

Interview

In conversation with Alexander H. Leighton

M. D. Teehan interviewed Professor Leighton in Canada in September 1992



Professor Alexander Leighton
AB (Princeton) 1932
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With the Stirling County Study in 1948, Professor Leighton initiated the first of the post-World War II researches in the psychiatric epidemiology of general populations in North America. Still ongoing, this programme was preceded by his psychocultural studies of Navajo, Yupik (Eskimo) and Japanese cultural groups and later accompanied by comparative studies in Nigeria and in what was formerly South Vietnam. Nine books have resulted from these investigations, together with over 100 articles in learned journals and as chapters in books. Following psychiatric training at Hopkins and after serving in the Navy during World War II, he was appointed Professor of Sociology and Anthropology and also Professor of Psychiatry at Cornell University from 1946 to 1966. In 1966 he moved to the Harvard School of Public Health from which he retired in 1975. At that time he was Professor of Social Psychiatry and Head of the Department of Behavioral Sciences. Currently he holds post-retirement positions as Professor of Psychiatry and Professor of Community Health and Epidemiology at Dalhousie University in Nova Scotia. He has served on a number of advisory committees for the governments of the United States and Canada and for WHO. Honours include Honorary Fellowship of the Royal College of Psychiatrists (1983), Honorary Doctorates from Acadia University (1974, Nova Scotia), and Université Laval (1991, Quebec), Canada's National Health Scientist Award (1975–84), Membership of the American Philosophical Society (1954), the Rema Lapouse Award from the American Public Health Association (1975), and the McAlpine Award from the National (US) Association for Mental Health (1975), Fellow of the American College of Psychiatrists.

Your early work was in natural history?

Yes, in animal behaviour. I had two papers published while still in high school, one on porcupines and one on turkey-vultures. Then there were three on beaver behaviour written during college and medical school.

How did you progress to the study of humans?

After graduating from Princeton I spent two years at Corpus Christi College in Cambridge studying the neurophysiological underpinnings of behaviour. When I moved on from there to finish medical training at Johns Hopkins, I was much attracted to physiology, but in the end I felt it would be many years before it would be in a position to contribute much to our understanding of behaviour as such. Then at Hopkins I came under the influence of Adolf Meyer. He was basically a naturalist himself and had a naturalist's approach to clinical phenomena. He emphasised the possibility of studying the person as a

whole as opposed to relying only on experimental studies of isolated functions at either the physiological or the psychological level.

He was at that time the outstanding figure in North American psychiatry?

Yes. He was the leading influence in psychiatric education in North America from 1912 up until World War II. Almost every important clinical teaching centre in the USA and Canada came to be headed by a former student of Meyer. In Britain, Henderson and Aubrey Lewis were former trainees of his. Up until the great wave of psychoanalysis swept over North America, he was pre-eminent.

After 1939 you devoted a lot of time to cultural anthropological studies among the Navajo and Inuit?

That was a step toward the study of normal personalities in my own north eastern American culture.

The reason I wanted to study normal personalities was because it seemed to me that research in psychiatry suffered from a lack of normal controls, and I became very much interested in the question of how people who are not patients handle the kinds of problems that seem to be so germane to the development of mental illness in the people we had as patients. All of our psychiatric ideas of normality were religious or philosophical in origin, or were inferences based on the study of pathological behaviour. There were very few systematic studies comparing the behaviour of ill people and well people.

Comparative studies were very much part of the naturalist scientific tradition, and I suppose it was this that directed my attention to what I thought of as a gap in psychiatry. Meyer had developed concepts and methods by which to examine and analyse the personalities of patients and of normal persons. It seemed to me that this might be adapted for a scientific study of personalities as found in a normal population. When I began seeking advice from social scientists about how to develop these ideas, I was strongly advised by cultural anthropologists to begin by making personality studies in one or more cultures that were not my own. Their reasoning was that this would greatly increase my awareness of cultural factors in my own culture and put me in a much better position to understand the ill and well persons I selected for study there.

A powerful influence in my accepting these ideas was Bronislaw Malinowski. Although born a Pole, he became, after World War II, a leader in British anthropology and like his friend, Joseph Conrad, a compelling writer of the English language. Two other anthropologists guided me toward choosing the Navajo and the Yupik-speaking people on St Lawrence Island. These were Clyde Kluckhohn at Harvard and Ralph Linton at Columbia University. My first wife, Dorothea Leighton, also played a major part in the carrying out of this anthropological work.

I might mention also that during and after World War II I had considerable exposure to Japanese culture.

The Navajo, Yupik and Japanese studies were preliminary to the Stirling County Study?

Yes. They also modified the aim somewhat, or perhaps I should say gave it more focus. I became tremendously interested in the hypothesis that cultural and other social-situational factors played some role in determining who developed mental illnesses and who did not. A first basic step in tackling such questions was an epidemiological study aimed at ascertaining the prevalence of mental illness (and non-mental illness) in a normal population. To my

surprise, I found it very difficult to interest psychiatrists at this time. Fortunately civic leaders and people in the foundations were favourable to it. As a result, I got three grants, one from the Carnegie Corporation of New York, one from the Milbank Memorial Fund, and one from the 'Dominion-Provincial Health Grants' in Canada.

Before securing these grants in 1951, we had held a faculty level planning seminar at Cornell University each academic year starting in 1948 or '49. This met once a week, reviewing literature and discussing theoretical and methodological issues. Out of it came a plan to divide the project into three segments. One segment was sociological and cultural in its make-up and had the task of describing the social structure and culture of Stirling County. This included the different socio-economic levels, the occupations and the cultural groups which, of course, were mainly French and English. This team had the task of identifying social and cultural variables and working out ways to measure them.

The second team had the task of developing a psychological screening questionnaire for identifying some kinds of mental illnesses in the population and tabulating their prevalences. A result from this was the 'Health Opinion Survey' developed by Allister Macmillan. It was a psychoneurotic screening instrument based on a numerical system of scoring.

The third team had the mission of developing a clinical procedure by which psychiatric cases in the population could be identified and a diagnosis made. The staff in this team were psychiatrists. For a time we thought that some way of checking the entire population might be worked out, but we were forced to give this up due to the size of the population and due to the numbers of cases present as suggested by our early estimates. Sampling methods were therefore adopted and the data collected by means of a "schedule" of systematic questions asked by an interviewer.

The first survey of this kind was conducted in 1952 and contained questions put forward by all three teams: information wanted for quantitative description by the social science team; the HOS psychoneurotic screening instrument from the psychological team; and data for a diagnostic evaluation put forward by the clinical team. After the survey, the latter data were given a clinical diagnostic evaluation by psychiatrists who worked according to standardised procedures which were based on Meyer's diagnostic concepts and on the whole similar to those maintained at the Maudsley.

What was the diagnostic procedure?

Typically two psychiatrists made an independent evaluation of the data on each respondent which had

been obtained in the survey. Following this, the psychiatrists read each other's evaluations and where discrepancies had occurred, they discussed the difference and reviewed the rules until they could arrive at agreement on a joint evaluation. It was this joint evaluation that was employed in the ultimate prevalence and incidence analyses, but the independent ratings were also kept and employed in various analyses of the reliability of the evaluators.

Was that the main way you tested the reliability?

No. In addition, without the evaluating psychiatrists knowing about it, a certain percentage of cases which they had already jointly rated were given to them again.

DSM-I must have been published about the time you were doing this work. Is that what you used as a guide to diagnosis?

Only to the extent of borrowing terms, which we re-defined so that they referred to observable phenomena only, and were devoid of psychodynamic assumptions.

Why did you do that?

Our research was aimed at discovering causes – or at least clues to causes. It was therefore necessary to avoid building causal assumptions into our definitions of disorders. Because in those days the word “diagnosis” in psychiatry generally meant identifying a psychodynamic cause, we used the term “evaluation” instead of “diagnosis” to label our procedure for identifying symptoms and syndromes. We suffered for this when it came to publication because many of our readers did not understand what we meant by an “evaluation”. They wanted to know: is it a diagnosis or isn't it? The answer, of course, depended on what the enquirer meant by “diagnosis”.

Since the advent of DSM-III and its widespread acceptance, this problem has largely disappeared. Let me give you an example from research in Africa. At one time prevailing psychodynamic theory assumed that guilt was part of the definition of depression. During this period epidemiological research in Africa reported that Africans did not suffer from depression. It turned out that they did in fact present the clinical syndrome of depressive symptoms and disability, but it was very rare that examination could elicit evidence of guilt. Ergo, they couldn't be depressed. When classification was made descriptive without reference to dynamic theory involving guilt, depressed Africans turned up in numbers comparable to those found in the population of Europe and North America.

Back to the main story. We had then in essence two methods of analysing the collected data: one was a screening instrument with a numerical scoring focused on identifying psychoneurotic kinds of disorders. The other was a diagnostic approach utilising psychiatric “evaluations” and focused on the whole range of clinically recognised mental illnesses.

What kinds of questions did the neurotic instrument ask?

Are you troubled by your heart beating hard? Do you feel frightened for no particular reason? Does your heart beat hard when you are not exercising? They were derived from many sources by Macmillan and tested experimentally. Their importance did not lie in their face validity but in the fact that they could serve as indicators or “markers”.

How did you sample the population?

For the first survey in 1952 we made maps of the county on a scale of 6 inches to the mile and then located on this every occupied dwelling. To do this our statistician and his wife had to drive every road in the county and locate every building in which people lived. Each of the mapped dwelling units was given a number and then from these the house in which an interview would be conducted was chosen by random methods.

How long did that take?

It took the greater part of a summer. But other things went on simultaneously. Having picked a sample of houses on the basis of this mapping, the interviewers went into the houses with a formula for sampling one of the adults who lived there.

In subsequent surveys we gave up the mapping system and instead made a complete census ourselves of all adults in the county and used this as a basis for drawing samples. It was not much more laborious than the mapping and afterward much easier to work with.

There were about 20,000?

20,000 including children. The census for adults was about 10,000. Our just completed 1992 census shows an increase to about 13,000.

After 1952 what happened?

The bulk of our data for the first survey was gathered in 1952 but there were sub-units for work that went

on for two or three more years. There was a mop-up operation, for instance, in 1953 for interviews that for one reason or another had been missed.

The sample was about 1000 in total?

Yes, it was closer to 1100 in all, but the number we eventually used in most of our analyses was 1010.

Why didn't you include children?

The diagnostic problems were much more difficult than with adults, and our thought was to try to walk before we ran.

I mentioned that the research programme was composed of three teams: the social science team described social and cultural properties, the psychological team developed the psychoneurotic screening instrument, and the clinical team developed the evaluation process. The clinical team also ran a community clinic and analysed some of this case material, exploring such questions as "are there any distinctive syndromes in this population and if so what are they like?". One paper on depression and population norms came out of this.

Did the survey site itself in the clinic as it developed or had it separate headquarters?

The survey team and the social scientists were together but in separate quarters from the clinical unit during the first survey. During the second and third they had different floors in the same building.

But there was a close relationship?

That differed at different points in time. Unfortunately in later years, with changes in personnel, the clinic unit largely withdrew from the research effort. Lack of interest in research, as you know, has been a common pattern in psychiatric services.

There was fieldwork going on then from 1950 through to the time of the second sweep in the late '60s?

Qualitative and descriptive fieldwork began in 1948. From then until now there was never a time without some fieldwork going forward, but it dropped off considerably at times, for instance, during the period that we were in Africa in the early '60s and in Vietnam in the early '70s. 1952–53 was a peak time and we had another in the period from 1962 to '64 and then from 1968 to 1970.

Incidentally, I should mention that the work greatly benefited from Kenneth Rawnsley's presence for several months one summer in the mid-'50s. He was a careful thinker and an incisive critic.

When did the fieldwork actually end?

Our last survey was in 1970, but some qualitative work has gone on intermittently until 1991 when we began a new survey.

Did the surveys end because the work was done?

No, we were drowning in our data by 1970 and needed to stop so that the next surveys would benefit from more extensive analysis.

Of what you had already gathered?

Yes, and that is where we are now. We finally feel on top of the analysis and have a reasonable sense of what kind of questions we should be asking next.

Is it possible to encapsulate the important things you have learnt from all of that data?

To my mind the first finding of significance was that the prevalence rates of the kinds of mental illnesses we see commonly in clinics, and as practising psychiatrists, are exceedingly high. If you lump all the different kinds of mental illnesses together you find that about 20% of the population has got one or more of them. This is important because no mental health service in the world can meet such a volume of need. It suggests urgent efforts to improve on treatment, prevention and health promotion. It raises serious questions about what we are now doing and what we could do to control such rates. The immediate reaction to this prevalence finding was, of course, disbelief.

Some of the psychoanalysts told us we were "just tabulating human miseries" which lacked psychodynamic meaning. The people in our tabulations, they said, don't need psychotherapy, they don't need psychiatric attention, they just need a better world.

We tended to think at first that this might be true and that the prevalence rates we had found did not refer to the kinds of cases seen in out-patient services and at in-patient services where we had been trained. We therefore ran a variety of validity tests. In fact we spent about five years working on validity alone, trying to determine to what extent these figures were valid and at that time I became persuaded – and the other psychiatrists also working with me all became

persuaded – that the figures were a good approximation of reality. Now, well over a 100 studies, maybe 200, all around the world have indicated that 20% is a reasonable figure. Different researchers estimate between 15% and 25%, the variation being in part due to methods and in part due to actual variation of prevalence rates in different populations.

I think all this adds up to uncovering findings of importance for understanding the nature of psychiatric illness in human populations. It suggests a need for some restructuring of psychiatry, some redefinition. This in turn implies changes in teaching, changes in philosophy, in outlook, changes in how society copes with these disorders. So far not much of this has happened. Perhaps humanity has too many problems. It has had some effect in the Scandinavian countries but, except for a few individuals, it didn't have much effect in North America. People just find it easier to deny or overlook what epidemiology turns up.

So people ignored your findings?

Maybe denied them.

Dr Murphy has reworked the analysis and applied DSM-III criteria to the Stirling County samples. Do these match the results obtained by your original methods?

You refer, of course, to Jane Murphy Leighton, my wife and partner in the Stirling work. For professional purposes she used Jane M. Murphy as her name. She joined the Stirling Study in 1951 and has been with it ever since. The reason she was able to make the adaptation to DSM-III criteria was because DSM-III and our methods for evaluating cases were based on the same essential notions about clinical phenomena and how to describe them. In addition, DSM-III and our approach have similar ideas about how to group and classify symptom information. Thus the new results are a close match to the findings produced by the original methods.

And how do your findings compare to the results from the Epidemiologic Catchment Area study in the USA?

They are very similar.

But you used different terms, is that right? For instance, you said "evaluation" instead of "diagnosis".

Yes. What we meant by evaluation is what DSM-III means by diagnosis. DSM-I and DSM-II were similar to each other but different from DSM-III. The

earlier manuals involved making inferences about psychological aetiology. When DSM-III focused on clinical description, then, it wasn't exactly a miracle that there was agreement with the evaluation results because the behaviours and complaints under consideration are widely recognised in psychiatry. It turns out that clinical descriptions look like clinical descriptions whether from different parts of the world, different populations, or different clinics.

And then?

The major finding has to do with the social and cultural factors as aetiological agents. We don't have final answers to this, but we have uncovered correlations and non-correlations that can give directions to thinking and further exploration. Take culture for example. We went into the field expecting to find that people with different cultures would have different kinds and rates of disorders.

As I mentioned, there are in Stirling County two cultural groups – Anglophones and Francophones (Acadians). Each occupies about the same range of economic levels and range of occupational activities. To our surprise we found very little difference between these two cultural groups. There were some minor differences but nothing major or striking.

It occurred to us then that perhaps we needed greater cultural differences to bring out the power of culture. That led to work in Nigeria and also among the Yupik in Alaska – the latter carried out by Jane Murphy. But again culture did not make as much difference as expected.

Did people believe that?

Many did not and do not. Some remained convinced that culture must be a powerful force. Not that we felt the influence of culture had been disproved. Rather it appeared as more subtle and less clear as an aetiological force. On the other hand we had not mobilised enough evidence in Popper's terms to reject it. What we had was evidence that the four different cultural groups we studied did not differ much from each other in the prevalence rates of mental illness.

The third major finding was in sharp contrast to the uncertainty about the effect of culture. This consisted in figures to show that social and economic factors had very strong correlations with prevalence rates. The higher up the socio-economic scale, the less frequent were mental illnesses; the lower, the more frequent. The effect of these findings was to direct our attention towards the economic and the social and to the effects of stressful factors related to social disintegration which often goes with poverty. We also had an opportunity to look at disintegration

effects on a civilian population in war-torn Vietnam and again found very high prevalence rates.

Why didn't people like the three sets of findings?

Well, the high prevalence rates were threatening to the psychiatric establishment because they suggested the need for major renovations in theory, practice, the way services are provided, and the way training is conducted. The cultural findings were not popular among those with strong convictions about the power of culture, and the poverty findings made those people uneasy who were strongly concerned with the rights of the poor. While they welcomed evidence that poverty was damaging they did not like to think of that damage as being mental illness. It smacked of blaming the victim whereas their belief was and is that the fault lies with society. So, many prefer to think that while psychological difficulties may occur more among the poor than elsewhere in society, what we and other epidemiologists are calling mental illnesses are not in fact really mental illnesses, but normal reactions to abnormal circumstances. I think work like ours may have helped provoke an effort to redefine mental illness in relativistic terms and somehow that became an urge to redefine mental illnesses out of existence by placing it almost entirely in the eyes of the beholder.

Has your work had any actual impact on how psychiatrists are trained?

I wish I knew the answer to that. My guess would be that the work of all epidemiologists considered together is beginning to have a little influence.

What would be the direction of the work in the future?

So far I have been talking about prevalence. Under Jane Murphy's leadership we are now beginning to look at incidence – that is the rate at which new cases emerge in a population and the factors that are associated with variation in rates. We are beginning to get at the origin of mental illness and also through long-term follow-up studies we are getting data on outcome.

Can we go back to social disintegration? I recall you mentioning a settlement that was poor and much disintegrated. Change came about and the settlement became better organised, and with that the mental illness rate dropped.

Yes, that was a study that lasted some 12 years. At the start the settlement was given what might be now

called a mental health promotion and treatment. At the time in the early '50s we talked about community development and tried to catalyse economic improvement, social reintegration and education. At the end of 12 years the mental illness prevalence rates went from being the highest in the county to being about average.

Do you think your mental health promotion caused this?

How can you tell on the basis of one study in one community? All we can say is that other similar settlements that did not get the community development treatment did not show a shift in the prevalence of mental illness rates.

Did you try to repeat that experiment in other settlements?

Yes, we did but we have not yet completed the analysis. What we have so far is a theory which says that social and cultural disintegration increases the stresses people experience and reduces the resources for coping and that this leads to high rates of at least some kinds of clinically recognised mental illnesses in the population. The theory also says that a change from socio-cultural disintegration to integration will result in a reduction in prevalence rates. We have one case illustration of how the theory might work if it is correct. As things stand it needs replication and development.

You were going to talk about incidence?

Yes. As you know, incidence is the rate at which new cases occur in a population among people who have never been ill before. The results here again were something of a surprise. In contrast to the prevalence rates, the incidence rates have turned out to be surprisingly low. Let us take two specific disorders: depression and anxiety. These are common enough to permit good tests of significance. Over a 16 year period their prevalence rate has been about 120 per 1000, while their incidence rate has been about 10 per 1000 per year. This means that high prevalence rates are mainly due to the long duration and recurrent tendencies of depression and anxiety. This appears to hold for treated and untreated cases.

How have these results been received?

Unhappily. They indicate that many cases are not getting effective treatment or any treatment. They

also indicate that even a successful programme in primary prevention could not have much effect on prevalence rates for at least a generation. This suggests that more immediate effects could be had by successful efforts to shorten duration and prevent relapses. If these two could be accomplished, prevalence rates would be reduced very markedly and very promptly.

Are you talking about severe depression or any of the more mild, more or less, neurotic types?

Both. Of those who were depressed at the beginning of our study, 82% had a poor outcome. By that I mean that 16 years later they were either still depressed, depressed again, or dead.

Dead?

The mortality rate among the depressed – death from all causes – was about one and a half times the expected.

Controlling for age and all that sort of thing?

Yes. Mild depression according to the data is a serious illness that not only shortens life but produces chronic disability and destroys its qualities. But it is important to keep in mind that although we are talking about an illness that has a high prevalence rate, we are still talking about a minority in the population. As Jane Murphy likes to sum it up, there is at any one time a minority of ill people and a majority who are well or more or less well. When these are looked at for a period of 16 years the ill tend to stay ill and die off. The well tend to stay well.

You said earlier that one of the reasons for undertaking the Stirling County Study was to make a study of non-ill people. Have you been able to do it?

Not as much as I would have liked, and some of the reasons for that are interesting. It is partly because the task of validly identifying illness in a population-at-large proved so time consuming. In fact it took many years and a considerable change in the nosology of clinical psychiatry before a reasonable degree of consensus about criteria began to prevail among psychiatric epidemiologists. We are only now getting into a position to look at the nature of health and its correlates.

A second factor of importance has been a general lack of interest in studying the nature of mental health on the part of funding sources, and I must add on the

part of many clinical psychiatrists as well. There seems to be a widespread assumption that we know what mental health is. Can you imagine pathologists so focused on pathology that they never gave attention to the study of physiology? Perhaps now with the rising tide of interest in health promotion, things will be better in this regard.

Can you give me any examples of findings with regard to health?

I have mentioned one: the well tend to stay well. Most people appear to have enough of what Rutter calls “resilience” to withstand considerable stress.

I have heard you say that you think there are at least three types of wellness. Could you explain that?

Perhaps it would be better to say that I have the impression that there are *three types of non-illness*. You realise that this is a clinical type impression based on rather few people examined in any detail. It goes like this: of the 20% of the population who seem particularly devoid of symptoms in a prevalence study, one sub-group is composed of people who have rather dull and listless personalities. They plod along, not much engaged with life and not caring very much about the things that bother other people. Love, fear and hate do not seem to run deeply in them. The second group are personalities who do give evidence of some vulnerability, but who also have strong capabilities. They get themselves into social roles which protect their vulnerability, leaving them free most of the time to deal with the world through their capabilities. They are a little like the hermit crab who backs the tender part of his person into a whelk shell and faces the world with his able snappers. I have the impression that should these people be forced out of their protecting roles and find nothing comparable immediately available, they might increase the mental illness rates in a socially disintegrating society.

The third group we call the “Elizabethans”. Their outstanding features are vigour and an active interest in life. They care and react when things go wrong, but they don’t get stuck in negative emotions. After a reasonable time, they regain their positive feelings and carry on.

A second notable feature is their ability to draw satisfaction out of what they do and out of the people and events around them. They seem to get “reinforcements” and pleasure out of things other people ignore and they have these experiences several times a day. A third attribute is that they have foresight and are pretty good planners.

Coming back to your overall picture of prevalence and incidence, what does this tell us about how to raise the level of mental health?

I think we have to keep in mind that the picture we have of these things is still unfolding as the research goes on around the world. A few years down the road it may all look different.

As of now, however, I would put emphasis on reducing relapse rates and on reducing the degree of disability among the chronically ill. This is a change in my opinion due to epidemiological findings. Forty years ago when we were starting, I had high expectations from primary prevention. Now, I am impressed that the incidence rates are so low in most kinds of disorders that it would take many decades before any effect could be visible. The high prevalence rates appear to be due to chronicity and relapse. In our data some 30% of the prevalence rate is due to relapse.

If we could shorten duration and eliminate relapse, we would have a major impact on the mental health of the population. All the talents of psychiatry and the mental health professions are needed for that. I don't see much evidence to support the notion expressed by some mental health promoters that making the healthy healthier will affect the mental illness rates. It is like fighting poverty by making the rich richer. The comparison is a little unfair, but I am afraid there is something to it, nonetheless.

And if in addition we could cut short duration?

You would have an immediate affect on the prevalence rate.

Much of your life seems to have been spent telling clinicians, educators and governments things they do not want to hear. How do you manage to be popular and sought after as a consultant?

I thank you for the compliment, but I wonder if it is so.

I hear what you say about plans for further analysis and new fieldwork at an age when you ought to be dozing in front of the TV. What is it you would like to accomplish?

The reason I am able to remain to some extent productive is my partner. Jane Murphy has taken charge of the research and I am now the one who helps out. In addition to the current field phase for the 40 year follow-up on the original study, I should like to con-

tinue participating in the analysis of the data already on hand.

I am also turning to several writing projects I have had on a back burner for some years. There are, in fact, too many to ever finish, but I am trying to line them up in some kind of priority and then push on.

Such as?

A major one has to do with trying to build more effective bridges between psychiatry and the social and behavioural sciences. I have lived most of my professional life in the combat zone among those fields, so it is possible that some of my observations might have value. During the 50 or so years on which I can look back, progress in the scientific study of human behaviour has been disappointingly slow and reversible. There was a burst of effort and success during and after World War II, but it faded out in the '50s and in a most amazing way. The British sociologist, Dahrendorf, has said that "history proceeds by changing the subject", rather than through developmental stages. There are signs now, however, of what may be renewed vigour.

As I look back on the scene unfolded by the years, I think a root problem is man's reluctance to look at himself objectively. He doesn't want to be an object of science and this feeling runs through members of our profession as it does among our patients and the public generally – and among many scientists too. We don't like anything that might cut into the idealised image we have of ourselves.

When I entered this field, most people in it thought of the study of human behaviour as a way to enrich life, examine it deeply and give it more meaning.

Now the issue for most thoughtful people is survival. Understanding human behaviour and nature is a possible chance for survival in a world of overpopulation, nuclear weapons and pollution. We have to learn to live together if we are to live at all and that means giving up some freedoms for the sake of a cooperative way of life. But cooperation means restrictions, and so far mankind does not want to face that. We are in a phase in which we are infatuated with the individual and his rights and I dread what may happen when overdoing this provokes a swing to the opposite extreme.

There is a chance, but no guarantee, that scientific understanding of human behaviour would open pathways into patterns of living that would save us from wasting and destruction. So far in the history of the world, neither religion nor secular philosophy has been able to divert mankind from massive destruction and monstrous cruelties. Applying science to man's behaviour is the one thing that has not so far been seriously tried.

It seems to me that there are important possibilities for scientific bridges between psychiatry and the social and behavioural sciences, as well as with


neurobiology – possibilities that are in keeping with what we know about behaviour and its well springs. It seems worth trying, anyhow.

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