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Marketing the evidence

Behaviour Therapy with which you have been very closely associated now seems a very practical thing and it is hard to see how anyone could find anything wrong with it. But when it was introduced first in the 60s, it caused considerable controversy. Clearly the analysts were not happy about it but many others weren't either. Many Catholics were unhappy – for example when I was planning to go to university the Department of Psychology in University College, Dublin, which was very clerically dominated taught a metaphysics oriented psychology degree while the corresponding Department in Trinity College, Dublin which was the Protestant College, was very learning theory oriented. I was dissuaded from going to Trinity College on the basis that it would not be proper to go to a University that taught materialist teachings of that sort. Pierre Pichot has said that there were considerable problems also over in France. Can I ask you to give me some feel for how the scene looked when you entered the field.

I was not in right at the beginning. By the time I came Hans Eysenck saw behaviour therapy as a role for psychologists although Joe Wolpe was not a psychologist – he was an MD – not a psychiatrist either. As regards earlier developments, Leonard in Germany was a psychiatrist as was an even earlier forebear, Janet. It is interesting how psychologists tried to take over the field – with some success because even now it is thought of as more a psychologist's domain than a psychiatrist's, although in fact only a tiny minority of clinical psychologists practise behaviour therapy. The greatest opposition came from psychodynamic analysts who perceived a conflict between the approaches. Whether one perceives a conflict or not depends upon whether one regards the glass as half full or half empty. In 1967 I wrote an article with Michael Gelder on common grounds between the two approaches for which we got many hundreds of reprint requests. There was enormous interest in the shared field. In 1970 I wrote an article about the integration of psychotherapy. It never happened. Why it never happened is not easy to say.

The idea that Janet was a forerunner of modern behaviour therapy is an arresting one.

His work on OCD was seminal and he adumbrated the idea of exposure and response prevention.

Whatever about the controversies Behaviour therapy very quickly established itself as possibly the premier therapy for OCD and phobic disorders. If we focus on OCD for a moment, in the early to mid 1970s Ciba-Geigy of course began to push Clomipramine for OCD and this seemed to put you in a position of conflict with them.

No single therapy has a monopoly. Where different treatments work together why not use them both? There was some evidence for synergy in a proportion of OCD's and a proportion of phobias so I don't see a conflict.

Okay but on the other hand you did two trials one published in 1980 and then one later again in 1987/88 where you were claiming that exposure therapy was superior to Clomipramine.

Yes, but there were certain cases where clomipramine was necessary as an addition. So I am not saying it has no use, just that it is not the first line -except in cases who have comorbid depression.

Fine but in many respects your disputes with Ciba seemed something of a dress rehearsal almost for the conflict over the role of alprazolam in the management of panic disorders and phobias.

I would see that in a different light. I can't see much role for alprazolam or other benzodiazepines whereas I can see a role for antidepressants in OCD and other anxiety disorders.

Do you think clomipramine is working as an antidepressant in OCD or could it be that the drugs active on the 5HT act as an anxiolytic in a weakly neuroleptic sense.

It is hard to say. The mantra is repeated time and again that SSRI drugs are anti-obsessive and that their effect doesn't correlate with the amount of depression at the start of treatment. Often this is true but the people who write this forget that usually they exclude patients below a certain threshold of depression so that they don't have a large enough range of mood to really test the issue. There is no doubt that various antidepressants, not only clomipramine, can have an effect in non-depressed patients. But there is equally no doubt that the effect is far greater in the presence of depression.

Do you think that the drugs that act on the 5HT system in this way, including clomipramine, have more effect on OCD than the other antidepressants that are not particularly active on the 5HT system.

I have seen a review by John Greist suggesting that clomipramine has more of an effect than any of the other SSRI's but I have not evaluated this in any detail.

Let me just move the focus to a further aspect to Clomipramine and the SSRI's and OCD that comes through again in the panic disorder story which is that Ciba-Geigy and now, with the four SSRI's that have been licensed for OCD, there are about five different companies who have an interest in pushing OCD as an indication. Now it isn't that they have done the work which has led to us now recognising that OCD is much more common than we thought it was 10-15 years ago but somehow they seem to have made the

market. We now do recognise it much more commonly than we did – in a sense it seems that the companies have the ability to market the evidence, as well they market the actual compounds

'Market the evidence' is a good phrase. I was lecturing to a big symposium on OCD at the APA in Washington D.C. about 10 years ago and presented some of the evidence for behaviour therapy. Much to my embarrassment, at question time a psychiatrist of Indian origin got up and spoke to this vast concourse of about 600 people. He said 'I don't know what all the fuss is about how common OCD is. We have known about OCD for many years. Professor Marks has done all this work with OCD for decades so what is the big deal about how common OCD is?' He was making a point about the marketing of evidence by drug companies.

It has always seemed to be that you have had an interest to produce low cost treatments – cheap enough to give away to people in one sense. You have been in the business of trying to encourage people to treat themselves with treatments that have been shown to work, which is just the opposite to what the pharmaceutical industry do. And in a sense it seems to me that your strategy while obviously in one sense the correct one is doomed in that you won't be generating the profits needed to give treatment away. In the real world it seems that you can't give things away.

Well there are hundreds of therapists in this country giving behaviour therapy for OCD. Not nearly enough. We need a thousand or two. But behaviour therapy is widespread in this country. It is also wide spread in France with psychiatrists being the main behaviour therapists there. But it has not spread widely in other countries – that's true. The hope is that with the introduction of information technology, we will have a package that is marketable and inexpensive. Until somebody can market it, it is not lubricated for dissemination. The marketed product need not be horrendously expensive.

It seems to me with all of these things you've got to hypnotise the public to some extent – not just the consuming public but the prescribers, the people who decide what the treatment is going to be. They have to be hypnotised by having the regular meetings, the symposia, the company people calling round reminding them to use the product.

Have you heard of Triumph Over Phobia? This is now an England-wide network of 13 self-help groups, self-exposure groups, run by lay people who are usually ex-phobics or ex-OCD's who teach other sufferers how to become ex-sufferers. They have expanded from 8 to 13 in the last year and are about to go to 15 in the next few months. On February 29th, I opened the first TOP help group in Australia. So there are initiatives for spreading this treatment, some of which by-pass the professionals – although this doesn't mean that we can do without the professionals. I imagine many sufferers would want to have professionals as well. So I am sanguine that on a 20-year timespan the development of self-help methods including computers will spread this form of treatment more widely than before.

Behaviour therapy is compatible with the use of antidepressant medication for those who need it. The difference between me and the drug companies is that the drug companies would like to see every patient on drugs and I say only about 30% need to have medication for this problem (when there is co-morbid low mood) and then usually only for 6 – 12 months. Very few need it for years.

Will the public tend to assume that what costs more must be best.

In that case one could make the computerised packages more expensive. But the patient doesn't pay much for medication on the NHS. So it depends ... many patients hate drugs. Perhaps I tend to get referred more of those who dislike drugs.

Can I take you back to the mid 1970s when you ran the first trial with exposure therapy compared to clomipramine. As I understand it your thoughts at the time were that it would be unethical to run a straightforward drug versus placebo study.

Well we already had data about the efficacy of exposure therapy and the obvious question was how it compared with clomipramine, for which great claims were being made.

Do you think the claims being made then were inordinate?

Our findings were that in the depressed sub-sample it worked well temporarily, as long as the clomipramine was continued, and in the non-depressed sample we could no effect. The results we have had since then and my review of other peoples' work points in the same direction over the last 15 years – namely that antidepressant medication is much less specific than used to be thought. To call it anti-obsessive or anti-phobic or anti-this or anti-that is wrong – it's probably much more broad-spectrum in its effect. You may well be dealing with antecedent problems of which the OCD is an expression. But it is a good marketing ploy to call it anti-obsessive and of course it fulfils FDA requirements.

What role did the FDA play in all this.

Enormous. The FDA is more likely to approve a new drug if a compound has data that the drug is useful for a particular syndrome. So the pharmaceutical companies jump to that tune. If it is granted a drug indication, a company is made. There is much less mileage for a broader indication; it boosts sales less if one says the drug has a broad-spectrum effect, yet I think that is what the data suggests.

We are caught then in a bind because the FDA are doing what they are doing in order to improve risk-benefit ratios – they want to make sure people have a disease before they undergo the risks of therapy so that the benefits that they are likely to get will be substantial enough to justify the risks they are taking. To say that these pills are tonics, which is not far from what I believe, doesn't seem to meet the needs of the times in one sense.

Tonics is not quite right. I would say that whatever mood disturbance represents – and we don't really know – I would say that that is a very specific indication. But the FDA doesn't work by problems, it works by diseases. This is an

issue for medicine as a whole. In fact medicine is much more problem-driven than we are taught – problem-driven rather than disease-driven.

I guess one of the hallmarks of behaviour therapy is that it is not disease driven it is disability driven. Is there a difference between a problem and a disability.

Disability tends to mean work or social problems in this country, which is one aspect of the problem. But think of the work of Lawrence Reed in the early 70s, who was a physician in the USA – he was problem-driven. But his excellent method of problem-oriented records in medicine did not gain wide acceptance.

Has it not taken off?

If it has I have not heard of it. He is still a voice in the wilderness, undeservedly I think. In 1982, I was looking at his work with fascination wondering why everybody didn't take it on. I suppose the answer is it needs a certain dedication and the average doctor isn't interested. Maybe it's more time-consuming. Also it was problem-oriented records without ratings. If we understood why that didn't take off we would understand better why audits of clinical outcomes are so rare.

Any thoughts

Well turkeys don't vote for Christmas do they?

This comes back to your quoting George Bernard Shaw that every profession is a conspiracy against the laity.....

Of course it's self evident isn't it? We all have our guilds and they perform a necessary function. If we didn't have this conspiracy there would be far lower salaries and if we were on far lower salaries we might have far less able people coming in etc etc. It's a double-edged sword. If you want good people you have to select, pay and give them decent conditions. On the other hand it is in the interest of the public to be even-handed to everybody, so there is a tension there.

Which cannot actually be resolved....

Hence the term tension. That is why we need controls over the professions. And we need controls over the lay people too, if you think of what happened with the anti-psychiatry movement. Anti-psychiatry in itself does harm and the writings of people who are anti-pharmaceutical can become too strident and mislead sufferers. So it is hard to get a decent balance.

To move on from OCD to the alprazolam story. Could you tell me how you see that having evolved.

Well, I suppose there were two driving engines for it – psychiatry and the pharmaceutical industry. Don Klein and other psychiatrists formed one engine – looking for a biological role for psychiatry and a panic button located in the brain which is thought to mean this is a brain disease – what you have to do is hit the button with a specific anti-panic drug. Then Upjohn as a marketing ploy said we can hit the panic button with alprazolam and so they supported

diagnostic conferences for DSM-III and so on. When I expressed considerable scepticism in 1983 about the validity of 'panic disorder' that was the last of the diagnostic conferences I was invited to. They didn't want opposing voices.

When one member of my team running the alprazolam study with me started to question the concept of panic disorder, the scientific adviser to Upjohn tried to get me to fire him.

Gerry Klerman

The very same. We had endless battles because we didn't accept the basic philosophy. When they approached me to run the London-Toronto study I said why are you bothering you're going to lose and they said well that's your opinion. Somebody had to test it out. So we accepted the money to run the study.

There was an endless series of conflicts, with Upjohn wanting to do this and us wanting to do that and uneasy compromises being arrived at. When we asked for the data on which rested the importance of panic the result was a scream of fury at me from Gerry Klerman in the lobby of a Washington Hotel: 'give the money back to the company' he yelled when I asked him for the evidence. That was his answer.

Can I chase this a bit further because it seems to me that Klerman and yourself have many things in common in some respects. He is also a man who developed a therapy – interpersonal therapy. One that is focused on problems and disabilities rather than a disease entity as such. One that is simple enough to give away to the public almost – which was actually devised for Social Workers at first. And again just as you, before evidence-based medicine became the band-wagon that it now is, wrote articles on public health policy asking why does medicine not follow the evidence that behaviour therapy works for instance, in the same way Klerman took up cudgels in the Osheroff case, saying really oughtn't we be following the evidence.

Inter-personal therapy did not take off despite his espousal, although it is beginning to take off more, now that it is being pushed on a commercial basis. But we have to realise that we don't know what is common between interpersonal and cognitive and behaviour therapies. Nobody has tested just a straight problem-oriented therapy although I received an article today from Neil Jacobson suggesting that it is the behavioural activation component of CBT that may be the most important. We need far more such dismantling studies.

I have always thought that one of the interesting things is that you rarely get the various different therapy modalities being tested against each other. Is the reason why we don't get these, because the clinical psychologists are in the business of trying to make cognitive therapy look reasonably complex so that you need 2 or 3 years training to be able to deliver quality therapy – they don't want CPN's doing it.

There are both cock-ups and conspiracies everywhere, and a lack of money, a lack of research funds. It is not easy to get money to do dismantling studies these days.

Isn't there a vested interest almost against doing them because you are probably going to come out of it with some very simple components being active in their own right. So simple that you could give them away and then we would all be out of business.

Well lithium is a very cheap medication and yet it is marketed with some profit and Woolworths have managed to stay in business selling some very cheap things. So you can make lots of money out of cheap things when your market expands. Transistor radios have huge sales. On the other hand perfumes and diamonds have their price maintained at an artificially high level – it depends upon which market you are aiming at. I would like to aim at the mass one, so that we can help all sufferers who want help.

But to come back, the reason to profile Klerman was to wonder whether the two of you were not similar in some respects and did this fuel what ended up being an acrimonious situation.

No I think acrimony came because my unit questioned the accuracy of the model that panic was a purely 'biological' problem. We know that panic is an unreliable symptom to rate. We know that it occurs across syndromes. And we know there is no relation between panic symptoms and alprazolam's action and that there is a dependency issue.

In fact the first acrimony with Gerry had nothing to do with alprazolam. It was at a WHO meeting in Copenhagen in the early 80s. I asked a question about Briquet's Syndrome, about the validity of the diagnosis and its implications and suddenly he exploded. Much to my surprise because we had been very good friends until then. He had written letters to me saying how much he admired my work and wanted me to stay in his home and he and his first wife stayed with us etc. then suddenly... I suppose it was this – if one dared to question the model this was taken personally, not just on panic but on the overarching importance of diagnostic classifications and categories.

His contribution to inter-personal therapy with Myrna Weissman was one of the most important things he did. And he was responsible for getting the epidemiological catchment area study off the ground and to some extent for getting the NIMH collaborative study of depression started.

He was also one of the people responsible for nine hospital study of chlorpromazine the one which was published in 65, Goldberg, Klerman and Cole.

Yes, Gerry was one of the foremost psychiatrists in the States over the last century. When he became Upjohn's scientific adviser, he was too close to the scene to see what was problematic about what Upjohn was sponsoring. And Upjohn made huge amounts of money. It was a brilliant marketing ploy to use panic criteria to make huge amounts of money but it had nothing to do with scientific reality or helping patients.

What happened when you wrote into Archives critiquing the first set of alprazolam studies; as I understand it there was a letter which took 15 months to appear then only appeared shorn off some of its essential details.

Eleven psychiatrists from 6 countries helped draft this letter and we sent it off. I got it right back saying this is unacceptable. I phoned the secretary to find out

what was unacceptable and was told that a) the margins are the wrong size and that b) the signatures were not all on one page – from six different countries. Danny was always rude in the way he wrote to me perhaps because I said to him about his publication in 1988 of all those alprazolam studies that they were very uncritical. When I told him that he said 'just you remember that I was on the selection committee that got you selected for the Stanford think-thank'. That was his response. He was always rude. I could never get a paper into his journal, Archives of General Psychiatry, from 1983 onwards. There were endless problems. They would all automatically go to Don Klein or someone like-minded for refereeing. But this is not just true for the Archives. In general anything that bucks a paradigm gets a hard time.

Eventually Danny did accept the letter, rather to my surprise because it was a quite damning letter. Then when I got the copy editor's corrections, I noticed the letter it was shorn of its accompanying table showing that with one method of analysis alprazolam had absolutely no significant effect whereas with another method alprazolam had some effect. So it depended on which method of analysis was used whether alprazolam had any action at all. I got an acerbic letter back from him saying I had never sent the table. I know I had sent it. I sent it to him again immediately. He replied oh there isn't time now, it's too late. And the letter appeared five months later without the table. So that's the kind of thing that went on. I was told by one of the referees of one of the main 1988 papers in the Archives that Danny had written to him saying 'we would like your quick opinion about this because we very much want to publish this article about alprazolam'.

Are you saying that his judgement was compromised.

I can only tell you what I know. I believe he was a consultant to Upjohn.

Was your London-Toronto study first submitted to Archives as well.

It was sent to the New England Journal of Medicine and they took twice as long as usual to send an opinion. They only sent me an opinion after I had faxed and phoned them. Finally I got trivial criticisms that did not address any of the substantive issues. They rejected it, so then I phoned up and spoke to the person who was handling it, saying 'I think we can meet all these criticisms quite easily I wonder would you be willing to have another look at it'. 'Sure sure'. I sent the revision along and exactly the same thing happened again – twice as long as usual – six weeks is the usual turn around this was over three months with still no answer to many phone calls. Finally they rejected it again, this time with a different set of trivial comments.

Why do you suppose the New England Journal was so hostile?

Presumably their referees were sold on the panic/alprazolam paradigm. So then I decided that I can't educate American Psychiatry or American medicine against its wishes and sent it to the British Journal of Psychiatry. It was accepted with almost no change.

But with the most amazing correspondence.

Subsequently yes. Upjohn solicited a committee to do a hatchet job on this. In fact David Spiegel, whose name appears first on it is still a good friend of mine. He is a fervent believer in the virtues of alprazolam. Some people received fees for participating in this exercise. It led to an interesting correspondence which attracted a lot of attention. All sorts of people wrote to me saying how wonderful it was and at conferences they congratulated me. People like a good fight perhaps, whatever the issue. Then there was this Upjohn conference in Geneva.

That was around 89. It appeared in the Journal of Psychiatric Research supplement to 1990. Wasn't there an exchange between yourself and Gerry there – a few of those who were there have mentioned it.

It was in 1990. The conference was a superb propaganda exercise by Upjohn. They even had the gall to display above the podium what looked suspiciously like a United Nations or WHO emblem for this meeting on 10 years of panic. Lew Judd and other top people gave papers on the concept of panic and alprazolam's value for it. Although the London-Toronto study was the most controlled of all the studies, it was sidelined, marginalised to be presented to only a quarter of the audience in an afternoon session rather than a plenary morning session. But word had got round because of our Abstract. The audience in our small room crowded into the aisles and overflowed in the corridor trying to listen to the presentations by Richard Swinson and me.

A Greek newspaper man said 'ah at last we have got somebody honest'. And he published something in an Athens newspaper. A local Upjohn representative there contacted him and tried to rubbish our work to undo the damage we had done.

There was an exchange with Gerry months before the Geneva meeting at an investigators' meeting in New York in Gerry's office in Cornell. The first results had come through, analysed by Upjohn not by us. And the first comment made as we had the results in front of us came from Matig Mavisakalian who said well this shows that behaviour therapy is doing much better than alprazolam. It was as if nobody heard it. Gerry said 'you must present these results at the CINP in Puerto Rico in December'. I asked him why do you want to present the results to the CINP? He said 'why shouldn't we?'. I said 'because they don't show alprazolam up very well do they?' He said 'what do you mean, what do you mean!'. In other words the figures were there but he could not take them in. Nobody could take them in because it was against the paradigm. And of course we weren't invited to the CINP.

We had already been invited to Geneva by then. In Geneva, Gerry was telling me about his flying from one renal dialysis machine in New York to another in Geneva and about his recent marriage to Myrna and how the children had responded well – it was as though we were friends back again in the 70s. Then suddenly it was like a switch being thrown and he started shouting at me. 'I

never want to speak to you again'. That was that. I tried to mollify him but he just wouldn't have it. Perhaps his terrible illness exacerbated his irascibility.

About a year later I saw him for the last time at a meeting on the Eastern seaboard. I saw him having breakfast in the hotel and went up and chatted to him. We spoke about family issues and then other things. He was always fascinating, even though he had blind spots. That was the last time I spoke to him but then we had our exchange of letters in the British Journal of Psychiatry.

Myrna Weissman told me that my last critique of his letter, in which I pointed out that his reply to us had not dealt with any of the salient objections we had raised, reached his office the day he died. Myrna was extremely upset by this and wouldn't shake my hand when I tried to give her my condolences at a conference, where we were on the same platform. She took our critique of his work very personally. I can understand this in the light of the circumstances.

It is difficult to get an unpopular message home. First biological psychiatry took over American psychiatry using its diagnostic system and Gerry played a big part in this. The other was that Upjohn used this view to market their compound. Both were extremely successful. From the point of view of their aims, their strategy worked brilliantly but whether patients benefited is another thing entirely.

Panic disorder as a concept has maybe temporarily taken the field. Do you have a feel for how temporary this is likely to be.

Who knows? ICD 10 articulates an alternative view of panic. Whether in time that will prevail is difficult to know. There are many criticisms of DSM classifications. There has been an excellent article on American diagnostic imperialism by Richard U'ren that he had difficulty publishing. Gambling and insomnia and many other problems all become disorders and therefore susceptible to psychiatric ministrations and therefore reimbursable etc. Recently came a bid for 'shopping disorder' to be recognised.

There are two or three points there. Is part of the reaction that you got not from Klerman as such but from US Psychiatry more generally down to the fact that in quite a few of the health schemes over in the States psychiatrists won't be reimbursed for anything other than actually prescribing.

Oh yes absolutely. Not only that but prescribing by a doctor of course. There was a fine controlled study in Burlington Ontario in the late 70s by David Sackett showing that nurse practitioners cared for primary care populations as well as did GPs. Eventually the work ended because nurses' care was not reimbursable and that rule was not changed.

Similarly in the 70s there was the Lobene project in Massachusetts showing that dental hygienists actually did better work than did dentists in drilling, filling etc. The dental hygienists had one year's training, the dentists had five years training. The net result of that was the closure of the dental hygienists training course. So the term 'conspiracy against the laity' becomes meaningful

with such events. I am sure we could get many such stories, and that when we get companies producing profitable computer self-care programs similar things will happen.

Will progress require the overthrow of Kraepelin or at least the version of Kraepelinism that Gerry Klerman, Eli Robbins and others set up in the 70s. This Kraepelin has almost become an idol that one can't question.

Who knows, Michael Shepherd's last paper on Kraepelin in the British Journal of Psychiatry was excellent. We obviously have to have classifications. The trouble comes when it becomes a fetish, when reimbursements are linked to it. Classification simply as a means of communication is fine. But when one can't survive financially unless one squeezes patients into unrealistic boxes then all kinds of problems result.

Talking about your approach to therapeutics, social phobia would appear to be the next battleground.

It's underway. This is much the same as for OCD and for agoraphobia. It is a time-worn, simple and successful strategy. Find a disease, make it out to be something that hasn't been recognised until now, remind everyone how common it is, then claim you have a drug for it.

And it looks like it will work again?

What's worked before could well work again.

Where did the concept of social phobia come from – you're generally cited as being its originator

It came out of my work with Michael Gelder in the mid-1960s.

Does the social phobia story bring out some of the positive aspects of the process in that you could argue that while we may end up with social phobia being vastly over-recognised in the near future, it has been markedly under recognised it seems to me up until now.

Well that is true of all the anxiety disorders. The ECA, Munich and Zurich studies have shown that over half the identified cases haven't been treated for their condition – that is clear – but whether treatment with medication is the right answer is moot. Behaviour therapy is effective.

To come back on that for a minute. I know you're involved in a collaborative venture with Pfiser at the moment. Are you selling out to the industry on this one?

If they are willing to promote computerised effective self-treatment of OCD, that's wonderful. Full marks for their prescience.

The outcome of the whole thing will be computer methods which they will own and market?

No. Dr Greist, Dr Baer and I own it but hopefully Pfizer will market it. If they don't we will. The pilot results in 64 patients in the UK – US studies show

that the results are about as good as those on medication. And so an eight-site controlled transatlantic study is being launched soon by Pfizer. That is a unique situation.

The outcomes of the pilot studies are sufficiently encouraging to go ahead with controlled trials. Obviously we can't cure everybody. But I am fairly sure we will help a lot of people to some extent at reasonable cost and without medication side-effects.

Can you give me some idea of where the idea of delivering therapy by computer came from? Who had the idea and when?

There was Weitzenbaum's Eliza program decades ago. John Greist in 1977 suggested that I try computerising exposure therapy for phobias. I did and it worked. John's computerised CBT for depression worked too.

Coming to Evidence-based-medicine. As I hinted earlier you were talking that kind of language before it became the fashionable thing it now is. But again is this one of the things that the pharmaceutical industry are going to turn to their own advantage in that they have the means of produce the data more readily than anyone else has.

The evidence for behaviour therapy is better than for most drugs and there is a simple means of producing that evidence; with the advent of computerised audit we have a great way of developing a Clearing-house of Clinical Outcomes with a quick turnover time. This would not be a prerogative of drug companies alone. It would help clinical trials of all kinds of medication, of psychological treatments and other interventions to be done more efficiently. Evidence-based medicine can be a healthy movement as long as it doesn't become a fetish.

But it will become one won't it?

It's a danger. But there is nothing new in what we have discussed today. I understand there was a controlled study of scurvy in the 18th century that showed very clearly that sailors would not die if you gave them citrus juice but thereafter it took 50 years before the British Navy regularly gave the limeys their ration of lime juice So there is nothing new in the present slowness to apply evidence-based medicine.

But it seems to me that the evidence is about a drug it is going to be applied much more quickly because the industry will see to it that people are aware of it.

There are all kinds of inertias once a paradigm, an ethos, takes hold. Take smoking – in the early 60s the first reports of its association with lung cancer came out and the retreat from smoking has only just begun. There are many reasons for this – in the case of tobacco there was addiction and money and a lot of other things besides – which affect whatever we do. The conservatism of our profession is good in some ways and bad in others. You don't want to go veering off in new directions every year. We have to be careful that each time

we see a new drug that it is not used as a panacea. But you don't sell a drug widely by saying it's got limited usefulness.

No but what has happened which will probably come as a shock to a great number of the public is that while the campaigns were waged through the media about the horrors of benzodiazepines and the pharmaceutical industry was probably perceived by many people to have been rolled back, in actual fact the 80s and the early 90s were the time when the neuroses have been medicalised. Panic disorder, OCD and now social phobia have been made disease entities. It has all happened seemingly without the media being aware about what has happened. You would have thought that the benzodiazepine controversies would have sensitised them but this has gone beneath the radar completely.

There is a tension all the time isn't there between those against and those for and the pharmaceutical industry spends billions of pounds on advertising.

Is there the other element though that in the approaches that you take quite apart from the fact that you don't have billions of pounds to advertise you are offering people hard work whereas something like Prozac offers a remake

Sure, but empowering the patient is the flavour of the decade.

Provided it doesn't require too much work?

It depends on the patient. Some love it and some hate it. Patients' demands for information will grow. Patients in this country are too passive. They don't ask their doctors for enough information and we are not trained to give information. Medical schools should pay far more attention to the dissemination of information to individual patients and to society as a whole.

Yes and to the ability to assess risks and especially relative risks from treatments which we don't do at all.

If I have just been paid to attend a conference on a lovely island in the Mediterranean or Caribbean then I will think of the sponsoring company's drug whenever I reach for my prescription pad. When companies start to market computer self-care packages and send doctors on lovely holidays then the doctors will think of those packages for treatment, which is no healthier a way to practise medicine. I am told the same thing happens with prostheses such as hip replacements. So that is always the tension. Perhaps companies that don't oversell are less likely to survive.

This is the point I have been trying to make to you the whole way through- in a sense you aren't oriented toward an oversell.....

Sure. The great epidemiologist from Oxford, Richard Doll didn't oversell the connection of smoking to disease. The evidence has to speak for itself and it takes time to affect policy. Duelling was a major source of mortality in French aristocratic males in the 17th century but when Louis Quatorze outlawed it it took a generation for it to die out.

Why have you ended up being a person who has been keen to give things way as opposed to becoming a consultant to Upjohn or whatever?

It is much more important to try and find effective treatments that help the people in the long run. In the very long run that is what will become accepted. There will be many mistakes along the route.

I trained in medicine in Cape Town, South Africa in the 50s. when there was very much a British tradition at that stage. Epidemiological evidence was very important. Once you look at epidemiological evidence, you have to look at the mass market – that is what epidemiology is largely about. And that is the way I was taught. If you work in a children's hospital where one wing has black patients and the other wing has white patients and the white wing has the rare diseases of metabolism and other odd things and the black one has mass misery with kwashiokor and gastro-enteritis and other diseases of poverty, it soon becomes obvious that what is needed is a remedy on a mass scale and you can't fix things just on the ward.

And is that what has been the formative influence.

For me yes. We took it for granted that one went for treatments that are easily delivered and inexpensive.

What about the Institute of Psychiatry when you came in the 60s? It has always had a certain skepticism about drug treatments. It hasn't seen them as the great breakthrough that they have been seen as elsewhere. As Aubrey Lewis put it if we had to choose between the rehab units we have and the new drugs the drugs would have to go.

That's interesting. Some current colleagues are gung-ho on drugs. That's more recent but epidemiology was very important in the training of psychiatrists in the 60s as well and that had an influence. Aubrey Lewis' attitude to drugs did not influence me. I have always accepted that drugs have a role. I was taught that too. And I was amazed at the way drugs could change psychological events.

Amazed in the sense of almost dualistically not actually expecting that drugs could change psychological events.

No I mean it makes absolute sense. When someone paranoid felt that the commies were after him last week and he has a bit of chlorpromazine and then this week they're no longer after him, that is quite an exciting change.

Who else has been a formative influence.

The most formative influences have been mainly philosophers of science, Thomas Kuhn, Carl Hempel, Karl Jaspers because they taught how to evaluate evidence and Paul Meehl looked at clinical versus statistical prediction. People who taught me to evaluate evidence are the ones I'm particularly grateful to and those who taught me experimental design – Bob Cawley, Bob Maxwell the statistician, Felix Post who taught me phenomenology and Bob Hobson who taught me aspects of psychotherapy. They were very formative for me.

Is there anyone who has been working with you who has been particularly helpful.

John Greist and Lee Baer have a great effect on me – wonderful colleagues. Richard Swinson has been enormously helpful and I could not have done such a careful study as our London – Toronto trial without someone as good as he is who shares a similar philosophy. He has published several studies about home treatment, telephone treatment, and how rarely behaviour therapy is given, and he will be joining us in the computer treatment trial.

In the early 70s Jack Rachman, Ray Hodgson and I had a seminal collaboration. I have mentioned Bob Cawley and Michael Gelder and I had a very fruitful partnership with John Bancroft and Adolph Tobena from Barcelona who is a neuropharmacologist and psychiatrist. He influenced me greatly as did Randy Nesse on evolutionary theory .....

How do you see things going from here. Are we wedded til death do us part to the disease model – the FDA seems to be. But increasingly the companies for instance are being encouraged to consider selling the drugs over the counter. Now if the H2 blockers can go OTC, surely the SSRIs could to. In which case would the companies stop selling the medical model or at least the bacteriological model for a psychiatric disorder and sell something more dimensional?

Another change is influencing pharmaceutical companies. Several are becoming disease management companies rather than just pharmaceutical companies. This may be partly a response to what you have just described. Another influence might be the move to managed care – it might drive them in that direction. This is a healthy move. It makes them less vulnerable to failure if they are less wedded to the success of a single product. And the patient gets a better deal and a broader spectrum approach. I can think of at least three major companies doing this on both sides of the Atlantic and this is why we are being funded by a drug company. Another drug company here has funded a computerised audit for general medical diseases yet. Another company also wants to apply this in general medicine. I think this is useful but of course the audit will need to be audited. As soon as money depends on results people are going to be tempted to massage those results. Few people pay more tax than they need to and few earn less than they need to.

Prescription rights. Do you think it is one of the areas of medical power. Could this be dismantled?

You were saying that OTC is getting more common I can't see everything becoming OTC. That would probably be undesirable. I would give prescription rights to some nurses and some psychologists who have been trained – another turkey voting for Christmas.

Has Hans Eysenck been any influence to you as such because quite apart from his learning theory work there is also a drug postulate to it, which is that drug treatment should be able to shift you along an introversion-extraversion axis and it may well be that

drugs, like the SSRIs, are actually doing something like that rather than treating a disease.

That has never been an influence on me.

Another reason to bring him into it again is he wrote his famous piece around 52 on the evaluation of psychotherapy and he followed it about two years ago with an article in Behaviour Research and Therapy reviewing the state of affairs 40 years later concluding that there is no science on earth that can overcome ideology.

That is probably true. A difficulty is that many call science what they believe in yet what others believe is 'ideology'. In fact the truth may be more subtle.

Does cognitive therapy qualify as part of behaviour therapy as you understand it. Are the cognitive therapists just finding other ways to expose people unbeknownst to themselves.

Exposure is not part of cognitive treatment for depression. It is a problem-oriented method. That leads to the question: is it the problem-oriented method, is it the breaking things down into manageable bits, or is it the activation component? Good dismantling studies are very important to do. It is too early to be sure what is useful. Neil Jacobson has just done one RCT finding that behavioural activation alone reduces depression as much as does full CBT. There will be others in time because it is obvious that we have to answer these questions.

Could the pharmaceutical industry support them because presumably the only way they are going to market another antidepressant would be as part of an integrated management package. You know drug therapy plus activation if it came out would be a very sellable package.

John Greist, Lee Baer and I in fact are producing a computer aided treatment for depression, which is now being pilot tested. Once a psychotherapy method is computerised it is much easier to add and subtract components and deliver them precisely. We have designed our system in such a way that we can add or subtract these components. So we hope to do dismantling studies in time to come if we can generate enough revenue to pay for them.

You are perhaps better placed than anyone else to comment on the tension between psychology and psychiatry. Clinical psychology seems to be growing partly on the back of cognitive therapy, perhaps in the beginning there was behaviour therapy but its moved on now to cognitive therapy..

There is a clash between the two professions, two conspiracies against the laity. I don't think it is resolvable. There is also a clash between nursing and medicine. These things are to be expected and have to be contained.

Will neuroscience feed into all this at some point.

Brain and mind are coming together. The dualist position has been undermined by neuro-imaging. Neuro-imaging has played no role so far in developing treatments but it is of great interest when one can show an area lighting

up that behaviour therapy can change just like drugs do, which you'd expect. This is obvious common sense. There are many exciting areas. We have a long way to go before imaging tells us anything about aetiology. You know the story about the scientist and the lamppost.....

Yes I heard it from Don Klein first – ironical in a sense that you should both cite it[1]

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