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Considerations on Research in Family Therapy:
An Interview with Rudolf Moos

Graham Martin*

Rudolf Moos, Professor of Psychiatry, Stanford University School of Medicine and Research Career Scientist at the Palo Alto Veterans Administration Medical Centre, is not well known to Australian and New Zealand therapy circles, yet his research into instruments applicable to research into the family and its dynamics is prolific, his personal energy, integrity and dedication are admirable and his persistence at researching the very difficult areas of social environments is unequalled. This interview was recorded at the Social Ecology Laboratory at Veteran's Administration Medical Centre at Palo Alto, where he is Director of the Laboratory.

Graham Martin: *Your work seems to have very much broadened out since the mid-1970's. There have been the Family Environment Scale, the Work Environment Scale, and the Ward Atmosphere Scale. Now you are looking at a comprehensive Multiphasic scale. How did that occur for you?*

Rudolf Moos: Okay. It actually went a little bit the other way. My initial interest was in treatment environments and that started back in the middle sixties when I was interested in the variability of people's behaviour in different settings. I came out of the Berkeley Psychology Department with a pretty dynamic personality orientation; with the idea of trait orientation and consistency of behaviour across settings. In my clinical work I had a lot of trouble predicting people's behaviour in settings other than my office. To put it simply, *(laughs)* when people were released from hospital they weren't doing what I thought they were going to be doing.

Graham: *Or even what they told you they were going to do.*

Rudolf: Exactly. I was a naive young clinician. That got me interested in the evidence for cross-situational variability and I started off doing a couple of studies, very early on, on some important clinical concepts like, for example, therapist empathy as rated say by the Truax and Carkhuff scales, and that's how I got interested in treatment environments. I was already at the Veteran's Administration and Stanford at the time and there happened to be a clinic where the patient would see a different therapist each week; they were trying to build 'counter transference'

*Lister House, 4/142 Ward Street, North Adelaide, 5006.

(as a concept) *to the Institution.* I imposed some experimental design on that and by rating tapes of transcripts we looked at how empathic a therapist was across different patients, and how much a patient who saw different therapists could 'elicit' therapist empathy; you know, looking for the situational aspects of it.

My interest was to identify the extent to which therapist empathy (as rated by these Truax and Carkhuff scales for the Rogerian concept) really is a trait. If the therapist sees say 4 or 5 different patients and you have several therapists then you find one therapist is almost always higher in empathy than another therapist. To some extent it is a trait. Some therapists *are* more empathic than others. But we also know (and this is more what I was interested in just because it was less emphasized) that some patients elicit more empathy across therapists than others do. This study was trying to support that point. Is it the case that it is easier to be empathic with some patients than others? In this unusual clinic a patient sees two, three, four therapists in different weeks; do all the therapists show more empathy with one kind of patient than with another?

I like that study; it was published in 1967 (Moos. R. & Clemes, H., 1967); it is almost twenty years old now. Anyway it and several other studies looked at the situational variability in behaviour in a number of real life situations. A couple of them were done on psychiatric wards and that got me more and more interested in some clinical experiences I had had on psychiatric wards, and in the issue of treatment environments. This led to the research on the Ward Atmosphere Scale and the Community Oriented Programs Environment Scale; two parallel treatment scales. That work was done primarily in the late 60's and early 70's. Actually, a book on evaluating treatment environments came out a bit later (Moos, R., 1974).

I spent a year in England collecting cross-cultural data on both those scales, and saw a lot of different psychiatric facilities there, although I was based at the Maudsley. From there the concept of social climate, the measurement of the social environment, became very intriguing to me and to a lot of other people. We began to get interested in extending the idea to develop measures of other kinds of social settings — correctional settings and educational settings such as classrooms — and we got involved in scale development in each situation. The scales are a little different because each environment is different; you obviously can't just take the scale for wards and apply it to classrooms.

Out of the experience we had over several years, in the early seventies, we began to get interested in extending our thinking vis-a-vis work and family; that's actually how the development went.

What happened at that point was I began to be concerned abut two issues. On the one hand, we had measured social climate and had pursued the logic of trying to assess the social environment in a kind of soft, subjective clinical way simply by asking people about their reports of that environment, essentially a phenomenological approach. I would still say today, that this is a very very valuable way of looking at the environment. But what was happening was that a lot of people were saying: "Why don't you look at more 'objective' aspects of the environment?" and I myself had that feeling.

I also began to get interested in why it is that one treatment programme is so much more cohesive than another, or one work environment so much more cohesive or more task oriented or less controlling than another. What 'causes' differences in the climates? Well, those two questions came together; the causes of the differences in the climates and the issue of looking at other more objective ways of measuring environment led to the Multiphasic Environmental Assessment Procedure, which we have operationalized in one broad type of setting — sheltered care settings. The conceptual logic is that we measure the social climates of places such as nursing homes, but also a broad range of other residential settings for older persons. What are the 'determinants' of the social climate? What are the environmental factors that help to determine (determine is perhaps a little strong) or influence the social climate?

Graham: *That makes me think of Dan Offord's research work in Canada. Each five years, apparently, headmasters in various schools move on, and researchers can track what happens in terms of not just academic standards in the schools, but social standards, interaction with teachers, the amount of social disruption in the community. His research found, I understand, that the 'stronger' the headmaster (the more he believed in rules and organization, school uniforms, no eating on the buses, and so on), the less trouble there was in the community. There was a lessening of, for instance, the amount of juvenile delinquency. Is that the sort of thing you are talking about?*

Rudolf: Well, that is probably going further; you are talking about one portion of it, I think. I don't know whether it is fully true, but let's put it this way: strong headmasters establish policies which develop school social environments that are somewhat more task oriented, highly organized and structured and, in turn, those headmasters, in conjunction with the social climates they establish, change the direction of the school and of its influence on students and perhaps even on others in the community.

Graham: *Yes, that is certainly what he was suggesting.*

Rudolf: Yes, I don't know that work, but it is going two more steps and it is always a little hard to make those linkages. What we did in the Multiphasic approach was to measure the policies established by a facility, but also the physical environment. The idea is that physical features should have an effect on climate also. If you have physical features that facilitate social interactions you may be more likely to have a cohesive climate. We also measured what I call 'the suprapersonal environment' or 'aggregate characteristics' of the people in settings, which often affect the climate quite directly.

Graham: *What do you mean by that?*

Rudolf: Well, I think it is one of the most underemphasized ways of looking at environments; so there is a lot of work to be done! It is the census tract logic of the environment. Basically, 'suprapersonal environment' is the average characteristics of people in a setting. It is a little hard to apply to families because the family is too small a unit. It applies to work environments, schools, and treatment programmes. Those average characteristics, to some extent, shape the type of social climate that develops.

Let me give you an example. In universities there is the issue of selectivity. Universities vary on the average intelligence and/or ability levels of the students they attract. Take a university where the average ability level of the students is very high. Such a high ability university is likely to have a more task oriented, competitive, but less cohesive climate, because people are more oriented toward achievement and independence. In turn, a student who comes into an environment where the average ability level is very high, and that kind of climate is established, is much more likely to feel low self esteem. Why? Because the comparisons they make are to people who are highly competent, so essentially they wonder whether they can really keep up and wonder how good they really are.

So that is an example where an average characteristic of a setting can have an influence on the type of social climate that is established and on the psychological reactions of the individuals in it.

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Graham: *As I mentioned in our correspondence we are considering a family therapy unit at a 44 bed psychiatric hospital. Specifically what I wanted to talk to you about was the Family Environment Scale. Medicare in Australia at the moment, as in the United States I believe, will not reimburse whole families in hospital unless each of the family members has some sort of 'designated illness'. So we are fighting a battle at the moment to get the family recognized as the treatment unit, and I guess we are going to have to argue about the relative merits of family therapy versus individual therapy and family therapy assessment methods versus individual assessment methods.*

Rudolf: In an inpatient setting?

Graham: *In an inpatient setting — that's the problem! Certainly we want to start out, as it were, on the right footing, measuring what we are doing with as many families as possible and if possible from the very first family that came into the hospital. It seemed to us that your work with the Family Environment Scale might be suited to what we are doing because it is relatively simple to give, it is relatively simple to look at, it is relatively simple to understand, and one can almost visualize what comes out of it.*

Rudolf: Well, I don't know whether most family therapists would feel quite that way. The Family Environment Scale is fine, it measures important aspects of families, such as the quality of interpersonal relationships (cohesion, expressiveness, conflict), the orientation toward personal growth (in such areas as independence, achievement, and the pursuit of intellectual, recreational and religious interests), and how well the family is organized and structured. However, it does not get at what one would call the deeper dynamics of family life; it is not intended to get at that. But in terms of a screening procedure it is a way of getting some handle on how family members perceive their family. It gives some idea about the phenomenology at the broad level of the family. However, it is not going to get at the kind of depth of understanding Lyman Wynne wants to get at, and if you talk to somebody like George Brown or Julian Leff, for example, they will say that there is a definite limit to what questionnaire techniques of this sort can really do.

Graham: *I think that is fair enough, because I know that when I am teaching family therapy I use the kind of parameters that exist in the Family Environment Scale (FES), but my list gets expanded to at least 15 instead of 10 and there are some specific things like power, boundaries, affect, which are implied in the FES but which are not necessarily measured.*

Rudolf: Yes.

Graham: *And yet therapeutically they are important, would you not agree?*

Rudolf: Well, we might have to talk about each concept. Take *affect* first. It is not that the FES doesn't tap some aspects of affect, it just does not get at all the details of the affect that exists. Certainly it is valuable to know in general whether one or more family members see their family as cohesive or whether they feel they belong together, but at another level it will not tap all the nuances of what happens when father talks to adolescent daughter and so on.

Power, I think, is a difficult one. It does get at some elements of power.

Graham: *Yes it does. I guess being a clinician I am aware of what is useful to me. Therapeutically, I use some of Jay Haley's concepts with regard to power in the family, and strategies based on power. I don't think that the FES necessarily pinpoints the changes I might want to be able to measure when I am working in this way with families.*

Rudolf: I would agree, it would not if you want to be tied to Haley's concepts.

Graham: *May I ask you about test-retest reliability? One of the things I would be wanting to look at is whether a given form of therapy in a given environment is creating sufficient and lasting measurable change in these particular families.*

Rudolf: Yes. In a global sense, actually the FES is not bad. You don't want to only measure small 'behavioural changes' that may fluctuate a lot from moment to moment, you also want to measure something about the global feeling about the family. Okay. Test-retest reliability. We have done studies of patients in treatment for alcoholism and different patients in treatment for depression. We have compared them in order to understand the changes that go on with and after treatment. We compared them with demographically comparable

non-alcoholic families who do not have an alcoholic or depressed member. For these average, stable families, the FES test/retest reliability — 'stability' let's call it — over a years time is fairly high. You would end up with individual subscale correlations, if there is enough variability in the subscales, somewhere between 0.5 and 0.75. In our manuals we report a 4 or 6 week test/retest reliability in that range, but even over long periods of time the dimensions we measure tend to be fairly stable with stable families.

Graham: *If we were trying to make political points with the kind of research that we might want to do, would you recommend that there in fact be three measures? For instance, if eventually you wanted to say something like: "Family therapy with this group, which we have matched with another group who are not having family therapy, seems to be effective in making not only measurable changes in such things as hospital re-admission rate, but also in terms of these given family dynamics* and *the general family functioning as far as can be measured by the FES".*

Rudolf: If you want to make political mileage for funding agencies to be interested, you have got to have 'objective evidence'. I put that in quotes because we all know that re-hospitalization is subjective; it's objective at one level but not at another. But, yes, that's the information you have got to have.

However, we were more interested in the conceptual issues than the political ones. Why is it that families that have been in one treatment programme perhaps have less re-hospitalization, more positive behaviour change, and better functioning children? Now you immediately get into issues of mediating factors. It is simply that one wants to begin to understand what mediates the changes that occur in family behaviour and family climate that may relate to the so-called 'objective climates'. That wouldn't I think, give you political mileage; that gives you academic conceptual mileage! *(laughs)*

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Rudolf: Currently I am working on revising the manuals for the Group, Work and Family Environment Scales. I guess they will be published soon. (Moos, R. and Moos, B., 1986.) I am just trying to review the mass of literature, research conducted by other people; trying to make sense out of it. One interesting and frequent use of the FES is to identify differences on some FES scales between demographically matched groups. Another is to examine how initial personal and family environment factors relate to psychosocial outcome.

For example, take this situation. It is important to know if there are average differences in the family environment between families with an alcoholic member and 'normal' families, or even possibly about average family environments of families with an alcoholic versus those with a depressed person. Even more interesting in the long run is to know that, let's say, average cohesion or higher than average cohesion in a family with an alcoholic member is predictive of better treatment outcome. Put a different way, below average cohesion on the FES for a depressed patient at treatment intake predicts that the patient is less likely to do well at a one year follow-up. That is a finding we have.

That kind of finding sensitizes you, in effect, to what might be an indication for family treatment. But it also may sensitize you to what might be an indication for a particular way of going about that treatment. In that sense the FES is like an MMPI.

Graham: *It is just a bit easier to handle!*

Rudolf: Well, you know it is shorter, yeah *(laughs)*. In a sense, the FES is a screening device that cues you into issues that probably, as a clinician, you might know anyway, but it provides something 'objective'. Secondly, sometimes there is something about the profile that is different than what you thought and actually I think that both in clinical use and in research when it is different than what you thought . . .

Graham: . . . *That is when you start to be interested!*

Rudolf: Exactly! That is an opposite approach to the validity question. People say: "If it is different, then that means it is not valid!" I think when it is different, *that* is when it is most interesting!

Why is it different? What is going on? Why didn't I see this? Is there something wrong in this case? Is the scale not giving me something accurate?

Graham: *In American family therapy there seems to have been an historical development through the different forms of family therapy. You seem to have gone through a 'communication' period, a 'communication and affect' period, through a 'communication and power' period, through a 'Milan systemic' period and now it seems to be shifting somewhere else. At the moment with what Maturana and people are saying you have almost got a kind of 'No-therapy'. Along the way there seem to have been people who have been left who have specific expertise in, let's say, a communications style of family therapy. Are you saying that at some stage it might be possible to use a scale like this to predict which sort of therapy or which sort of therapist might in fact suit a given family better?*

Rudolf: Well, I would say to predict part of that outcome. We know clearly that neither the FES nor any other single scale will be able to predict outcome that well. But one might think of it in the risk and resistance factor model, vulnerability factors, the George Brown depression model. In that sense one can take the FES and say: "I can tell you from a profile on a probability basis something about how well a patient is likely to do later on." Yes, given the right research, I think a scale like ours can be used for that. But you are asking a more complicated question about differential outcome with different treatments. That, of course, is what everyone is interested in. A lot of people in the Laboratory here are particularly interested in the matching question (that is the matching of patient families to a particular therapist or style of therapy), and as you know we are simply not very far along in understanding that issue yet.

Graham: *And you are years and years ahead of us, I think. Can I come to a more general question? In a recent Networker, Salvador Minuchin is reported to have said that with the increased acceptability of family therapy to the Establishment, it may be beginning to lose its creative edge and urge. Where does research fit into that particular situation? Do we research away . . .*

Rudolf: . . . the creativity . . .

Graham: . . . *the creativity or the kind of excitement that seems to have been generated in the field over the last twenty years? You must have sat back and watched all of this process?*

Rudolf: Yes, you know, I have watched the process. I have also been just a little peripheral to it and so . . .

Graham: *That's supposed to help objectivity.*

Rudolf: Objectivity is supposed to help. Yes, I think you are correct. There have been tremendous waves of enthusiasm, but in the field at the moment there is a little bit of a downer, a kind of searching for new ideas. It is in a low phase at the moment, but how long that will last is hard to know.

Graham: *Perhaps it is part of the natural cycle of events.*

Rudolf: Exactly. Whether that *is* just the natural curve of things and it will return . . . There is a general tendency as new fields become more established for them to become more detailed, more structured perhaps, less expressive. And we may be seeing some of that.

I think the research tradition does contribute to making things just that bit more structured, that bit duller and more methodological. However, I don't think you would have, say, Alan Gurman and many other people in this field if there were no research perspective! They have established and helped to establish some of the research traditions in the field. I think the biggest advance we have seen is that people who are doing research in families (not only family treatment but on families in different areas), are more likely to cite each other's work at the first level, talk to each other at the second level, and begin to do some kind of collaborative work at the third level and . . .

Graham: *You mean rather than standing back and sniping at each other (which seems to have occurred in the field of family therapy), you are saying that research may begin to draw people together?*

Rudolf: I hope so. My orientation has always been more cooperative. Well, looking for ways in which one can make connections between different theories and fields, let's put it that way, connecting both areas and people.

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