THE WISCONSIN WATERSHED – OR, THE UNIVERSALITY OF CCT

In memory of John Shlien

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Abstract

This paper argues that the major reasons for the ambiguous and disappointing results of the Wisconsin Project were the failure of the researchers to take client motivation into account and failure of the therapists of the project to respond on a level of concreteness that matched the client's level of expression.

The paper asserts that correcting for these two factors leads to the major hypothesis of the Wisconsin Project being, after all, true: Client-centered therapy does effect therapeutic change in persons diagnosed with schizophrenia. This result strengthens the hypothesis that client-centered therapy is a universal therapy.

John Shlien contributed in many ways to the author's critique of the Wisconsin Project. The paper is also the history of his contribution.

The purpose of the present review of the Wisconsin Project

My purpose with the present paper is to apply the evidence from diverse experiences with Client-Centered Therapy, which has been accumulated after the end of the Wisconsin Project, to shed new light on the conclusions of the project.

The paper is also my way of honoring the memory of John Shlien, who meant much to me, and with whom I shared my concerns about the Wisconsin Project.
A short introduction to the Wisconsin Project

In 1957 Rogers accepted a post as professor at the University of Wisconsin. He moved there from the University of Chicago and the Chicago Counseling Center, much to the surprise and sorrow of most of his co-workers and students in Chicago (Thorne, 1992, p. 16), with whom he had made such fertile work in consolidating and researching the theory and practice of client-centered therapy – work for which he was honored with the American Psychological Association’s Distinguished Scientific Contribution Award.

According to Thorne (ibid. p. 16) the motivation for this move was Rogers’ wish to research the applicability of client-centered therapy with a psychiatric client population, particularly patients diagnosed with schizophrenia. There was ample opportunity to do this in Wisconsin, because Rogers was not only appointed professor in the department of psychology, but also in the department of psychiatry, which had the Mendota State Hospital as its field of practice. Also, according to Thorne (ibid. p. 16), Rogers wished to gain appreciation for client-centered therapy within the circles of academic psychiatry.

In 1967, Rogers and his co-workers published the book about the scientifically impeccably, and ingeniously, designed large-scale research project, which Rogers headed in Wisconsin. The project investigated the effect of psychotherapy on a group of “normals”, a group of “acute schizophrenics”, and a group of “chronic schizophrenics”, compared with matched and paired controls who did not receive individual client-centered therapy.

Both this book, and other commentaries on the Wisconsin Project (Thorne (1992) and Shlien (1992)), bear witness to Rogers’ disappointment with the results of the project.

Brian Thorne writes (1992, p. 17): “Rogers was later to describe the project as “without doubt the most painful and anguished episode of my whole professional life”.”

John Shlien writes (1992, p. 1084): “Really, it was a watershed for the man, theory, and movement – a point when both means and ends were bent out of shape.”

Rogers, himself, writes (quoted by Shlien, 1992, p. 1083-1084): “Our recent experience in psychotherapy with chronic and unmotivated schizophrenics raises the question whether we must modify our conception of this condition. Very tentatively it appears to me at the present time that, in dealing with the extremely immature or regressed individual, a conditional regard may be more effective in getting a relationship under way, than an unconditional positive regard”.

In the following, if only reference to page(s) is made, it is a reference to the book by Rogers et al. (1967) about the Wisconsin Project.
Survey of the hypotheses investigated in the Wisconsin Project.

On pp. 17-19 Rogers lists the hypothesis investigated in the Wisconsin Project. The following list extracts the main points from these pages, those points that Rogers put in italic:

1. "The greater the degree to which the conditions of therapy exists in the relationship, the greater will be the evidences of therapeutic process or movement in the client." (p. 17)

   The corresponding, implicit, hypothesis, (1A), with respect to outcome (as opposed to process) reads: "The greater the degree to which the conditions of therapy exists in the relationship, the greater will be the evidences of constructive outcome." (p. 19)

2. "The same variables of process movement will characterize the in-therapy behavior of more acute schizophrenics, more chronic schizophrenics, normals, and neurotics." (p. 17)

   The corresponding, implicit, hypothesis, (2A), with respect to outcome, reads: "The greater the evidences of process movement in the client in therapy, the greater will be the evidences of constructive outcome." (p. 19)

3. "The process of therapy will occur to a significantly greater degree in a group of individuals to whom therapy has been offered than in a control group, paired and matched to the therapy group, to whom therapy has not been offered." (p. 18)

   The corresponding, implicit, hypothesis, (3A) with respect to outcome, reads: "Evidences of positive outcome will be greater in the group of individuals to whom therapy has been offered than in a control group, paired and matched to the therapy group, to whom therapy has not been offered." (p. 19)

John Shlien's contribution to the paper

I received the news of John Shlien's death a few days after I had started to write this paper. It was John who made it possible for me to write it. He knew that I felt as troubled about the Wisconsin Project, as he did, and for more or less the same reasons. Therefore I think he sent his old and well-worn copy of the book about the project (Rogers et al., 1967) to me as a gift two months before he died. Without the opportunity to read that book closely, to mark quotations and make comments in the margin, I could not have written the present paper. Furthermore, my reading of the book was immensely enriched by John's own comments in the margin. I mixed my own comments among his with a sense of our e-mail dialogue about the Wisconsin Project continuing, and John still being there for me, to feel supported by and to
learn from. In a very real and significant way, John is a co-author of this paper. He raised the questions I have tried to answer. I think I have done it in a way, which would have sat well with John.

Here are John's questions (Shlien, 1992):

The real question is what to do when a vaunted project is failing, when patients are not voluntary, self-initiated clients with free choice, but are unwilling and unresponsive. Should we declare that the therapists are inadequate, that the population is unsuitable, or that the theory is wrong? Is the test too severe? Should the practice be changed in a way that violates the theory? Should the theory be changed to accommodate the practice? Really, it was a watershed for the man, theory, and movement — a point when both means and ends were bent out of shape. With that denied, now could they recover?

I shall make a case for the following answers to John's questions:

1. Yes, we should declare that the therapists were inadequate.
2. Yes, we should declare that the population was unsuitable.
3. No, we should not declare that the theory is wrong.
4. Yes, the test is too severe.
5. No, the practice should not be changed in a way that violates the theory.
6. No, the theory should not be changed to accommodate the practice.
7. Yes, without denial, they could recover.

Comments on the conclusions from The Wisconsin Project

On pp. 75-93 Rogers reviews the conclusions he and his co-workers drew from the findings of the research. The margins of these pages, in John's book, are heavy with his comments. I'll comment on the conclusions of the Wisconsin Project and, throughout, I'll also quote from John's comments in the margin of his book. (I like to think that he meant me to and John's wife, Helen, has kindly permitted me this favor).

I'll proceed in a systematic fashion, by restating each hypothesis with its corresponding comments and conclusion.

1. With respect to hypothesis 1 ("The greater the degree to which the conditions of therapy exists in the relationship, the greater will be the evidences of therapeutic process or movement in the client." (p. 17)), the following conclusion was drawn (p. 82):
"In general there was no differential amount of process movement over therapy in our schizophrenic group as a function of therapy". Rogers writes (p.82) that this finding "was disappointing".

On p. 83 Rogers writes: "With our normal individuals, who were not motivated for therapy, there was even a trend toward a more superficial level of experiencing in second halves of the therapeutic hours. This finding in the normal individuals seems explainable on the basis of their tendency to execute a defensive retreat from therapeutic engagements". I suspect that many individuals in the schizophrenic group, out of lack of motivation, did exactly the same, and that the conclusion reflects on degree of the client's motivation, rather than on anything, which is specific for individuals with diagnoses of schizophrenia. I'll expand on the issue of client motivation, later.

1A. With respect to hypothesis 1A ("The greater the degree to which the conditions of therapy exists in the relationship, the greater will be the evidences of constructive outcome." (p. 19)), the following conclusion was drawn (p. 86):

"Those patients who perceived a high degree of congruence in their relationship with their therapist were independently rated as showing the greatest degree of change. Those whose relationships were rated high in empathy ranked next. The control group followed, and those patients who were in relationships low in empathy and congruence showed no change or even regressive change". Summarizing this, Rogers concludes (p. 86): "Relationships rated high in a sensitively accurate empathic understanding and high in genuineness as perceived by the patient, were associated with favorable personality change and reductions in various forms of pathology, particularly in schizophrenic pathology." This might be read as supporting evidence for the universality, with all kinds of clients, of the core conditions in bringing about therapeutic change. In the margin, however, John comments: "What about another theory: Someone who relates poorly to people and is forced to be with another person will evoke relatively non-therapeutic behavior in him and will act more disturbed. Someone who relates well will evoke therapeutic behavior and will act better. With less intense contact such people on the average get better." (I think John, in the last sentence, refers to people diagnosed with schizophrenia, since it is also my experience that people diagnosed with schizophrenia, compared to other people, generally respond better in relationships where the intensity of contact is low, and that this is the type of relationship they often invite one to enter into). John's overall intention with his comment, though, is clearly to warn against the mistake of confusing a correlation with a cause-effect relationship. Rogers is well aware of this and states (p. 89-90) that: "We cannot, from the findings, conclude positively that these causative hypotheses were proven". Rogers continues: "When we examine the characteristics of those schizophrenic patients who were in relationships high in the attitudinal conditions, as compared with those who were in low conditions relationships, we find differences, some of them significant... High levels of empathic understanding, genuineness, and warm acceptance in the therapist's behavior are more likely to be evident when he is dealing with a reasonably expressive individual with a socio-educational level closer to his own. The therapist's attitudes are clearly important, but the patient's
characteristics appear to play a definite part in eliciting these qualities. High therapeutic conditions seem to be a product of interaction between the person of the therapist and the person of his client”.

I think there are two factors at play, which are pertinent to the conclusions quoted above.

First, it seems evident to me, that the more like themselves (the therapists), or, alternatively, the more like the original client population of client-centered therapy, the better was the outcome for the client. Not only these quotations, but many others as well, (and I shall expand on this issue later) seem to tell a story about therapists who were very inexperienced with people diagnosed with schizophrenia, and ill prepared for the difficulties one encounters in therapy with such people.

Second, in these quotations, Rogers actually foresees the results of much later research of psychotherapy outcome. Jerold Bozarth (1998, p. 168), for example, sums up this research as follows: “...reviews of outcome research (Lambert, 1992; Lambert, Shapiro & Bergin, 1986) suggest that 30% of the outcome variance is accounted for by the common factor of the client-counselor relationship across therapies, technique accounts for 15% of the variance as does placebo effect and 40% of the variance is accounted for by extra-therapeutic change variables (factors unique to the client and his/her environment)”. This research clearly points to the major importance of some significant client variables for the outcome of psychotherapy, just as Rogers hypothesized in the quotation above.

2. With respect to hypothesis 2 (“The same variables of process movement will characterize the in-therapy behavior of more acute schizophrenics, more chronic schizophrenics, normals, and neurotics.” (p. 17), the following conclusion was drawn (pp. 75-76):

“Regardless of the degree of understanding, acceptance, and genuineness offered by the therapist, schizophrenic patients tended to perceive a relatively low level of these conditions as existing in the relationship, and only slowly over therapy did they perceive somewhat more of these therapist attitudes... Neurotic clients appear to perceive primarily the understanding and genuineness of the therapist and thus it is natural that their central focus appears to be on self-exploration. Our schizophrenic patients on the other hand perceived primarily the levels of warm acceptance (positive regard) and genuineness. Their focus appeared to be on relationship-formation” Further, Rogers writes (p. 90): “The major change which emerged from our findings is that the schizophrenic initially focuses more on relationship formation than on self-expression, and thus some of the most characteristic elements of process for the neurotic are not initially present for the schizophrenic. Indeed, such self-exploratory behavior may never occupy as prominent a position in the therapy of the schizophrenic as it does in the therapy of the neurotic”.

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When Rogers wrote this, Garry Prouty (1994) had not yet developed his Client-Centered Pre-Therapy that is designed, precisely, to enhance relationship-formation, rather than self-exploration. The "contact reflections" of Pre-Therapy have been shown to enhance the more withdrawn schizophrenic patients' contact capacities, (sometimes) enabling them to benefit from ordinary client-centered therapy at a later point in time. The level of concreteness appropriate in empathizing with these patients, who do not seem to want the therapist to understand anything about them, is extraordinary, and was probably rarely reached with the withdrawn schizophrenic patients in the Wisconsin Project. This, I think, is a major reason for the conclusion quoted above and also for the conclusion that there was no significant difference in outcome between experimental and control groups (3A, below). This too will be expanded on later.

2A. With respect to hypothesis 2A ("The greater the evidences of process movement in the client in therapy, the greater will be the evidences of constructive outcome.") (p. 19), the following conclusion was drawn (87-88):

"The more successful cases showed a degree of process movement sharply different from the less successful cases. In general, these less successful cases actually showed negative progress, moving definitely lower on the process scale, becoming more remote from their experience, showing less expression of, and less ownership of, their feelings... We found that the process level exhibited by the client or patient was positively associated with many of the objective measures of outcome variables".

This finding supports Rogers' theory that good psychological adjustment is associated with "closeness to feelings, greater awareness of gut-level reactions, more use of these inner personal referents as bases for self-understanding and appropriate behavior" (p.88). Although, of course, important, this result does not bear, particularly, on therapy with individuals diagnosed with schizophrenia, and will therefore not be commented further upon.

3. With respect to hypothesis 3 ("The process of therapy will occur to a significantly greater degree in a group of individuals to whom therapy has been offered than in a control group, paired and matched to the therapy group, to whom therapy has not been offered." (p. 18)), the following conclusion was drawn (p.91):

"In general it [hypothesis 3] was not confirmed. There was little process movement in either group and the difference in process level between groups in the sampling interviews was not significant".

This, I think, is not surprising in light of the fact that the process variables measured were those more pertinent to the original client population of client-centered therapy, and that population's focus on self-exploration, than to a population of individuals diagnosed with schizophrenia, and this population's focus on relationship formation. Garry Prouty's research (1994 pp. 44-46) has documented significant process movement, compared to a control group, in a group
of schizophrenics offered Pre-Therapy, where process movement is defined as enhanced contact capacities, i.e. variables relevant to relationship formation. In addition, I suspect that the conclusion with respect to hypothesis 3 is also a result of therapists empathizing at too abstract a level with the clients of the group who was offered therapy. This point will also be discussed later.

3A. With respect to hypothesis 3A ("Evidences of positive outcome will be greater in the group of individuals to whom therapy has been offered than in a control group, paired and matched to the therapy group, to whom therapy has not been offered." (p. 19)), the following conclusion was drawn:

"In many respects the therapy group taken as a whole showed no better evidence of positive outcome than did the matched and paired control group. It had however a slightly better rate of release from the hospital, and this differential was maintained a year after the termination of therapy. The therapy group also showed a number of positive personality changes, which were not evidenced by the control group. The differences between the two groups, however, were not great." (p. 80)

In the margin, John asks, dryly: "I.e. small?" and when Rogers on p. 81 proceeds to list the positive personality changes in the therapy group, John comments: "All this is quite meaningless unless we consider the size of the difference."

A closer scrutiny of the findings of the project shows that John had good reason for these comments. With respect to better release rates, from hospital, for the therapy group, it is unclear whether this had anything to do with therapy. When initial differences in experimental and control groups were considered, and corrected for, the investigators found (p. 282) that "it seemed probable that the discrepant ego strength scores for therapy and control groups accounted for their different hospitalization rates. While the therapy group continued to show a greater percentage of time out of the hospital, the difference was no longer statistically significant. Thus, when initial ego strength was controlled, the resultant findings became consistent with those for other outcome measures, indicating no differential improvement for the experimental and control groups."

Concluding this section on a more general level, it seems evident to me, from a vantage point of almost 30 years' experience of working in a psychiatric hospital, of which the majority of patients are diagnosed with some kind of schizophrenia, that the results of this research, with respect to the universality of CCT, should be disappointing. It couldn't have been otherwise, it could have been predicted. On pp. 81-82 Rogers writes: "In considering how small were the differences between the two groups [experimental and control], some words of qualification should be borne in mind. The members of the control group were receiving all the best treatment, which a modern hospital could afford - occupational and recreational therapy, group meetings of an essential therapeutic nature, in some instances group therapy. As to the therapy group, for a number of the patients who were particularly resistant to any helping relationship, the question might be raised whether they were actually
in therapy at all." To this, John has the following comment, in the margin: "It also shows that they were extremely naive! To begin such a massive project without some pilot work!!" I feel convinced that "some pilot work" would have led to significant changes in the research design, and thereby also to significant changes in the conclusions of the research project.

**John's questions – my answers**

Let me start this section by repeating John's questions (Shlien, 1992, p. 1084):

The real question is what to do when a vaunted project is failing, when patients are not voluntary, self-initiated clients with free choice, but are unwilling and unresponsive. Should we declare that the therapists are inadequate, that the population is unsuitable, or that the theory is wrong? Is the test too severe? Should the practice be changed in a way that violates the theory? Should the theory be changed to accommodate the practice? Really, it was a watershed for the man, theory, and movement – a point when both means and ends were bent out of shape. With that denied, now could they recover?

1. Should we declare that the therapists are inadequate? Yes, I think so.

Throughout the book there are numerous indices of the therapists' inexperience with schizophrenic patients. I'll give a few examples:

On p. 8 Rogers writes: "For the majority of the therapists this was the first extensive work with hospitalized psychotics…"

Further, on p. 76, Rogers writes, "Our therapists were sometimes baffled by the lack of self-exploration among our schizophrenic clients, since they had come to think of self-exploration as characteristic of most psychotherapy".

As a final example of therapist inexperience, Rogers writes on p. 92: "... our therapists – competent and conscientious as they were – had over-optimistic and in some cases seriously invalid perceptions of the relationships in which they were involved".

Relevant to the issue of therapist inexperience is also, I think, the fact that the therapists were not necessarily particularly experienced with client-centered therapy, nor, necessarily, dedicated to client-centered therapy. Rogers writes, on p. XV, about a therapist who, in a second interview, says: "I won't listen to any more of that crap!" and on p. 12 he writes: "... the therapists in our group found themselves trying out and developing many new and different modes of response behavior. The variety of specific behaviors among the therapists increased sharply".

As I read the book, I got a still clearer impression that the therapists sometimes felt rather utterly helpless in the therapeutic relationships with their clients, and probably responded out of this feeling, and for the sake of their own "cathartic release", more than out of anything else. The following quote (p. 67-68), I think, conveys something of this: "The experience of most of the therapists on the project
had been primarily with outpatient clients who came voluntarily for help. They were faced with many difficult problems in establishing a relationship with the hospitalized schizophrenics and likewise with the normals, both of the groups being composed of individuals who were not seeking help. The problems of the therapist and the solutions to these problems were manifold: sometimes pathetic, sometimes amusing. How is a male therapist to deal with a female research client who dashes into the women's washroom when she sees him coming? ...The therapists came to realize that they were dealing with individuals who were unmotivated, often unreachable, largely without hope, lacking in any concept of therapy, and certainly lacking in any belief that a relationship could be helpful”. One therapist said (p. 68): “...simply a supply of cooperative therapists such as ourselves will not suffice to clean out the back wards of state hospitals. I have been quietly horrified by the monolithic acceptance of the status quo I have seen among the patients here”. After this comment, Rogers continues (p. 68-69): “To be consistently rejected, over and over, to be unable to do anything helpful, to see no progress over long periods of time, to see no sign in the patient that he has any understanding of the relationship, to desire very much to be in touch with this person and to be unable to get through – these added together constituted a devastating experience”.

The above quotations should, I think, suffice to document my conviction that the therapists, more often than not, felt helpless and inadequate, and behaved correspondingly.

According to my experience, the therapists lacked two competencies, which are essential when one wants to relate with a withdrawn schizophrenic person. (I use the term “withdrawn” to differentiate these persons from the many persons, diagnosed with schizophrenia, who are self-expressive and relate actively to others.)

The first competence is acceptance of the fact that this person, apparently, does not want the therapist to understand anything about him/herself. Garry Prouty (1994, p. 55) calls these persons pre-expressive. One does not have the experience that they define themselves in any way with respect to others, and with a therapist in particular, they do not define themselves as persons who wish to be understood by the therapist. They seem to live in their own world; others do not seem to exist for them as significant others. John Shlien (1961, p. 296) writes: “The mind emerges through a process of communication. This involves social interaction on the basis of what Mead calls “significant symbols” (usually words). A significant symbol is one that is “reflexive,” i.e., when it is used it presupposes another person, anticipates his response, involves on the user’s part some sense of how that other will feel. ... Acknowledging the other is essential to the existence of mind, from beginning to end”. It is precisely a non-acknowledgement of the other (the therapist included), which seems, to me, to be the central feature of the experience of being “out of contact” with the withdrawn schizophrenic individuals. This means, of course, also, that there is no “other” whom the withdrawn schizophrenic individual would wish to be understood by. This is, naturally, hard to accept for a client-centred therapist, whose main activity with clients is supposed to be empathic understanding, and who is mostly, from the original client population of client-
centred therapy, familiar with clients who acknowledges the existence of the therapist by wanting to be understood by the therapist.

The second competence, which I think the Wisconsin therapists generally lacked, was competence in empathizing with withdrawn schizophrenic individuals on an appropriately concrete level, which is often the extraordinarily concrete level of the contact reflections of Garry Prouty’s Pre-Therapy (1994, p. 38-40). With such competence, I feel convinced that there would have been no responses like “I won’t listen to any more of that crap”, no “developing many new and different modes of response behavior”, and no “variety of specific behaviors among the therapists increasing sharply”.

Before I became acquainted with the “contact reflections” of Pre-Therapy, I normally did not try to approach withdrawn schizophrenic individuals because I didn’t know how to be with them and feel comfortable and acceptant of them, and follow their own process at the same time. But knowledge of Pre-Therapy has enabled me to respond empathically (verbally and non-verbally) on such a concrete level that I can now be acceptantly together with still more withdrawn individuals, without interference from my own frame of reference. The concrete level of empathizing with these individuals is reminiscent of empathizing with small children. When little Peter claps his hands in jubilation at seeing the train pass by, I think the accepting and empathizing parent will, more likely than not, clap his/her hands, too, with an expression of jubilation on his/her face. That is, the empathy will be extraordinarily concrete since Peter is absorbed with the train passing by; he is oblivious to his parents. There is really nothing he wants them to understand; no inner experiences he wants to disclose to them.

Of course, a therapist as experienced and sensitive as Rogers did, most often, empathize on a suitably concrete level to relate acceptantly to the withdrawn schizophrenic individual. I’ll end this section with an example of this. It is a short excerpt from an interview Rogers conducted with a teenage girl, Elaine, in the Mendota State Hospital (Brodley and Schneider, 2002):

C 16: Well, my heritage for one thing.

T 16: About your heritage.

C 17: I feel incriminated myself.

T 17: You feel incriminated yourself.

C 18: You see (inaudible) my son was in it?

T 18: Your son was in it.

These three therapist responses are examples of the type of very concrete responses Garry Prouty (1994, p. 38-39) calls word-for-word reflections. They are
appropriate when the client’s statements seem incoherent or disconnected, one from another, in order that the therapist does not assume a meaning with the statements, which may not be there.

2. Should we declare the population unsuitable? Yes, I think so.

The criteria for inclusion in the research, and for matching pairs of research subjects to the control group and experimental group, respectively, relied much too heavily on criteria which are relevant to the psychiatric medical model, but in no way relevant to a developmentally oriented psychotherapy like client-centered therapy.

Although the pairs were matched with respect to degree of disturbance, only one among more than 25 criteria for measuring degree of disturbance (p. 551-552) seems, to me, to be immediately relevant for a test of the effectiveness of psychotherapy. That is “awareness of need for help”. Among so many irrelevant criteria, the “awareness of need for help” was not allowed to weigh as heavily as it should have been allowed to, according to my experience. The reason this criterion is so important is, of course, its connectedness with motivation for therapy. The researchers did become aware of this. In a footnote on p. 26, Rogers writes: “With the wisdom of hindsight, we realize that motivation for help should probably have been another variable in our stratification, since this too has been judged to be related to therapeutic outcome. This factor was not, however, included in the design.”

In my experience, motivation is one, of two factors, which makes a difference with respect to outcome. The other is the degree of self-expressiveness of the individual offered therapy. This criterion was not used in the research, either. It relates to the level of abstractness/concreteness, which is appropriate in order to empathize with the other person, and which has already been discussed in the preceding section. The original clients of client-centered therapy wanted their therapists to understand something, and the therapist tried to understand what the client wanted him/her to understand, as the client wanted him/her to understand it. As has already been mentioned, however, the withdrawn schizophrenic person apparently doesn’t want the therapist to understand anything. There is no disclosing of inner experience, and this demands a level of concrete empathy, to which the client-centered therapist inexperienced with this group of individuals is quite unaccustomed.

If the research had focused on therapy with individuals diagnosed with schizophrenia, who were also both motivated for therapy and self-expressive, I am convinced, from my own experience, that the outcome measures for the group of schizophrenics that was offered therapy would have shown statistically significant improvement compared with the control group that was not offered therapy.

The psychiatric, medical model, diagnosis of schizophrenia is irrelevant to the outcome of client-centered therapy, and in that sense the findings of the Wisconsin
Project did support a tenet, which is central to client-centered therapy - the irrelevance of any kind of diagnoses of the potential client. Again, it is not psychiatric diagnoses that are important with respect to outcome of ordinary client-centered therapy. It is degree of motivation and self-expressiveness.

My own experience is that many individuals diagnosed by psychiatrists with one or the other kind of schizophrenia will often be self-expressive and motivated for therapy. This is evident by the implicit or explicit way they actively define themselves as clients and the therapist as therapist. They are, admittedly, often more difficult to follow empathically than the original kind of client, because they can seem incoherent and their perception, and interpretation, of reality can seem very unrealistic, but the therapist can, nevertheless, follow these clients just like he follows other clients.

3. Should we declare that the theory is wrong? No, I don’t think so.

I’ll discuss this point on the basis of Rogers’ theory of the therapeutic conditions in his 1959 theoretical statement:

1. That two persons are in contact.

2. That the first person, whom we shall term the client, is in a state of incongruence, being vulnerable, or anxious.

3. That the second person, whom we shall term the therapist, is congruent in the relationship.

4. That the therapist is experiencing unconditional positive regard towards the client.

5. That the therapist is experiencing an empathic understanding of the client’s internal frame of reference.

6. That the client perceives, at least to a minimal degree, conditions 4 and 5, the unconditional positive regard of the therapist for him, and the empathic understanding of the therapist.” (p. 213)

Although I do not think the theory is wrong, neither with respect to the original client population of client-centred therapy, nor with respect to client-centred therapy with individuals diagnosed with schizophrenia, my experience with this second group does lead me to have some critical comments about Rogers’ statement of the conditions for therapeutic change. My reason is that it can easily be misunderstood in ways that are not conducive to therapy with persons diagnosed with schizophrenia.

First, it must be very clearly understood that the first condition is a pre-condition, or necessary condition of contact, for therapy to occur. It is not a sufficient condition of contact, particularly not when condition 5 demands an empathic understanding of the client’s internal frame of reference. As Rogers
defines being in contact, it is possible to be in contact with even the most withdrawn schizophrenic individual. (Rogers' definition of being "in contact" reads (ibid. p. 207): "Two persons are in psychological contact, or have the minimum essential of a relationship, when each makes a perceived or subeceived difference in the experiential field of the other.").

This, however, does certainly not mean that it is possible to understand anything of this individual's internal frame of reference, as Rogers (ibid. p. 210) defined this concept. "This is all of the realm of experience which is available to the awareness of the individual at a given moment. It includes the full range of sensations, perceptions, meanings, and memories, which are available to consciousness. The internal frame of reference is the subjective world of the individual. Only he knows it fully. It can never be known to another except through empathic inference and then can never be perfectly known".

The reason one can be "in contact" with the withdrawn schizophrenic individual without understanding anything of his/her "inner" frame of reference is that the withdrawn schizophrenic individual typically makes no disclosure to the therapist of his or her inner experience like the sensations, perceptions, meanings, and memories mentioned in Rogers' definition. There is nothing "internal" that this person wants the therapist to understand. The term "internal", I think, does imply something that is not observable. This is very unfortunate with many people suffering from schizophrenia who tend to withdraw (further) at anything but the most concrete empathizing with what is externally and observably there. As I see it, the term "internal", in the fifth condition, is not only superfluous (empathic understanding of the client's frame of reference, however "internal" or "external" is, I think, what this condition is about), it also misleads the therapist into trying to understand more than the client wants the therapist to understand. Another reason I think the term "internal" is superfluous is that if the client's "internal" frame of reference is not in some (maybe very subtle) way "external", the therapist couldn't perceive it, and therefore could not empathize with it.

Second, motivation for therapy must be implied by the first and second conditions. Without a person who defines him/herself as a client, there can, in my opinion, be no therapy. This does not mean, of course, that person-centered interaction cannot take place with people who are not motivated for therapy; for example, with people who have been forcefully committed to a closed ward and who define themselves as prisoners of psychiatry. The motivations, however, for this interaction, is certainly, and self-evidently, not those of offering and receiving therapy.

4. Is the test too severe? Yes, I think it is.

The reasons for my answer to John's fourth question should be evident from the preceding sections. Selecting people diagnosed with schizophrenia, without correcting for the factors of motivation and self-expression, and then testing the
effectiveness of client-centered therapy with such people relative to the hypotheses of this research project is, I think, a test that is so severe that it is doomed to failure.

5. Should the practice be changed in a way that violates the theory? No, I don’t think so.

When I first read the quotation of Rogers, in John’s paper (1992), about the possibility that persons who suffered from schizophrenia would respond better to conditional regard than to unconditional positive regard, I felt shocked and sad. I thought that Rogers must have felt extremely dejected about the findings of the Wisconsin Project when such a thought entered his mind. After all, unconditional positive regard is one of Rogers’ three therapist conditions, and it is thought by many to be the primary curative factor of client-centered therapy (see, for example, Bozarth, 1998, pp. 83-88). In addition, independence of any kind of diagnostics is at the center of the philosophy of client-centered therapy. For example, Rogers writes (1951, pp. 223-224): “... a diagnosis of the psychological dynamics is not only unnecessary but in some ways detrimental or unwise. ... the very process of psychological diagnosis places the locus of evaluation so definitely in the expert that it may increase any dependent tendencies in the client ... it appears to lead to a basic loss of confidence by the person himself, a discouraging realization that “I cannot know myself”. ... When the locus of evaluation is seen as residing in the expert, it would appear that the long-range social implications are in the direction of the social control of the many by the few.”

From my own experience, it is evident that relating with acceptant empathic following to persons diagnosed with schizophrenia facilitates actualization of these persons’ most constructive potentials, just as it is the case with everybody else. It is, however, important to bear three things in mind with these persons: 1. The level of empathic following will ordinarily be very concrete. 2. Efforts at “deep” empathy, “additive” empathy (Mearns & Thorne, 1999, p. 45), or empathy at “the edge of awareness” (ibid. p. 52) will, more likely than not, have negative consequences, because such efforts can easily over-stimulate these persons and/or be experienced as intrusive. It is extremely important to accept the level of experiencing of these persons and to empathize with them on this level, i.e., to resist any temptation to deepen their level of experiencing. (I think this is important with any client as an expression of true following of the client’s own orchestration of his/her process, but that is another story). 3. The more constructive potentials that are actualized, will, for a very long time, be contact potentials or potentials for relationship formation, rather than potentials for self-exploration and deepening of the level of experiencing. With these caveats, client-centered therapy is, in my experience, better suited to meet and be of use to the individual diagnosed with schizophrenia than other kinds of therapy. I have been trained in psychoanalytic therapy and cognitive-behavioral therapy, and I have found these approaches much less useful than the person-centered approach.

6. Should the theory be changed to accommodate the practice? No, I don’t think so.
It should be evident from the preceding sections that my experience over the years with a very large number of persons diagnosed with schizophrenia has convinced me of this much: A therapist who genuinely believes in the potential of acceptant empathic understanding to facilitate actualization of the client's most constructive potentials, and therefore follows his/her client with just that, will effect therapeutic change in the client diagnosed with schizophrenia, as in any other client. The process will probably be more longwinded and feel more difficult for the therapist, but that is another issue, and no reason to change either theory or practice.

With an eye to the inmates of psychiatric hospitals, and the difficulties encountered in the Wisconsin Project, I could have done, though, with a couple of slight modifications in Rogers' list of therapeutic conditions:

I would have liked condition 2 to read: That the first person, who defines him/herself as a client, is in a state of incongruence, being vulnerable, or anxious.

With that formulation of condition 2, I think the researchers in the Wisconsin Project would have corrected the research design for the variable of client motivation.

In addition, I would have liked condition 5 to read: That the therapist is experiencing an empathic understanding of the client's frame of reference.

With a removal of the term "internal" from condition 5, I think empathizing on the very concrete level that is often appropriate with individuals diagnosed with schizophrenia, would have been more easily accessible to the therapists of the Wisconsin Project.

With these changes, I feel pretty much convinced that the results of the Wisconsin Project would have been much more supportive for the assumption that client-centered therapy effects therapeutic change in all kinds of clients, i.e. for the assumption that the theory of client-centered therapy is a universal theory.

7. With that denied, now could they recover? Yes, I think so.
8. What did John refer to, when he wrote: "With that denied"?

One answer can be found in John's 1992 paper (p. 1084), where he comments on Rogers' speculations about offering conditional regard, rather than unconditional positive regard, to persons diagnosed with schizophrenia: "Unconditional regard proved to be something the therapists were unable or unwilling to provide. The definer "unconditional" was cast off, leading to a final hedge" (and now John quotes Rogers): "Though our work has changed in a number of significant ways it is a matter of some interest that we have seen no reason to alter in any basic way our conception of the relationship." I agree with John that maintaining that no reason has been found to alter in any basic way the conception of the relationship, after
having speculated about changing the condition of unconditional positive regard, is denial.

Another answer, I think, can be found in the 1967 book:

In his summary of the results of the research (p. 92), Rogers writes: "It suggests that whether we are dealing with psychotics or normals, delinquents or neurotics, the most essential ingredients for change will be found in the attitudinal qualities of the person-to-person relationship." John comments on this in the margin: "This is a gross overgeneralization. They have by no means studied all aspects of the relationship. Since they produced very little change, they can hardly claim to have discovered the most essential ingredients!"

I agree with John that this, too, is denial. Rogers denies the disappointing results of the Wisconsin Project with respect to the assumed universality of client-centered therapy. I also suspect that this denial has been evident to others, since so very few client-centered therapists have found their way into psychiatric hospitals.

However, from my own personal "Wisconsin Project", of almost 30 years duration, I am convinced that Rogers was, after all, right. With persons who define themselves as clients and with awareness of the extraordinary concrete level of empathizing that is sometimes appropriate, client-centered therapy is, I'm convinced, universal. I hope my arguments for this conviction, as presented in this paper, will convince others, and thereby contribute to the recovery John asked about, and contribute to client-centered therapy finding a much more prominent position in psychiatric hospitals.

Conclusion

A reexamination of the conclusions of the Wisconsin Project in the light of later experiences with Client-Centered Therapy with persons diagnosed with schizophrenia, demonstrates that the degree of successfulness of client-centered therapy depends on degree of motivation and/or self-expressiveness of the client, rather than on the client being diagnosed with schizophrenia or not. Stated otherwise, I think the data demonstrates that the psychiatric diagnosis of schizophrenia is irrelevant to therapy outcome. The relevant factors are client motivation and self-expressiveness. This evidence, therefore, strengthens Rogers’ hypothesis that client-centered therapy is a universal therapy.

The conclusion is important because many persons diagnosed with schizophrenia are well motivated in therapy and self-expressive, and when that is the case it is my experience, from almost 30 years of working in a psychiatric hospital, that they normally benefit from client-centered therapy. It would therefore, I think, be a sad mistake if one, on the basis of the conclusions of the Wisconsin Project, as they were presented in 1967, avoided offering client-centered therapy to people with a diagnosis of schizophrenia, solely on the basis of this diagnosis.
With the present paper I hope to have reduced the risk of this mistake being made.

References


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