a biological track record. Biology and drugs are now considered to be the major pillars of psychiatry, the successors of psycho-analysis; too much, I think, in that other realms are being neglected. Cavalier-like people say "oh psychotherapy that is for social workers and psychologists". I think that attitude is destructive for the profession. Psychotherapy and the underlying theories are an integral part of psychiatry. Training in psychiatry should include biological and psychological theories; mastering of at least two forms of psychotherapy, as well as being an expert in the use of psychotropic drugs. Biology is domineering American psychiatry; not at the grassroots though. If you interview new residents you note that most of them want to be trained in the psychotherapies. But for “official psychiatry” it’s biological research that truly counts. And so a drug like Prozac gets enormous attention. Partly for good reasons. Partly it is pure sensationalism: the drug shouldn't be all over the newspapers, on TV etc, that’s a kind of circus. Again, we have no miracle drugs - after such a period of elation, disappointment will follow, to the detriment of the profession.

All of these concepts like irritability and impulsivity and this way of thinking are in your book <u>Make Believes in Psychiatry</u> which I think is one of the few serious books in this area that could reach out to the public. But you said to me on the way here that you were surprised that there it has caused so little fuss.

Well I had guessed that quite a number of people would strongly and publicly disagree with my views on diagnosis and diagnosing; also with the chapter on Make Believes - discussing the, what I have called in another context, “nosologomania”, embracing as we do all kinds of new disease entities of dubious validity, such as multiple personality disorder, chronic fatigue syndrome and winter depression. I do not, of course, deny that depressions may occur preferentially in the winter. I do not deny that some people are very fatigued and I do not deny that certain people may have dramatic changes in personality, but I very much question on the basis of the prevailing data that these are discrete entities - diseases that you can study as such, that you can develop specific treatments for, that you can scrutinize for their epidemiology, biology and so on.

I regret that the book has not generated more discussion. One might disagree with my critique, but I flatter myself with the idea that my arguments are relevant and that the points I have raised, are of considerable importance for the further development of psychiatric research, particularly for biological research.

One of the reasons that there may not have been much correspondence to date is that there's widespread support for these views in the profession.

That is a very positive view. But it is also possible that people strongly disagree but find it not so easy to object to my points of view and then it is easier to be silent. Because if the majority is working as they do..

If the zeitgeist is going the way you want, then why object?

That's right. So far the Zeitgeist is not in favour of what I am trying to say. So the majority just ignores it. I hope that you are right and that the majority thinks that my ideas are not so bad, but I wonder whether it's right. So far my ideas about functional psychopathology, functional psychopharmacology, verticalisation of psychiatric diagnosing, my view that the DSM provides diagnostic guidelines unsuitable to guide bological research; my rejection of the “nosologo-mania”, i.e. the unfettered numerical increase of psychiatric “disorders”, have received little support and, again, too little attention.

You know, at the APA some years ago I organised a symposium on the DSM classification system and its validity or invalidity for biological research. It was a good symposium but, there were few key figures from the DSM establishment present and the turn out was only 50 - 60 people. That struck me. In my opening speech I raised the issue - how on earth could the turnout be so low. These question touch the very roots of our research efforts. If you have no diagnostic system that is reliable and valid you can forget the whole thing. I mean, in that case you can only pretend to conduct science, but you can hardly expect that your research will yield anything useful and reproducable.

Clinically, for daily practice, a highly inaccurate diagnostic system is perhaps still acceptable, because our treatments are so far not very specific, but for research it is fatal. If the psychopathological phenomena one studies, are not defined in detail and diagnosed in valid constructs, there is no basis for research. So not to be there to discuss those issues, I thought, was regrettable.

It was an expression of a state of affairs in the States, in which the DSM, as a system, as a diagnostic philosophy is the new holy cow. It is the successor of psychoanalysis. You cannot and should not question its fundamentals. You can discuss for hours whether the

duration of disorder A should be four months, or five months, or six months, or whether you should add a symptom or subtract another in a given definition, but the very roots of the system, the premisses on which it is based, are not open for discussion anymore. That attitude is extremely detrimental for the profession and for psychiatric research in particular.

It is fair to say, that so far in spite of 35 years, 40 years of intensive biological research, there is no single biological variable with any diagnostic significance. That might tell us something about the diagnostic approach we have taken - it should lead to reflection. How on earth could it be that nothing of diagnostic significance has been forthcoming. Okay it could be the crudeness of the biology that is to blame, but another possibility is that the diagnostic system is inept and that option should be taken seriously.

The change of culture from Europe to the US must have been awfully interesting because concepts like irritability and impulsivity are European concepts, indeed continental European concepts, that aren't found in the American or English literature. You also moved over with a background in Kraepelin and interest in psychopathology to a country that then had little interest in either nosology or psychopathology.

Well, it was just when I came that DSM III was introduced. There was a lot of protest against the new DSM concept but it was already introduced as an official APA document. But you see, there is an interesting American quality that is flexibility, ability to quickly change direction. It was fascinating to see how a continent that for years had been under the spell of non-nosological ideas - no diagnosis at all - the psychodynamic points of view, rather dramatically and quickly changed to a quite conservative neo-nosological approach. It was amazing to observe that a society could change so easily. From “no diagnosis at all”, a DSM diagnosis based on structured interviewing, was now considered to be mandatory, at least in the research community. America became more Kraepelinean than Kraepelin was, so to say.

The dimensional approach - again, I rather call it the functional approach - has not been accepted. Psychiatric diseases are what we study. One has to explore the antecedents, the epidemiology, the biology and what not of discrete and separable diseases. Diagnoses are being made by counting symptoms, preferably those that are easily observable, and those that are easily agreed upon by direct questioning of the patient. In the process of nosologisation of psychiatry, the need arose to objectify psychopathology to put a value only on that part of the spectrum of psychopathology that is clearly observable or that is clearly indicated by the patient - "yes I'm depressed, yes I'm very anxious" - rather than on experiental phenomena. Some years ago, in the British Journal of Psychiatry, I discussed the importance of reconquering that subjective domain by experimental psychiatry.

So apart from the non-acceptance of the functional approach, the tendency to neglect the subjective elements of psychopathology is a second reason I have to complain about impoverishment of diagnosing in psychiatry.

But there had to be a DSM III, didn't there?

Oh yes. The first step was right. Even if it's based purely, or mainly, on expert opinion there had to be a standardised, operationalised classification system. But we should have left it at that: at the DSM III; until all the proposed concepts had been thoroughly studied and validated or invalidated. There should have been no DSMIII-R nor a DSM IV, unless

based on solid data. What does, for instance, a concept like dysthymia mean. Is it valid? Is it distinguishable from major depression, from certain personality disorders, from generalised anxiety disorder, from adjustment disorder? Such validating research would have taken much time, much effort, much money, but it would have been the way to real progress. If I would have been the director of the NIMH, I said one day to Fred Goodwin, I would have invested most of the research moneys in that type of studies, not in biology. Again, without a detailed map of psychopathology, psychiatric research, particularly biological research, is easily led astray.

So when I came to the USA I experienced a change from a dislike of diagnosing, from a highly individualised way of describing and interpreting psychopathology, to a rigid nosological approach. The new psychiatry, however, is seemingly precise, but in fact we have introduced broad, heterogeneous diagnostic concepts, insufficiently validated and too much stripped of subjective paraphernalia.

You've said there is something of a nosological mania now.

Indeed, it is absurd. There has been an exponential increase in the number of diseases from DSM II, to DSM III, over DSM IIIR to DSM IV; the number tripled. What a nonsensical state of affairs. And then to think that you can study all of these entities, rather pseudo-entities, as discrete diseases, as if they were things in their own right, each of them having their own aetiology, pathogenesis, course and treatment. You know it's the clothes of the emperor again, a world of make believes.

We're almost back to the situation with Esquirol describing all the monomanias in the 1840s

Yes, mainly on the basis of a few symptoms one decides to the existence of and pushes for the recognition of a novel disease, a disease with its own particular course, biology, outcome and treatment. A research program is initiated, grant proposals are being submitted, patients recognizing the symptoms organize into lobbies. Soon the fata morgana looks like an edifice. I ironize a bit, but the essence seems to me true. So, after so many years of research I am to an extent disappointed. There are beautiful new methodologies; approaches; new psychotropic drugs; interesting biological data, such as the imbalance of the CRH/ACTH/cortisol axis in several patient categories, but their diagnostic value - if your diagnostic orientation is nosological - is little.

From the functional point of view, however, things look different. There is increasing evidence that biological dysfunctions correlate much better with psychological dysfunctions than with syndroms or the nosological entities, we distinguish.

On that point there seems to me that there's a major split opening up in a lot of the organisations at the moment, like the BAP and ACNP and possibily even CINP, where there's an explosion of knowledge within the neurosciences so that we've got 5HT-14 and D-35 receptors, but who knows what the clinical relevance of any of these things is. How do we hold the field together?

That is certainly something I see with trepidation. How many receptors can one study in humans - if you want to study the balance between serotonin receptors and their relation to dopamine and noradrenaline receptors, there are so far maybe 35 in total - that's almost undoable. That certainly is an embarrassing idea. Two points here: first, nobody has demonstrated yet that all these receptors are functionally active or that they are sufficiently different functionally to deserve a separate categorisation. It could be that for the

transmitter X the receptor A and B and C functionally form one cluster and D and E another cluster, so that you could reduce the number of receptors to be scrutinized. If that's not the case and that is the second point, the only way out is to shift our attention to post-receptor events. Those seem to be less heterogeneous.

Are you saying these get more molar again, almost more physiological again

No, I mean to say that we have to broaden our horizon. For years now we have been focussing on transmission per se and on receptor function. The latter field becomes more complicated every year, at least from the point of view of clinical research. The avalanche of new receptors is overwhelming. Perhaps we should broaden our efforts to include post-receptor events, trying to study second and third messenger systems. Here the number of possibilities seems to be more limited.

In biology things will become evermore refined and detailed. That is not the case in psychopathology. Clinically we are still working with very crude and imprecise diagnostic concepts, and the terrible thing is, we do not seem to realise that. The DSM system is regarded as a kind of endpoint, not as a primitive beginning. Discussions on how to refine psychiatric diagnosing are hardly ever heard. The gap between the degree of sophistiction in biological and psychopathological analysis is widening rapidly. That is devastating for biological psychiatry. The irreproducability of almost all genetic findings in psychiatry, for instance, is probable the consequence of diagnostic inaccuracy. We need new diagnostic conceptualisations, to even try to follow the biological express train and to translate the fruits of the neurosciences into new methodologies that are diagnostically and therapeutically meaningfull. In my book “Make believes in psychiatry” I have tried to generate a impulse to that end.

Earlier you hinted at the role of the pharmaceutical companies in playing down the personality issue. What role has the pharmaceutical industry, both in terms of wanting to produce drugs for a category that will give them a sufficient return on their investment and the insurance industry wanting to pay for treatments for which there are outcomes - how much have they, between them, brought about the current dominance of categorical points of view?

There is a strong reinforcement here. For insurance companies, for lawyers, for court authorities and the like categorical constructs, “diseases”, are easy to handle. One has a disease, that's it. The client is a schizophrenic, a depressive. But if you would say: "listen, there exists a “basin” that is called affective disorders, its not a diagnosis, it's just a global indication (comparable to the pronouncement: the person suffers from an abdominal disorder) and actually, this particular patient belongs in this category, but again, it is not a diagnosis; he has however a psychopathological profile that we can describe in detail and measure, though we cannot give it a specific name yet”: that's much too difficult for insurance companies, for lawyers, even for docters. So the DSM lobby is strongly supported by practising psychiatrists, by insurance companies, by lawyers, who all find it a neat, easy and practical system to structure their practices on and to ground the payment system.

Dimensional, or better: functional diagnosing is complicated and does not lead to easily transferable disease concepts, but it gives a much better picture of the clinical reality of a given patient.

You've reintroduced into the argument recently, ideas not heard since Adolf Meyer - the idea of reaction formations. Tell me about his role in your thinking.

The reaction formation I always felt is an extremely interesting and valuable concept. The idea, that there are a finite number of psychological domains that can be disturbed by biological or psychological or social stressors but not in a pre-configured way. The phenomenology of the response in a given patient, is very much dependent upon such things as the hardware of the brain, its present condition, on environmental circumstances, on personality structure and so what ultimately transpires in terms of behavioral and experiential changes varies very much from individual to invidual and within an individual.

The basic idea of reaction forms to me is that there is a noxious stimulus, biological or psychological in nature, perturbing certain brain-systems, but to a different degree; dependent on such factors as I mentioned a moment ago. Consequently a number of domains, psychological and physiological domains, are disturbed though to a degree that varies individually. Thus the composition of the ultimate syndrome cannot be other than very variable. The reaction form idea explains much better than the nosological premise, why so few patients fit the definition of a particular disorder, why there is so much so-called co-morbidity in psychiatry.

If I have to bet, if you ask me do you believe in nosological entities, the answer is no. Deep in my heart I think the disease concept in psychiatry is probably a fiction. The existence of a limited number of reaction forms seems to me more likely; for instance, the basin of the affective disorders, the basin of the psychotic disorders, the basin of the dementing disorders and a few others. The expression of these disorders in individual patients however is as variable as the shape of clouds in the sky. One recognises the cloud, but its configuration varies unpredictably. Attempts to uncover the cause of a particular reaction form, of a particular diagnostic “basin”, would be idle. To understand its biology one has to study the biological underpinnings of the psychological dysfunctions that make up that particular reaction form.

That is my belief system; but of course I keep wide open the possibility that I am wrong and that one day we will indeed discover the cause, the biology of, say, schizophrenia or some other categorical, diagnostic construct. But frankly, in that case I would be a very surprised man.

Let me try to phrase this viewpoint in another way. Take an entity like schizophrenia, considered to be a discrete disorder. In fact, it is as heterogeneous a concept as one can think of. It is a group of disorders characterised by a great number of psychopathological phenomena - e.g. delusions, hallucinations, cognitive disturbances etc. etc.- and the individual psychopathology varies widely. The degree of delusion, the degree of hallucination, the degree of inertia, the degree of cognitive disturbance is variable from patient to patient. The prognosis of schizophrenia is very uncertain. Some improve, some recover, some do not. The treatment response is also very variable. This state of affairs allows for no more than one statement, that is, that the group of schizophrenic disorders is a diagnostic basin with a wide variety of expression forms, some correlated with discernible brain dysfunction, others not and with very different outcome and prognosis.

Within that diagnostic basin of schizophrenia I could imagine the existence of a particular subform, which could be called a nosological entity because it is very tightly coupled to a very circumscribed brain dysfunction. This is possible. But in general, the reaction form idea appeals to me because it is of great heuristic value and my bet is that the view on

psychiatry as a compilation of independent diseases will eventually turn out to be untenable.

But apart from who is right and who is wrong, I want to emphasise again how extraordinary insightfull the functional approach to psychopathology will be. What does it mean to say “he suffers from major depression”? It tells you little about symptom picture, about prognosis, about functional abilities, about treatment response etc. If you could, on the other hand, provide an analysis of what psychological functions are disturbed in a given patient and to what degree, and which of them are still functioning within normal limits, the informative value of such a statement is much higher.

These data could and should guide psychological interventions and in the future, I hope, also psychopharmacology. You remember I have introduced and advocated the concept of functional psychopharmacology. It holds that psychopharmacology will move away from a nosological, towards a functional orientation, in which drugs will be prescribed not to treat the presumed “disease” ,but the basic, the primary psychological dysfunctions underlying a given disorder; much like the cardiologists are doing. They do not treat myocardial infarction as such. They treat the resulting cardiac dysfunctions and that with a variety of drugs. That is goal-directed, dysfunction-oriented polypharmacy; that is the direction I see also psychopharmacology develop. But, no doubt the denosologisation of psychiatry will be difficult because psychiatrists cling to the disease concept.

What the profession really want, is a few tumours, isn't it?

Actually the tumor metaphor makes a certain, though limited, sense. A tumor is not a discrete disease. It also is a “basin” of a variety of disorders with a few common clinical characteristics, such as the dysfunctional multiplication of cells. For the rest, they differ in degree of malignancy, growth rate, prognosis, treatment response, pathogenesis, possibly etiology etc. So indeed a concept like tumor and a concept like schizophrenia share the qualities of being a “diagnostic basin”. One can carry the analogy even one step further.

Apart from surgical interventions, tumors are nowadays treated with a variety of compounds slowing down cell division; those are in a way comparable to the present neuroleptic treatment of schizophrenia. The search however is for treatments geared towards elimination of the various catalysts of morbid cellgrowth. A comparable goal we pursue in schizophrenia research: to find and treat the neuronal processes underlying the key disturbances of a schizophrenic syndrome. I presume that those processes will not be the same in every schizophrenic patient, but that there will turn out to exist a variety of schizophenia’s, much the same as we probably deal with a variety of tumors.

Currently UK psychiatrists are caught on a hook. The government have set up targets for health gain and for psychiatry the target is to reduce the suicide rate from something like 15 per 10,000 to 10 per 10,000 by the year 2000, which seems impossible to many. You are one of the people who is most associated with suicide research and with the idea that it might be possible to predict the people who are likely to kill themselves. Do you want to tell me how you got into all that and how it evolved and where, if anywhere, you think it's gone.

How we got into this was as follows: from the beginning I had two research targets: schizophrenia and depression and from depression research we moved into anxiety research, simply because mood- and anxiety disturbances are so highly intertwined. It

was via depression research that we began to see the advantages of functional psychopathology or dimensional analysis for biological psychiatric research. We discovered that low CSF 5-HIAA in depression seems to be related to the anxiety component of depression. Serotonin disturbances occured in other diagnoses with increased anxiety as well. In the 70s, at a conference, we mentioned that. Then, the reply was: in that case the finding is just non-specific. That is a misconception, I answered. It is non specific syndromally and nosologically, but specific on a functional level, coupled as it is to the state of heightened anxiety.

A second derivative of our depression research was a growing interest in the biology of suicide and later also of aggression, simply because both are common in depression and highly intercorrelated. We knew, at that time, of course a lot of social and psychological predictors of suicide but nothing on biological predictors, i.e. on brain dysfunctions increasing the risk of suicide in a situation of unbearable misery. It was a novel research line in the 70s and it provoked an enormous resistance among antipsychiatrists. Suicide, they said, had absolutely nothing to do with biology, but everything with social conditions and perhaps a little bit with psychological make up. Anyhow, we initiated research into the biology of suicide, but Asberg et al in Stockholm were ahead of us. They found, as you know, a correlation between low CSF 5-HIAA and suicidal behavior. Later this correlation was also found in non-depressed suicide attempters.

I should like to add that the story of low 5HIAA and suicide in a way is a shame for psychiatry. The relation between low CSF 5HIAA and suicide was first published in 1976. You would have expected soon afterwards an avalanche of papers trying to confirm or refute this finding; even more so because Asberg et al had found that the variable CSF 5-HIAA contains predictive information. Low values of that serotonin metabolite seemed to increase the risk of suicide in the year after the index admission. Nothing of the kind happened. Only one repeat study, concerning the predictive value of CSF 5-HIAA concentration for suicide has so far been published. If an internist had published a finding predictive of, for instance, tumor growth, many papers would have followed soon afterwards. In psychiatry: silence was characteristic. That is a clear sign how non-research oriented, how non-biological the profession still is. Psychiatrists do not seem to believe that biological findings might have practical significance. If the Asberg findings had been confirmed on a larger scale, measuring CSF 5-HIAA should be routine in the diagnosis of multiple suicide attempts. Done skillfully, the lumbar puncture is a minor intervention, well worth the potential information it could provide. Search for less invasive methods with the same informative value would of course be indicated.

Who were the key people who influenced you ?

I have to answer you in an apparent haughty way. I have had no real guides, no tutors, at least not in biological psychiatry. When you are at the beginning of a new development, there are few people who can influence you. You have to create a domain for yourself, your own image, your own carreer, your own philosophy, your own methodology. That’s not to say that there are no figures in the field that I do not greatly admire. For instance, in the States, a man like Kety. He's a most personable man, a man of great intellectual power and great scientific standing. He was crucial for the development and the acceptance of biological psychiatry in the medical field, because of his knowledge, because of his diplomacy, because of his eloquence and his ability to phrase things in the right way.

People like Biff Bunney, Fred Goodwin and Bernie Carroll were brothers-in-arms in the early days of biological psychiatry, when it was still psychiatry's stepchild. And there were quite a few more. Some of them went to NIMH and made it into a very important institution that has influenced the profession very much.

There were more warriors, though initially not many. In England, for instance, I think that Alec Coppen was a man who from the beginning became a symbol of the importance of biology for psychiatry. In Germany, Hippius was one of them. In Switzerland Kielholz was fighting on behalf of psychopharmacology. He was prominent in WHO-circles and increased the respect for psychotropic drugs within the medical profession. He was not so much a great researcher but a statesman, a man who could really translate the importance of psychotropic drugs to the politicians and to the general public. Rafaelson has been important; he died too young. In the States, there was of course my old friend Ed Sachar and he too died too early. Those early years of biological psychiatry were fascinating. A bunch of bright young men fighting for the incorporation of biology in psychiatry.

Ole Rafaelson was another impressive figure professionally and physically. He was very much involved, in lithium research and bipolar depression. Copenhagen had an excellent metabolic ward and in addition he also had statemen like qualities and expressed them well at the WHO, where he and I established a Biological Psychiatry Program in the early 70s, in an attempt to get that field on the psychiatric map.

He seemed to have a feel for the need to bring on young people as well. Some of these high powered men don't always have that. They try to kick away the ladder. But he seemed to want to bring people on.

You're right. And so he was important not only as a researcher but also as a politician, in the best sense of the world. He was a charming man, also. Good looking, strong, a Viking-like appearance; in many ways he was a very visible creature. I should have mentioned also, apart from many others, Delay and Denniker. In the early days, they were heavy weights on the biological psychiatric scene; the proud discoverers of the neuroleptics, and they were acknowledged as such. Mogens Schou I forgot to mention; he almost single handedly "made" lithium. His professional life was shaped around that little ion. But let me stop here and just mention that many figures of importance were not mentioned.

Can I take you to some more personal issues. You've been in a concentration camp..

Yes, for almost three years. That was a crucial experience in my life. An experience of how low mankind can sink; of the ultimate failure of humanity, both of the perpetrators and those that failed to interfere. On the other hand, for me as a person, it has had, paradoxically, positive effects. The camp experience has made me, I feel, stronger, more mature, more hardened and more armoured against the vicissitudes of life.

But I rather like to mention another momentous episode of my life, that you did not touch upon. That is my visiting professorship in Jerusalem, in '76-'77. I went from Rotterdam to Groningen and from Groningen to Jerusalem, for one year. I had been invited to become Chairman of Psychiatry at the Hebrew University in Jerusalem. I seriously considered the offer. I worked there for a year as a visiting professor as a trial period. In the end I didn't accept; mainly because of the language. I felt so clumsy, not being able to speak, to

understand and to read the language. I thought, how on earth can I function here as a psychiatrist. Besides, my wife and children had not joined me because my oldest son was sitting for his matriculation. Facing linguistic difficulties alone, make them look even more insuperable.

So I didn't accept. If you ask me, are you happy with that decision, the answer is: not quite. Would I have been happier in Jerusalem than I have been in the USA and in Holland? Perhaps. I have been a Zionist all my life. Jerusalem meant a lot to me and it still does. I didn't mention it but one of my motivations to go to New York was the notion of New York as “little Jerusalem”. Einstein is part of Yeshiva University, being the only Jewish University outside Israel. So New York was partly a substitution for Jerusalem. Emotionally, being a Jew, I feel now, so many years later, it would have made more sense to stay in Israel.

I've been a Zionist all my life, but in 48 when the State of Israel was established I said: now you cease being a Zionist - either you go to Israel or you don't. If you don't, you are a supporter of the State, but not a Zionist. A Zionist is someone who goes. I never went for all kinds of reasons. I was in the Concentration camps during the War and when I came back I first wanted to finish secondary school. After that I said I first want to finish my medical study. After that I said before I go I first want to be a psychiatrist. After that the offer from Gronigen came to establish a Department of Biological Psychiatry and I decided I first want to do that job. So it never came to anything really, despite a lot of deliberations. Partly, it had to do with the language but partly I wavered for self-serving reasons. I realised that if you spent two or three years learning the language, you haven't got the time to do research. So that was also a point. Perhaps Israel would have been even more satisfying than New-York, because the country means a great deal to me.

Let me ask you something. Earlier in the interview, I was teetering on the brink of asking you a question I've asked one or two people, which is why has psychopharmacology been Jewish? Not completely but if you look at the names - Kety, Axelrod, Snyder, Kline, four of the ten Presidents of the BAP and a lot of other eminent people. Why is this? Can you tell me?

Well, it is not so easy to say. First of all, psychiatry has been a Jewish profession for many many years. Certainly psychoanalysis is. Also, it is not uncommon to find, relatively speaking, many Jews in new fields, both in the sciences and in the arts. Are we more ambitious than other people? Perhaps. Intellectual and later also artistic achievements were felt to be important in Jewish circles. Another factor could be that we have been a discriminated minority for so long. In that position, the urge to demonstrate that you are good, that you are not as bad as the discriminators think you are, that actually you are better than them, might develop, and among the Jews it apparently did. Besides, the description People of the Book is no slogan. For centuries, studying biblical books and commentaries was part and parcel of Jewish life. Not only of the rabbi’s, but of the common people as well. Nothing was taken for granted. Discussion, dialectics, weighing words and concepts is ingrained in the Jewish mind. Two thousand years of exile has thus been a thorough school of intellectual discipline.

The dialectical intercourse, the constantly challenging of each others ideas; that is for me the magnificent essence of Judaism. Few dogma’s are acknowledged. There exists no catechismus. Every statement is open for discussion. It is the power of argument that finally counts. In no other religion do you find this - if you read the Talmud one Rabbi claims this , but is immediately challenged by another Rabbi, the latter by a third and so on. That scrutinizing orientation could have been inducive of moving into new fields,

towards new horizons. So I think there are a number of reasons that Jews are attracted to the yet unknown, to domains still to be discovered. Psychopharmacology and the area of brain and behaviour is just an example.

I think it is an interesting point that here we have an Irishman asking a Jew this question. Brendan Behan, an Irish playwrite said that all nations have a history but the Jews and the Irish have a psychosis. The interesting thing for me is that, whereas the Jewish response to this, at least in the psychiatric area has been to try explore the frontiers, the Irish response has been the opposite. Until quite recently we more or less denied that we had a national mental health problem, even when we had more beds occupied per head of the population than any other nation on earth has ever occupied. It seems extraordinary. That two people can actually respond so completely differently..

The Jewish people are Jewish, the Irish people are Roman Catholics. Catholics have never promoted the study of the Bible, whereas the Jews, particularly after the destruction of the Temple, became more and more obsessional about studying the Bible and text exegeses. That has been an extremely powerful tool to sharpen intelligence and to encourage voyages of discovery; constantly asking questions about what God meant, and argue about the different interpretations. Catholics discouraged study of the bible by the common people. Jews in the majority, have been literate for thousands of years. That was not the case with many other people. Jewish rabbi's, encouraged critical analysis of data. Intellectual curiosity is probably the reason that Jews so much like to explore frontiers.

What about the camps. Do you want to mention them?

What should I mention. I was there for almost three years. That was one of the reasons for that chapter in Make Believes in Psychiatry, on the children of holocaust survivors. The way I read the literature, is that it is completely unproven that holocaust survivors failed in the upbringing of their children. Even for the holocaust survivors themselves, it is unproven that a large percentage decompensated afterwards; were mentally crippled. I think it is a shame that such ideas were ventilated. It is typically a psychoanalytic point of view that after such a trauma, such an existential nightmare, one can not be able to live a normal life and one should be unable to raise one's children properly. The facts do not speak, the theory prevails. The theory demands that many people should have been crippled, so never mind what the facts tell. There is, however, very little good evidence for lasting holocaust-induced mental pathology in the survivors as a group. Completely disregarded is the evidence that, thank God, a large percentage of the few survivors have done quite well and were very well able to raise their children in a decent and reasonable way.

I finished that chapter by saying that soon, no single survivor will be around anymore to say just that. I added that in spite of all the horror, the notion that for some the camps have been an ego-strengthening rather than weakening experience should be seriously considered. In my case, I have a strong feeling that it changed me profoundly, but not for the worse. This might sound paradoxically. You could suggest that I survived because I was strong. But I don't think so. I believe the camps have made me more defensible, increased my fighting spirit; it also augmented my contendness with what life has to offer; it instilled a measure of material modesty. The expectations I have of my fellow citizens are also low. Probably also a result of war experiences. I like people, but I do not expect much of them; that lowers the risk of disappointment.

So, if you ask me did the war influence the rest of your life negatively, my answer is: I don't think so. The stereotypes about concentrationcamp survivors I thoroughly regret. It reminds me of the terrible things psychiatry did to the mothers of schizophrenics and the mothers and fathers of austic children - you know blaming the parents for the misery of their children. The idea that the holocaust survivors were so bad for their children and reponsible for personality deformations in the second generation, is based on a similar theory-driven callousness.

Until World War II, German and middle European psychiatry was the world force. After World War II, it was UK and US psychiatry. Is that because so many Jewish and other intellectuals migrated from Germany and middle Europe. This seems to me to be an obvious explanation although not everyone I ask agrees with me.

There was an enormous brain drain from Germany since 1933, in physics, in medicine including psychiatry and in the belles-lettres. Many of them were Jews, many non Jews, but for obvious reasons the Jews were, relative to their total number in the majority. Many went to the USA. I think Germany suffered greatly and until this very day has never regained its pre-war position - in the arts, in literature and in the sciences. I think the Americans would say: in the 30s we were graced with manna from heaven: a cohort of immigrants from Europe who enriched the country enormously.

So things have changed since you went into psychiatry first

It was indeed a totally different profession, when I started in the 50s. It has developed, notwithstanding all the criticisms one might have, into a scientific discipline. When I started there were hardly any drugs. There was one form of psychotherapy - psycho-analysis - often watered-down psychoanalysis. Indeed, I didn't mention it, but I witnessed a second revolution in psychiatry: the development of other, non-analytical psychotherapeutic strategies such as behaviour therapy and cognitive therapy and furthermore the de-individualisation of psychotherapy; the inclusion of more than one individual - group, family or spouse - in the therapeutic process. When I was a resident, it was a crime of sorts, while treating a particular patient, to involve his or her spouse or children or any other important other. Not even for diagnostic reasons. That was considered to be psychotherapeutic malpractice. I could never understand the reason why. I thought, people are living together; why to isolate them in treatment? Family and group therapy for me were a refreshing revelation.

So in terms of diagnosing mental disorders, of biological treatment, of brain and behaviour research, of psychological interventions, of epidemiology, of genetics, there has been so much going on in the profession in the past 35 years. I feel fortunate to have lived in that particular period and proud and gratefull to have been an active participant. The combination of being a teacher/clinician/researcher, moreover, offers satisfying compensation in times one's research is not going so well. Diagnosing and treating patients, teaching students and supervising residents are creative activities in their own right. We, clinical researchers, really enjoy the best of every aspect of academic life. And finally, permit me the platitude, research is important. If your daughter, your spouse, your father becomes psychiatrically ill, you thank God that there have been researchers, that there is development, progress, that there are pills, treatments and even preventative measures nowadays.

That's a very upbeat note on which to end but given all you had to say about DSM-III, I can't resist citing the quote at the start of your book *Make Believes in*

Psychiatry **from Barbara Tuchman's** ***March of Folly*** **to the effect that a folly is something that is perceived to be counter-productive in its own time and not merely by hindsight, that alternative feasible courses of action should have been available and that the policy should be that of a group rather than just of an individual.....**

References:

van Praag, H M (1993). "Make-Believes" in Psychiatry or The Perils of Progress. Brunner-Mazel , New York.