

**A MANUAL FOR DIAGNOSIS AND STATISTICS
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DSM III really was a watershed. I'd be keen your take on the issues and to throw some angles at you. One of the events that seems to me to have triggered off the process out of which DSM III ultimately came was the Williamsburg Conference in Virginia 1969, which the NIMH set up.

Right, I wasn't there. Eli Robins was at it and at this meeting he proposed his classification for depression into primary and secondary depression.

That's right but they also seemed to have the idea of testing Joe Schildkraut's catecholamine hypothesis. Then they began to realise to do this you've got to have operational criteria for mood disorders etc.

Did they recognise that they needed operational criteria?

Well its not said that way in the actual papers of the Williamsburg Conference but I think it was that group of people beginning to talk about it who began to recognise the issue.

I wasn't there but my impression was that was the beginning of the recognition that we needed different sub-types - we couldn't just talk about depression. I don't know that operational criteria came in yet.

What were the steps from sub-types to operational criteria?

Well okay. You're familiar of course with the Feighner criteria. The way I got into the Feighner criteria was through Marty Katz who was at NIMH. He organised the psychobiology of depression study, which is actually still going on - I think they are in their 28th year, it's amazing. I knew him from meetings at ACNP and he knew that I was into rating scales and structured interviews with Jean Endicott. It was his idea to bring Jean Endicott and myself, because of our expertise in structured interviews, together with Eli Robins to help develop a way of comparing different diagnostic systems. I mean that was the Psychobiology of Depression project.

When did he approach you?

It must have been 71 or something like that because work started on DSM in 74 - you'd have to look at the RDC papers. Williamsburg was 69 and it was after that, so it must have been 70/71. Now prior to that time I had no particular interest in psychiatric diagnosis.

Didn't you?

No, I was working with Joe Zubin. I was a resident at the Psychiatric Institute and I met Joe Zubin because the Institute, like all State Hospitals, had dining rooms where the staff had these wonderful meals served to you. So, when I was a resident I got to meet Joe Zubin, who was a senior research person there. I wanted to get into psychiatric research and he encouraged me to become a Fellow. He hooked me up with Gene Burdock, who was a psychologist in his department. Gene was working on a structured interview to interview nursing personnel in State Hospitals about the behaviour of their patients. It was in the days of psychopharm studies and behavioural checklists. So I saw him do this interview of nursing personnel about patients' behaviour and I got the idea why couldn't you do a mental status with a

structured interview. I didn't realise that John Wing had already been doing that. So I started on structured interviews. Then I hooked up with Jean Endicott and we did the Mental Status Schedule, which never took off. So I had not been interested in diagnosis, I was just interested in structured interview of mental status.

That's interesting because you've often been portrayed as a person that was pulled in by the APA because you were interested in diagnosis.

Oh but that's later. I'm just saying when Marty Katz got me to meet with Eli Robins I had no particular interest in psychiatric diagnosis. I was interested in structuring the mental status. So I then met Eli Robins and the job was to make improvements on the Feighner criteria because the Feighner criteria didn't subdivide depression. So with the RDC we had all these various sub-types of depression and then to fill it out we updated the Feighner criteria and developed criteria for panic disorder, agoraphobia and what not.

When you met Eli did he give you any feeling for why they'd gone down this route of the Feighner criteria? Did he talk at all about links back to Mandell Cohen?

Oh yes sure. Now when I say I had no interest that's not totally true because I had some involvement with DSM II. And again it was through the dining room at the Psychiatric Institute. Ernie Gruenberg was the Chair of the DSM II Committee and in 1967 he came to me, as a research fellow in Joe Zubin's department, and said we're finishing work on DSM II and we need somebody to take notes at Committee Meetings and may be help in editing some of the stuff, would you be interested? I said to him well is there any money involved and he said no there's no money. So I had to make a quick decision and I made the right choice – not to worry about the money.

So I joined the committee as they were finishing up work on DSM II. I wrote a little article introducing DSM II, which appeared in DSM II. So there was some involvement with the APA. But I didn't have any particular research interest in diagnosis. I mean that's as far as I'd gone until I became involved with Eli Robins which was a few years later in 1971. So that started the RDC.

Then being a member of the DSM II Committee, I had another chance event. I became interested in behaviour therapy. It had nothing to do with what I was doing professionally, I was just interested. I was attending a meeting on the behavioural treatment of homosexuality and the meeting got broken up by some gay activists. As the gay activists were leaving the meeting I started to talk to one of them and I told him I didn't like them breaking up the meeting. The speaker, the poor guy, had come all the way from Australia to present his findings and he couldn't because the meeting was broken up. In talking to this gay activist, I mentioned that I was on the DSM II Committee. He got very excited and eventually asked would it be possible for his group to make a presentation to the DSM II Committee because they wanted to remove homosexuality from subsequent printings of DSM II.

I went back to the DSM II Committee - I was just a junior member of that committee - and I told them what had happened and asked what should we

do. Should we arrange to do this thing? They felt there was no way to avoid it. So a group of three or four activists came and they made this long presentation. When they'd left, the head of the DSM II Committee, Henry Brill, came to me and he said "okay Bob you got us into this mess now what do we do?"

So I got the idea of organising a symposium at the next APA meeting on homosexuality - should it be eliminated? I went through a long rethinking of my own views. I was attending the Columbia Psychoanalytic Centre, which was very homophobic. Actually within psychoanalytic groups, it was one of the groups that was most interested in treatment. They believed that homosexuality was always a very serious pathology. Anyway, I finally came to the conclusion that it should be eliminated. I wrote a position paper. There's a whole book on this written by Ron Bayer "Homosexuality in American Psychiatry", which came out in

Ron is a very good friend of mine. We've stayed friends. But anyway because of that I became known more to the APA leadership. In the DSM II Committee we would meet once or twice a year because there was nothing very much to do in the intervening years following DSM II. We became aware that work was starting on ICD 9 - they started sending us drafts and their timetable was that ICD 9 was going to come out in 1979. So it seemed to me that it would be useful if we started work on DSM III and I got the idea that I would like to do it.

I started trying to suggest this to APA people. Brill was not very ambitious. He was an elderly guy who ran a State Hospital and he had no interest in the thing. I started talking to the APA people about DSM III and being in charge of that. Their attitude was well we can't fire Brill, he's the Chair of the Committee. We have to wait. It turned out that Brill moved to another job and so he resigned in 1973. Now that year Judd Marmor was the President of the APA. Judd Marmor was unique in American psychiatry in that he had publicly taken the position that homosexuality should not be considered pathology. So he knew of me in the 1973 homosexuality controversy. By that time I guess I also had some reputation as a researcher but I think he mainly saw me as somebody who knew the DSM Committee stuff. You have to realise also that at that time it wasn't regarded as that important a job. So when Brill resigned, Marmor became the President and the President elects the heads of these committees and he elected me in 74 to chair the committee. So that's how it happened.

What about the influence of Mandell Cohen on Eli and Wash U – how did that feed through if at all?

Well I only heard about Mandell Cohen. I never met him. Is he still alive?

He is apparently. He is meant to be at the APA meeting this year he's getting a life time achievement award. He's 92 or something like that. No, I got to know Eli. We hit it off and I spent a bit of time with him and Lee Robins.

How important were Wash U in all this? Would the same thing have happened if they hadn't existed?

I don't think so. You know so many good things you wonder why didn't I think of that. The whole idea of diagnostic criteria is such a simple idea and it was so clearly evident but no one else picked it up.

This was a point when these things were being referred to as the Chinese menu approach.

Well I think that term was used by Gerry Klerman but he was actually very sympathetic to it. He was always self-mocking. But if there had never been a Wash U, what would have happened? I don't know. What would have happened, if there hadn't been me? Would Wash U just have been a little renegade outpost? I assume DSM III would have been like ICD 9. DSM II was actually the same as ICD 8, there were just very minor little changes. I guess eventually people would have recognised the need for diagnostic criteria but I don't know. I like to think that having been there made a big difference. But I don't know.

So the RDC criteria came out first and you built on those?

People ask when you started DSM III how much had you already thought through about what was involved. The answer is I hadn't thought through anything. I mean I just knew it was a wonderful opportunity. I didn't know it would become as important in terms of American and World psychiatry. But I quickly just assumed that we were going to use diagnostic criteria. The interesting thing is there was hardly any controversy about that.

There was only one person in the DSM Taskforce who didn't see it that way. Of course I picked the DSM Taskforce. Now that's another thing that's kind of interesting. In those days since it wasn't regarded as that important a job, they just let me, the Chair, pick anybody I wanted. So I picked everybody that I was comfortable with, which was very heavily influenced by St Louis people. I had Nancy Andreasen and Bob Woodruff and Don Klein and what not. When it became clear that this was going to an important endeavour, the psychoanalytic community got very uptight. Herb Pardes headed a delegation that came to me in 77 or something like that and said listen your committee is not balanced. You don't represent American psychiatry. You need to have some psychoanalytic input. So we asked Bill Frosch and John Frosch to join the committee. John Frosch later quit. But they both had some interest in psychiatric diagnosis.

The point I'm making is I immediately assumed that of course we would have diagnostic criteria and everybody else on the committee did with one exception, which was Bill Frosch. Bill Frosch thought that we ought to have a research criteria version for research, which in some ways is the way the ICD have gone – they have separate research guidelines. But there was really no controversy about it. When you ask what is the decisive thing that DSM did, well there's diagnostic criteria, which even the analytic people didn't really object to. What they objected to was the elimination of neurosis as a concept. Because they felt that really was an attack on Freud. The whole descriptive grouping together of the disorders is what made them go antsy.

They never complained about diagnostic criteria, so in a way I think it was maybe a paradigm shift, an idea that you just couldn't argue against. You might argue about a particular diagnostic criteria. You could make fun of it as Chinese Menu. But the idea of the need to operationalise what you meant when you said somebody had schizophrenia, nobody could say that was a bad idea. No one could argue its better to have just a few sentences like DSM II had. But they did argue about eliminating neurosis.

You seemed with the two major groups, schizophrenia and depression, to have done almost the opposite thing. You moved from a fairly loose vague concept of schizophrenia to a more restrictive Kraepelinian concept. But in the case of depression you moved from types of depression, such as endogenous depression, to major depressive disorder, which was loose and amorphous and heterogeneous. Joe Schildkraut would say you did the wrong thing for depression, how can we ever find a biological marker for this.

Well we retained melancholic sub-type but nobody cares. No you're absolutely correct. Well in doing that of course we were pretty much adopting the St Louis approach. The St Louis approach said there's this thing called major depression and the primary secondary distinction is important. We dropped the primary secondary distinction because that just didn't make much sense.

No one really understands do they?

Oh I think they can understand it but it doesn't seem to help - you know whether a depression comes first or after, which was the basis of the primary secondary distinction, doesn't seem to make much difference. But the other interesting thing was we also dropped the reactive non-reactive or reactive endogenous breakdown as a fundamental distinction. Now the reason for that was that there seemed to be a consensus that once a serious depression started it didn't make much difference in terms of treatment whether it was precipitated or not. So if you eliminate reactive depression and if you don't have unipolar or bipolar, all you have is major depression. And the actual criteria are almost the same as the Feighner criteria. They had one month, we had two weeks.

When you drew up the criteria for mood disorders, this was pre the SSRIs and the explosion in mood disorders we appear to have. A lot of the nervous problems out in the community were still seen as anxious conditions being treated with the benzos or behaviour therapies. You can't have anticipated really the way major depressive disorder later suited the industry. Did that come as a surprise that you could look at the major depressive criteria and using the same clinical features but in milder forms you could begin to say well half the community is depressed?

Yes. Not quite half. When we do primary care studies, which is what I've gotten into recently the figure is up to 25%, which just seems incredibly high. The whole disability criterion actually is an interesting issue, which has come up in DSM IV. There they've added a clinical significance criteria where they

try to get disability or distress as a requirement. I actually think this is a very bad idea. I think the WHO idea, which is that you have a syndrome and phenomenology and you make that judgement independent of disability makes much more sense. But I guess we didn't anticipate that major depression would be found as commonly in the community. Essentially what we were doing was we were saying okay the Feighner criteria identified 9 common symptoms, if you have 5 of them for nearly everyday for 2 weeks or a month that's it. I mean that it seems clinically reasonable.

Yes it did when you actually drew the criteria up.

Does it seem less reasonable now to you?

In the sense that I think you were dealing with depression largely from hospital plus clinics linked to hospitals. Now at least within the UK most depressions we deal with are primary care depressions. Whether they should be called major depressive disorder rather than anxious depression is unclear to me.

Sure, there's no doubt they're milder. I think Sir Aubrey Lewis says depression can be acute, chronic, recurrent, episodic and there are all kinds of intermedia - that's my view. I don't know where you would put the dividing line. There have been studies that looked at does it make a difference if its five symptoms, four symptoms, three symptoms and its just a straight line with more disability and impairment the more symptoms you have and the longer the duration. So I don't know how you would cut it. I think you have to have some kind of a syndrome to call it major depression but to me major depression just means you have a relatively full syndrome of many significant symptoms occurring together with some reasonable duration. The four weeks of Feighner seemed for a clinical manual a little too long. If somebody is acutely ill, the idea you have to wait for four weeks doesn't make much sense.

Let me hop back to somewhere around 72 or 73, the term neo-Kraepelinian appears. Gerald Klerman again was it?

Yes that was Gerry Klerman. I actually resigned from the neo-Kraepelinians. He wrote some article some place in which he said here's the credo of the neo-Kraepelinians and somebody mentioned me as being in support of this. I replied with a kind of humorous thing that this implied a biological etiology and I said I don't subscribe to any particular etiology. As far as I'm concerned I'm totally neutral.

What you begin to get out of the neo-Kraepelinian idea was people like Roger Blaschfield writing about invisible colleges. Things weren't as collegiate as that you're saying.

Oh no, I think they were very collegial.

In the sense of friendly yes but in the sense of some kind of conspiracy - people with a common agenda?

Well I think there was a common agenda.

An agenda for diagnostic clarity but not biological etiology necessarily?

Well if you think of the Kupfer's, they're a nice example and even Myrna Weissman, she's a good example of someone totally committed to considering both psychological as well as social factors. The pure St Louis group they're not big on psychology but I understand they're also very sceptical about treatment. I was told that Eli Robins didn't believe in the efficacy of most of the drugs at all. But I think neo-Kraepelinian's meant people who were certainly not analytically oriented and they had a heavy influence in biological studies like Sam Guze. But to me the neo-Kraepelinian's just meant that psychiatric disorders should be considered medical disorders and that the techniques used in medicine, epidemiology, laboratory studies, family studies were appropriate. That's all the term I think really meant.

But the coining of a term like that often has political implications. It does get a kind of band-wagon rolling. People think they know what's going on once this group got called neo-Kraepelinian. They had an icon. Let me ask it in a slightly different way then. Okay we've got St Louis having an input. We've got ...

The Psychiatric Institute with Don Klein and Jean Endicott.

Gerald Klerman what was his role because he seemed to...

He actually had very little. He didn't have a major influence on DSM III. He was on the DSM IIIR Task Force but that committee didn't really do very much. But he was a very important public spokesman and he had tremendous influence on a large number of people – Myrna, Bob Hirschfeld. And he very often articulated the view of the need for controlled studies and to get away from speculation.

When the Upjohn work along with Alprazolam, Gerry portrayed it as he was using Upjohn in order to get the idea of operational criteria out into the popular culture, to get a big trial going in using this kind of thing to sell DSMIII at the same time. Is that how you saw it?

I didn't see it as selling DSM III. The concept of panic disorder was a DSM III contribution. But I never saw it as a DSM III thing more of an American contribution. It represented I guess a splitting tendency. DSM III is on the side of splitting and of course panic disorder is the ultimate splitting of anxiety into panic disorder and generalised anxiety disorder.

Clearly DSM III was going to have an influence because it became important for researchers etc.

Everybody except England. England ignored DSM as best they could. Michael Shepherd wrote a book review of DSM III. I met him it later before he died and I tweaked him. I said do you remember that book review. He said what did I say. I said the last sentence was serious students of nosology will continue to use the ICD 9. He was dismissing DSM III as not a very important document.

Yes I think that was the UK view.

Oh sure but I think there was a lot of envy. We were treated in the US/UK study, which Joe Zubin had organised - I was a very junior auditor in that – as second class.

How much of an impact did the US/UK study have?

Oh it had an important impact in that it showed that differences in national trends were due to different conceptions of what the diagnosis meant. And that inevitably led to justifying diagnostic criteria. So yes that was very important. But also we were treated with total contempt by the British psychiatrists.

You were the guys who couldn't make a diagnosis.

We didn't know how to make a diagnosis. John Wing and Cooper and those guys. So I think when DSM came along they were still dismissive. And of course they had total control of the ICD. The ICD always looked towards the Maudsley trained guys. Even Sartorius was trained in the Maudsley. They'd go to Michael Rutter when they want to know about child psychiatry. So they were totally contemptuous and I think it was a tough pill for them to swallow.

So Brits had got hold of World psychiatry in a sense and they were losing an empire?

That's right.

How important then was Joe Zubin's work in all of this. Because he was linked into the International Pilot Study of Schizophrenia.

Well he organised the US/UK study obviously. He had no direct impact on DSM other than that I was in his department. He created an atmosphere in which I could work with Jean Endicott but I never even wrote a paper with him.

Jean Endicott can you go through her role in all this.

We started working together. I had only been in biometrics a year or two before she joined. We worked together very closely on RDC up until the beginning or towards the middle of DSM. Then she wanted to go her own way and she now has her own research unit. So she helped somewhat with DSM but primarily with the RDC and the SADS and she's retained the SADS.

As regards the diagnostic interviewing how did all of that evolve?

Well it evolved again from Marty Katz bringing me together with Eli Robins and needing an interview for the psychobiology program. That led to the Schedule for Affective Disorders and Schizophrenia - SADS.

They had their own schedule?

They had the Renard yes, which made Feighner criteria. But the job of the SADS was to make diagnosis according to RDC. So that was the SADS and then developed its own independent life. After the NIMH sponsored the DIS, they decided they wanted a NIMH sponsored clinical interview that would be comparable to the DIS. So they put out an RFP and we got that to do the SCID, so the SCID became the NIMH approved version of a clinician

administered interview, which is competitive with the PSE which interestingly never took hold in the United States.

It hasn't really taken hold in the UK either. During the 80s yes but its not used much now.

Well I guess it's because it's got so complicated. It's probably very good. Wing is so brilliant, he's so smart. I have tremendous respect for him. But I must say I'm not unhappy that the PSE didn't make it. Its amazing the SCID is just accepted as the gold standard in American research.

To come back to DSM III, circa 1976, things became to get hot and sticky. You've got people like Otto Kernberg saying Bob Spitzer has this not so latent hostility towards psychoanalytic ideas.

Well Ron Bayer wrote an article, which I co-authored with him on the dispute about neuroses in DSMIII and that appeared in the Archives of General Psychiatry. That gives the whole story.

But how did it actually felt in flesh and blood terms?

Well what it felt was, you had all these people who prior to DSM III hadn't the slightest interest in the DSM. In fact, there's a guy called Irving Biber, an American psychoanalyst who did many of the psychoanalytic studies of homosexuality. At the Hastings Institute, they had a meeting about 10 years ago, where they brought people who had been involved in the 1973 controversy about homosexuality. They brought Biber back and me to recount that dispute and Biber told this wonderful story of how in 1973 a colleague called up and said Irving there's a terrible thing that's about to happen. They're going to take homosexuality out of DSM II and he said what's DSM II. He had no idea. Even the name didn't mean anything.

So here are all these people who previously couldn't have cared less and they now were getting scared because they realised this was something that could really affect American Psychiatry and their whole authority. They had been regarded as the people who knew the in-stuff by residents and everybody else. And here comes this man who all but says neurosis is an old-fashioned etiologic hypothesis and they felt very threatened. They were angry, they were just furious.

The American Psychiatric Association kept appointing committees to meet with me and to try and work out our difficulty. One of them I remember said was you know you look at DSM III graphs and he said we know so much more that you don't have - meaning that we have a psychodynamic understanding of all of these things. A lot of what I had to do was diplomacy. I had to meet with these people. I came up with all kinds of ways of muting their concerns. At one point, since we were getting rid of the word neurotic anxiety we put it in parenthesis in DSM III. Then I also had a proposal that they could qualify a disorder as being neurotic etiologically, due to an intra-psychic conflict. Some way to make them feel more comfortable. But eventually all these things meant nothing. When DSM IIIR came around we dropped the parenthesis. Can you imagine that somebody would now say obsessive compulsive neurosis? It would just be absurd. I think history has proven what we did.

How much did it get down to politics in the end with people on the DSM III task force becoming critical of you making too many political concessions?

I don't think that was the case. I was the guy out there, the spokesperson, I think very often on committees people resented me. They may have felt I was too tyrannical, bossy because everything did go through my fingers. Outside of our group there was a lot of feeling that we had taken over and who asked us to do that. We were supposed to represent American psychiatry and we clearly didn't. We were always doing end runs. Allan Stone was the President of the APA and he once later said to me that I was like a magician. When I would appear in front of the committee, I could just get my way by sweet talking and what not.

The issue about it being a research manual was one thing but really what sold it has to be the fact that it got virus-like into the financial system as it were.

When people talk about why the thing became so popular, that's a very interesting issue. People usually say the reason it became so popular was because of diagnostic criteria but I don't think that's the reason. I think the reason it became so popular is that we were inclusive. We added diagnostic concepts that people were using that had not been recognised by the traditional systems - like borderline personality disorder. Here is a concept that a large number of clinicians really thought was important in their work and the ICD didn't recognise it. DSM II did and we did. Post Traumatic Stress Disorder etc. We took the attitude that we would include anything that seemed reasonable, that we could make some attempt to operationalise. I think clinicians felt their language was now expressed by the DSM III, which it had not been by DSM II. And I think that's what made it successful. We were inclusive and adopted the language that the people were thinking in and were comfortable with. We also had bigger descriptions and I think the descriptions were pretty good. I think clinicians felt when they read these descriptions and they looked at the criteria, it had face validity. That's what it amounted to.

So you don't take the point that was put by Ming Tsuang at this meeting that you guys went for reliability and sacrificed validity..

But that's silly because we never knowingly sacrificed validity. What is the validity he's talking about? People have eye tracking movement abnormalities but they don't have any impairment. They may be part of some schizophrenic diathesis but you wouldn't want to give the diagnosis of schizophrenia to somebody who has no symptoms but just has eye tracking abnormalities. This whole notion that we only gave attention to reliability I don't think holds. We often added categories like narcissistic personality disorder. Why do we include that? We included it because the psychoanalytic community said this is an important concept. It's not reliable. I don't know if it's valid but it's not reliable. We included it because we wanted to be inclusive. We didn't want anybody to feel that their diagnostic concepts were being excluded.

Occasionally we would meet with a group, the example that I often give is a group of child psychiatrists. There was the concept of the Atypical Child and some child psychiatrists said we want DSM III to have the Atypical Child. I said fine tell me how you would describe it. They said well it's very hard because there are a lot of different things. We finally concluded there was nothing there. We couldn't define it.

The other thing you hear from Joe Schildkraut or Ross Baldessarini or Sam Guze - a wide range of people - who say yes operational criteria are great but where it breaks down is if you get three or four experts to do inter-rater reliability for particular cases. Then if you take 100 people who have got psychoses, we'll put 15-20 into the schizophrenia bin and 15-20 into the bipolar psychosis bin and 60% will have psychoses but we're not really sure which one they've got. We can achieve wonderful reliability provided we're allowed to say well we're not sure which psychosis.

Right. There's always an NOS, a not otherwise specified category. There's always in DSM been a residual category for one you can't make any specific diagnosis. Maybe I'm not understanding what the question is.

Perhaps I'm not understanding why everyone else then has a problem but people don't use NOS very much. There seems to be pressure to be more specific.

People want to avoid that NOS.

Is this is where you get the argument about its reliable but its not valid or whatever. I guess someone like Sam Guze would say that if we're going to use...

Oh know I know what Sam Guze would say. He would say there are too many categories.

But if its being used for insurance and reimbursement purposes you can't be too purist can you?

No, insurance issues had no effect. It never came up in discussions. The insurance people were always happy with DSM II, they never complained about it. I suppose they like DSM III, maybe more, but there was never any way in which we tried to fashion the manual to make it more acceptable for insurers.

There's a big article in the American Journal of Psychiatry on DSM III about 1990 by Mitchell Wilson supposedly looking at the history of the politics of DSM III and he says really it did play into the insurance industry and pharmaceutical industry hands but you're saying no.

Absolutely no. I mean in what specific way. Some people would have said that with panic disorder we were trying to make Upjohn happy. That's not why we had panic. We had panic disorder because Don Klein convinced us that it was different from generalised anxiety disorder. Alprazolam was never part of our discussion and I don't see how psychopharm would have influenced the DSM. I know it never was part of any discussions.

But if you're getting to critiques, Sam Guze would say that there are too many invalidated categories. We were talking about having borderline personality disorder and I remember Don Goodwin in St Louis on the DSM III taskforce, saying as far as he was concerned borderline personality disorder was a ridiculous idea but who cares. If the psychoanalysts wanted it and we could have operational criteria for other disorders then let them have it.

How do you see DSM IIIR and IV compared to DSM III? There is the introduction of comorbidities in DSM 111R and DSM IV becomes much more consensus by committee.

I mean that's a whole thing in and of itself – DSM IV and Allen Frances. The whole thing was run very differently. I was involved in all aspects of DSM III and DSM IIIR. Allen Frances didn't want that kind of involvement for himself, so he pretty much let each committee do its own job. Occasionally he would get quite involved. He also decided every committee has five people no more. I had some committees, like the personality committee, which had 30 people because I believed if there were people I could work with who were helpful include them. The DSM IV group wanted to avoid controversy. They felt that the previous controversies that I'd gotten involved in, like self-defeating and sadistic personality disorder, was bad for the profession so they tried to avoid that.

I think they encouraged some decisions like should pre-menstrual dysphoric disorder be upgraded from the appendix to a bona fide category. My own view was that the evidence was pretty conclusive favouring it being upgraded. I think Allen Frances engineered the whole thing so that it stayed where it was because he didn't want the feminists to give him grief. There was also the issue of clinical significance, which had a far reaching influence in the manual because they added this criteria to about half of the categories. The taskforce never discussed it. It was a last minute decision that Frances made - he just did it without it really being discussed, which I think is pretty outrageous.

They presented DSM IV as being more user friendly - that was the advertisement. A few of the criteria sets are a little simpler like somatization disorder and anti-social personality disorder but most of the criteria sets are actually a little more complicated. I think in some ways it's gotten maybe more complicated than it need be. There's a whole thing on acute stress disorder, which I think was handled very badly. But you know it's also a personal thing because I wasn't in charge, although I would have loved to be in charge of it. It took me quite a while to get over not being there.

Was there any reason why you weren't involved?

Oh yes there were a lot of reasons. First of all I'd done it twice before. It had become very much personally identified with me and I think there were many people who felt it would be better for American psychiatry that the thing should move on to somebody else. In addition I think there were some people you know who didn't want me in particular. Frances made sense - he had been involved in DSM IIIR and he had some credentials in that way. We didn't get along well and there was a lot of bitterness. That's just the way these things go.

There's two aspects to the DSM process one is it's become financially important to the APA and the other thing it's become a cultural event. No one noticed the publication of DSM II but DSM III, IIIR and IV have actually become cultural events in that even the mainstream media takes notice.

It is amazing. I guess it defines things. Why do people get so upset when they have arguments about diagnosis I guess because it defines what is the reality. It's the thing that says this is our professional responsibility, this is what we deal with. Once it means something it's bound to have importance. If it doesn't mean anything like DSM I and II, then it doesn't have much importance.

And then of course, there's the reimbursement issue. But when people talk about DSM III becoming popular and part of it being due to insurance reimbursement, actually DSM became popular immediately to everybody's surprise, where there was no change in the insurance reimbursement. I mean insurance had been going along with DSM I, DSM II, DSM III and suddenly everybody's interested in the connection. I think it has been greatly exaggerated. I assume insurance companies are happy to have some coding and I guess they'd like to know what the condition is they're being asked to pay for but I don't think that's going to have a major impact.

It's not just culturally within the Mental Health profession it's had impact, its got out into the wider community they all know about it.

Well I guess people are interested in psychiatric problems, I don't know what to say about that. The other thing that DSM represents is a focussing on what is the central mission of psychiatry. DSM says it's to treat mental disorders. Well that's a narrower definition than one had in the 60s. In the 60s we were doing other things, we were understanding the human conditions.

Well on this score you had the article in the Archives from Tom Detre recently saying we've almost come back to where we were 30 years ago. Psychiatry is still trying to change the human condition and not just treat mental illness.

This is the Listening to Prozac thing. Well DSM represents a focussing but it's still pretty broad. DSM certainly has a lot of mild conditions. It has adjustment disorder. So you can defend it as not being too narrowly focused.

Is there a sense that even with operational criteria, you cannot actually stop psychiatry trying to treat humanity? We were doing it once psychologically - that's how we were doing it in the 60s. Now we're doing it biologically - put Prozac in the drinking water.

I don't have any insights to say about that. I've gotten interested in this whole notion of what is a mental disorder. Have you read any of Gerry Wakefield's articles, he's a Social Worker, who's written mainly in the psychological literature - American Journal of Psychology, Abnormal Psychology. He's written on harmful dysfunction - a way to understand mental disorders as a failure of an evolutionarily based mechanism. He thinks one of the real problems with the DSM is there are a lot of false positives - that a lot of things

that DSM recognises as disorders are really normal ways of coping with difficult situations. It's an interesting issue.

One of the views you get from Europe about DSM IIR from people like Pierre Pichot is that its extraordinary in psychiatry you can have four different diseases at the same time. Nowhere else in the rest of medicine can you do this unless you're a really old man.

I don't think that's true. You know if you go into a general hospital there are all kinds of people with diabetes and hypertension and heart disease. I don't know that the level of co-morbidity in psychiatry is that vastly out of proportion.

He would argue that we're going to have to collapse things back to hysteria for instance at some point.

Hysteria is a good example. Actually I once wrote an article about Hysteria split Asunder. Because that is what happened with hysteria in DSM III - there's dissociative disorder and there's conversion disorder and there's Briquet's syndrome and often there is maybe one underlying illness. That may have been an example of splitting going too far. But it would be hard to define hysteria more broadly. And you do see some people who just have conversion symptoms and some people who just have dissociation - whether it's all part of hysteria I don't know.

Could you have the recovered memory phenomenon if you guys hadn't created criteria for multiple personality disorder?

I think so.

How would it have happened?

You had the concept of multiple personality disorder - I don't think the diagnostic criteria had any influence on that. Multiple personality was recognised by DSM II. Also the people who are making multiple personality disorder diagnoses, commonly haven't the slightest interest in diagnostic criteria. You must admit that. Those are not neo-Kraepelinians. Neo-Kraepelinians are not running around diagnosing multiple personality disorder.

Somehow though putting MPD into DSM III sort of crystallised things and people began to use the concept - if only to find the patients that met the criteria. Whatever way you define the criteria they began to find the patients that met it where there hadn't been any before.

Well you see I think MPD comes out of an interest in trauma and the interest in finding what are the antecedents, the bad childhood experiences, which can explain adult psychopathology. But that's not a DSM III notion that's more an old psychoanalytic notion isn't it? It's a harking back to the idea that terrible things in childhood cause adult problems. That's not a DSM III concept. In fact there's nothing in DSM III that even talks about trauma pre-exposing. In III-R we said a predisposing risk factor is a history of child abuse.

But in a sense just having the criteria legitimised it and you get wide sections of the community making the diagnosis.

Would they if there had not been a DSM III with MPD in it? I don't know. Well okay there were other non-DSM diagnosis like the concept of dysfunctional

families, that's not a DSM diagnosis. You don't hear it now but around 10 years ago everybody had dysfunctional this and dysfunctional that which was not a DSM concept. There's satanical ritual abuse, for instance.

So an awful lot of the stories that swirl around the place about what got DSM III going and publicly accepted, you feel there's not a lot of evidence to support them. In a sense it must have been somewhat mysterious to you that DSM III became the phenomena it did. Can you explain it?

Well I'm repeating myself but I think the success was due to seeming to solve the reliability problem, which was a great source of embarrassment to American psychiatry. Psychologists could do studies showing that psychiatrists couldn't agree on diagnosis. Having diagnostic criteria, although I think we initially exaggerated how much improvement it made, certainly helped. That was number one. Number two by including all these other categories that people were using but were not officially accepted, we legitimised diagnostic concepts. People could say my notion of narcissistic personality or borderline has been accepted by the APA, I can use this book I don't have to ignore it.

There was the fact that psychiatry wanted to get closer to medicine and it looked more like a medical speciality after this. We could say the responsibility of psychiatry is treating mental disorders not helping the worried well. So it legitimised that trend. And those I think are the reasons. DSM looks scientific, it may not be scientific but it looks scientific. So it was very good for the self-esteem and then the American Psychiatric Association found out they could make a lot of money out of it. That made them so happy that they were interested in a revision. When I came back to them three years later, I said we need to make some minor revisions it will only take 18 months. They said fine do it. It took four years to do III R.

You hear the view that that III-R came too early.

It was seven years work. I don't know if it came too early. We made some corrections like we changed some of the diagnostic hierarchies. In some cases there was no real justification but some of the criteria weren't clear. But a lot of clinicians didn't like the idea that they had to learn a whole new system. They had the same complaint with DSM IV and will have with DSM V.

Will there be a DSM V

They're thinking of starting on it in 2004 to have it come out in 2010. They're also working on a text revision, where they don't want to change the criteria at all but they want to change the text. I'm sure the APA is doing it to make money. And that's supposed to come out in two or three years.

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