

JOURNAL INTERVIEW—57

Conversation with Frederick B. Glaser

In this occasional series we record the views and personal experience of people who have specially contributed to the evolution of ideas in the Journal's field of interest. Frederick Glaser is an American psychiatrist who has advanced and explored influential ideas on treatment-matching and treatment system development.

A liberal education

A: Fred, you've had a lengthy career in the field of alcohol and drug problems. One thing that has struck me is your ability to consider layers upon layers of thought. You seem to be interested in making things clear that are often incredibly contradictory. I would like to understand that.

FG: I think that started early. I was one of a group of 30 or 40 Ford Foundation scholars who attended the University of Wisconsin; I was 15 years old when I entered college. I enrolled in the Integrated Liberal Studies program on the advice of my advisor, Dr Herbert Howe, and I think that ILS was the single most important formative influence on my development. We learned about things that I never would have studied on my own, because I did not know what they were. The principle example was the course in Classics. We had a solid year of Classics with two of the most remarkable teachers, Professor Walter Agard and Professor Paul McKendrick. We also had courses in anthropology, literature, earth science and many other subjects. We had courses in philosophy during each of the four semesters of the program. I think ILS provided a humanistic perspective that was invaluable and has influenced me all my life.

A: The original concept for the Wisconsin Experimental College that preceded the Integrated Liberal Studies program was that there would be a deep study of the classics and also a connection between classic thought and contemporary society. Was that still the philosophy?

FG: Absolutely, and not just in ILS. For example, Professor McKendrick taught a course in the classical origins of contemporary literature. We



Figure 1. Frederick B. Glaser. © ASAP.

met twice a week. In the first session, he would lecture about the classical source, and in the second session about a related contemporary example. For example, the first lecture of the week might be on Homer's *Odyssey* and the second on Joyce's *Ulysses*. The course well illustrated the relevance of earlier thought to contemporary life. It was the greatest imaginable treat. One reason I went into psychiatry was a wish to

be involved in something in which multiple aspects of knowledge were relevant.

A: Then to medical school.

FG: After Wisconsin, I went to medical school. It had been fully ordained that I would do so; my father was a physician, and I never seriously considered anything else; but when I got my medical degree I had no idea what I wanted to do. I had wanted to be a psychiatrist since I was about 11 and I picked Karl Menninger's *The Human Mind* out of my aunt's library and read it.

' "Chentlemen!" she said. "Dere is nutting to life but sex und aggression, und the sooner you recognize that, the better" ... Such was my introduction to psychiatry'

A: When you were 11?

FG: When I was 11. I thought, "Gee, this is fascinating—this is what I want to do". But when I got to medical school one of the first psychiatry lectures we had was by a well-known psychoanalyst. She was very striking; she had silver hair which was pulled back very severely into a bun, she had a marked Viennese accent, and she smoked like a chimney.

A: Even during the lectures?

FG: Yes, she strode onto the stage cigarette in hand. 'Chentlemen!' she said. 'Dere is nutting to life but sex und aggression, und the sooner you recognize that, the better.' Everybody looked at each other and said, "What on earth is this?" Such was my introduction to psychiatry.

A: Did this turn you off to psychiatry?

FG: Yes, it made me think I had been mistaken. As time wore on, however, we had very good experiences in psychiatry at the Massachusetts Mental Health Center and elsewhere. By the time I graduated, I was thinking about it again. But I continued to worry about psychiatry as a career because I had had some unsettling experiences with the children of psychiatrists, and I

wondered about potential adverse effects on my future family. Fortunately I met a young staff psychiatrist at Massachusetts Mental Health who invited me to his home and I met his children. They were normal and I thought well, maybe I do not have to worry about that.

A: Did you think it had something more to do with the quality of the parents?

FG: I think so. Some of them did not relinquish their professional role when dealing with family members.

Psychiatric training

A: What next?

FG: After medical school I took a 2-year rotating internship, and had a significant amount of time in five different fields (surgery, obstetrics-gynecology, pediatrics, medicine and psychiatry). It gave me a chance to decide what I wanted to do based on actual experience. I received my greetings from the President of the United States when I was an intern. I did not want to go into the army. I knew that the life of a general medical officer in the army was not terrific. They could send you anywhere and ask you to do anything, and I would be far better off going in after I completed my specialist training. But the second thing was that I did not want to go to Vietnam. Had I not been successful in joining the Public Health Service, I think I would have gone to Canada earlier than I eventually did, because I was determined I was not going to participate in that war.

A: How did you end up in the Public Health Service?

FG: I learned that the Public Health Service offered a residency deferment program. The PHS was also not overtly militaristic. There would not be things such as drill and how to use weapons. We had to wear uniforms, but we did not have to wear ties. And, we were not armed.

A: So you applied to the US Public Health at the point that the government came calling and then the service was deferred to after that?

FG: Exactly right. I then had my internship rotation in psychiatry, and took my 4-month elective in psychiatry as well, so I had 8 months of psychiatry and I really liked it. I decided that was what I wanted to do.

A: *What did you do in psychiatry?*

FG: Oh, it was fascinating; we had a variety of experiences. Most of my elective rotation was in the emergency department. We had a separate suite that consisted of a very large seclusion room and a couple of offices. We literally got to see everything. We frequently got police cases, acutely psychotic people and all other kinds of things.

A: *Was there a supervisor right there in the beginning or did they just throw you in?*

FG: There was a chief resident who was responsible for emergency psychiatry, but it was understood that we would not call him unless we really were in trouble. Occasionally, you could not get hold of him. It sort of aggravated me, and when that happened, I would call the professor and chairman of the department, Dr John Romano, 'The Professor' to all of us. He was one of the most remarkable people I ever met in my life. He was always extremely helpful. He never got angry at me for calling. His knowledge was truly amazing. I remember my colleague Howard Axelrod summing it up admirably: 'The Professor is BIG'.

A: *What made him so big?*

FG: He was a real gentlemen. He was kind to everybody. He was vitally interested in the residents. We met with him every week as a group. He did rounds on every one of the floors. He had a marvelous perspective on things. Psychoanalysis was a good example. At that time, psychoanalysis was the dominant body of thought in psychiatry.

A: *This was in the early 1960s?*

FG: Yes, I was a resident from 1961 to 1964. Everyone on the faculty was an analyst except for Dr Romano. Dr Romano said that he had been analyzed, but was not an analyst. He had a rule that residents were not to enter analytical training. He felt very strongly that they should be fully trained first. Then if they wanted to do it, fine, but not while they were residents, because he felt that we were too impressionable.

A: *Was his conceptualization primarily psychoanalytic?*

FG: No, I would say he was much more eclectic in his approach. It was at a time when medication was just coming in. The first two medica-

tions we ever used were reserpine and chlorpromazine, followed by chlordiazepoxide and imipramine, but we had nothing like the huge pharmacopoeia available today. We also made fairly significant use of ECT. The Professor was interested in all forms of psychotherapy, but he was not hostile to psychoanalysis. We had very distinguished psychoanalytic teachers. The principal analyst was a wonderful gentleman by the name of Sandor Feldman. Dr Feldman had personally attended Freud's 'New Introductory Lectures on Psychoanalysis' while a young medical officer in the Austro-Hungarian army in World War I: a remarkable link to past history. He offered a seminar based on Freud's *Interpretation of Dreams*. Regrettably, only two of us attended it regularly in my year. We learned something about the interpretation of dreams, but we learned a lot more about Dr Feldman's life and his experiences and what Budapest was like when he was growing up.

A: *He was also teaching you about the human mind?*

FG: Yes. He had written a book called *The Psychopathology of Everyday Life*, which was quite wonderful. Dr Feldman was also a very devout Jew and taking his class was a little bit like studying Talmud. He would read a passage from *The Interpretation of Dreams*, and then he would associate to it, telling us about his experiences (usually in Budapest) and the patients he had seen and some of the dreams he had interpreted. At about that time Erik Erickson published a paper suggesting that one of the major sources of psychoanalytical thought was the study of the Talmud. Many of us in the residency program were Jewish, but we had never studied the Talmud. So we engaged the services of a local conservative rabbi and studied the Talmud for a year. We concluded that Erickson was exactly right. By the time I finished my residency, my ambition was to become a psychoanalyst.

A: *Even though you hadn't been analyzed yet?*

FG: Yes; I had followed Dr Romano's advice, although not all of my colleagues did. I knew that the Public Health Service had an outpatient clinic in Washington, DC, and that if I were assigned there I could enter the Washington-Baltimore School of Analysis and begin my analytical training while I was discharging my service obligation. But I then learned that the plan of the Public Health Service was not to send me to

Washington. They were going to send me to Lexington.

The stereotype of addicts as ‘horrible people’

A: Kentucky.

FG: Kentucky. And I thought, what have I gotten into? Who ever heard of Lexington, Kentucky? Plus, my big concern was I would ever after be typecast as a person who dealt with these horrible people—these addicts—and I did not want that.

A: What were your experiences with alcoholics and addicts as a resident or intern?

FG: Very few. We had admitted a number of women who had been prescribed sleeping pills and become dependent on barbiturates. It was also the era of the ‘fat doctors’, and we admitted a number of women who had become psychotic from using too many amphetamines. Everybody thought that if we put them in the hospital for 30 days they would revert to normal, but that was not what happened. Most of them had to be committed for long-term care because they continued to be psychotic. There was one case that was a real foretaste of Lexington, a man who had become dependent on meprobamate. When he came into our unit he was taking enormous quantities of meprobamate, as much as 14 g a day.

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A: 14 g?

FG: Just huge; the standard dose was 1.6 g per day. I had no idea what to do. My attending said, ‘Not to worry. We’ll do what they do at Lexington.’ I said, ‘Oh—thank heavens—somebody knows something about this. What do they do at Lexington?’ He said, ‘Cold turkey; we cold turkey him’. The poor man was completely psychotic for about 2 weeks.

A: Did you think that was the right thing to do, to have him go cold turkey?

FG: No, but I did not know, and my attending was completely confident that this was the appropriate approach. When I got to Lexington, I found out that was not what they did at all. They withdrew people very gradually. For narcotics, they always used methadone. For barbiturates or any drugs that were cross-tolerant to them, including alcohol, they gave a pentobarbital challenge test. They would administer 200 mg of pentobarbital orally. If the patient became delirious, had a drop in blood pressure when they stood up suddenly, and so on, they did not have to withdraw them because they had to have been on small doses. If they were not at all affected by the 200 mg dose, they did have to be gradually withdrawn. A loading dose would be calculated, administered and then gradually reduced.

During my residency training we almost never saw people with alcohol problems in the department proper. They were sent to a special clinic which was run by one of the attendings who himself had had alcohol problems, and that was it; residents were not assigned to that clinic. We never saw narcotic addicts either, and I developed the opinion there must not be any in the city. I found out at Lexington that I was wrong, because there were many patients there from Rochester. They were there all the time; we just did not see them.

A: How did you decide these alcohol and drug addicts were terrible people and that you didn’t want to get branded as being the person who cared for them?

FG: Well, I think it was sheer ignorance. Ignorance and fear.

A: Was there absence of contact or were there actively hostile and negative attitudes?

FG: Both. As I said, we did not see addicts or alcoholics during residency training. And there was a body of professional opinion in support of such an exclusion. For example, a very influential psychiatrist had written several articles in which he said that psychiatrists should not deal with people who took drugs or alcohol because it obscured the purity of the unconscious process. That influenced me. And, of course, there was my own experience. I had worked on surgery and medicine, as well as in emergency psychiatry, and we saw a lot of people

with alcohol problems. Many were drunk, sick and difficult, and it was really not a good experience. I did not want to have anything to do with them.

Lexington—remarkable place, intense experience

A: Tell me about the transition. Here you were in Rochester where you had not seen many addicts, so you thought there were not many of them. And you thought that they were people that psychiatry should have nothing to do with. And that to treat them you should deprive them or restrain them and let them tough it out. So this is the model you were trained in. And then you get to Lexington, and you became a staff physician.

FG: Lexington was a remarkable place. The total census was about 1100; it was a big, big place.

A: It was a hospital facility? Or a prison facility? Or some of each?

FG: Both. There was a saying at that time that Lexington was more like a hospital than most prisons and more like a prison than most hospitals. It was a Federal prison.

A: So everyone there was in jail?

FG: Everyone there was in a jail, but some of them were there voluntarily. At that time, 1964 to 1966, Lexington and Fort Worth were the only two hospitals in the United States that would treat narcotic addicts. The majority of patients at Lexington were from New York City, 1000 miles away. They were also from every other big city. Lexington took men from east of the Mississippi river and all women from the country as a whole. Fort Worth took men from west of the Mississippi; it had no women's unit. Lexington accordingly had an incredibly polyglot population, and we were thrown right into it.

'There was a saying at that time that Lexington was more like a hospital than most prisons and more like a prison than most hospitals'

A: What was your first reaction? Not happy?

*FG: No, I was filled with trepidation. I had tried to find articles in the literature about the treatment of narcotic addiction, and there was very little. I remember reading a section in the *American Journal of Medicine* that had been written by the staff at Lexington, but I started my tour with essentially no knowledge of narcotic addiction, and I was really very frightened. But I quickly found that Lexington had an excellent staff. Indeed, not only the professional staff, but the custodial staff were superb.*

A: When you say custodial, you do not mean people who are cleaning the floors?

FG: No. These were security personnel, prison guards basically. Lexington was, in part, a prison. It looked like a prison. It had grills everywhere, floor to ceiling iron gates. About three-quarters of all patients were there as part of their prison sentence, almost always for violation of narcotics laws, but there were also volunteers. They could come and go as they pleased. Most of the admissions were volunteers, but most of the patient population were prisoners, since the volunteers often did not stay long.

A: What was the treatment? Beyond the detox, what was the actual treatment?

FG: There was a fairly well structured course of treatment. It began with a very careful assessment. Patients were then staffed and a decision was made about treatment. The principal modality on offer, given the numbers, was group therapy. Staff were allowed to take a small number of patients into individual therapy, but group therapy was much more widely used. It was also felt that being confined was therapeutic and I think that was true. Lexington had very active medical, dental and surgical services. I learned later that many volunteers came to Lexington purposely for these services because they could not get them elsewhere. There was also an element of milieu therapy. It was not very well conceptualized, but all of us found ourselves working jointly with the security personnel to manage the patients. It was a particular pleasure to work with Mrs Redman, the wise and kindly security chief of the Women's Unit. There was a lot of debate about whether the treatment offered at Lexington was effective. A medical school classmate of mine, George Vaillant, was a member of the staff

during the first year that I was there. His research suggested that if patients stayed at Lexington for a significant period of time, I believe 9 months or longer, and if they had parole supervision in the community after leaving, they often did well.

A: Give me a feeling for what went on.

FG: Let me tell you a medical story about Lexington. One of our principal duties was to be on sick call at night. When new medical staff came in, the lines for sick call went around the block. Everybody wanted to see what these new guys were made of. Their complaints were remarkably consistent. The men all said they had headaches, the women all said they had menstrual cramps, and everyone said they were allergic to aspirin. The hospital had provided elaborately for just this contingency. It had three unique medications, called Rubrasa, Flavasa and Vertasa. They were (respectively) red, brown and green aspirin. I fell into using them initially, but then I began to have second thoughts. Their complaints were of conditions that could not be verified objectively. In addition, perhaps because my father was an allergist, I began to worry that one of them might actually *be* allergic to aspirin. And I wondered whether using medications to which the patients attributed magical properties did not play into their basic problem with drugs. So I started saying to them, ‘Look, here’s the story. You are here because you have a problem with narcotics. One of the best things about this place is that it is relatively free of narcotics. I do not think a lot of the people I see here are really having pain, but are looking for a drug. I think it would not be in your best interest for me to give you narcotics just because you say you are allergic to aspirin. If you are having pain and I should have given you medication, you have my apologies. I am sorry, but the pain will pass and it is very important for you to learn that not every pain requires medication.’

A: How many months had you been there?

FG: A couple of months.

A: Oh, so it did not take you too long?

FG: It did not take me too long because I really was worried about the allergies. After I started this I noticed that, when I was on duty, few

patients came to sick call. I thought, well, they probably all hate me. But one day a man to whom I had refused medication, provoking an angry outburst, tapped me gently on the shoulder and said, ‘Atta boy, Doc.’ From that time on, I had no qualms about what I was doing, because at least some of the patients recognized that I was really trying to act in their best interest. I found the patients absolutely fascinating.

A: Because?

FG: Because they were often highly self-aware, as the story shows. They were difficult. They were in many ways more like real or ordinary human beings than the patients that I had been used to. I had been used to chronically psychotic patients, who were enormously tractable. One of my first impressions of Lexington was that, although I had thought it was difficult dealing with the chronically psychotic patient, it was a piece of cake compared to these people. What became rapidly apparent was that to be even a modestly successful narcotic addict, you had to be very clever and very smart and you had to have your wits about you at all times. If you were seriously psychiatrically impaired, you very likely could not hack it. These patients had led eventful and often dramatic lives. They were a talented group of people. I well remember a young black woman, 16 years old, from New Orleans. She was one of the most difficult patients I ever had. She was not notably intelligent. She was truculent. One day I asked her, ‘What do you like to do?’ ‘Oh’, she said, ‘I like to sing. I’m a great singer’. I was very skeptical. There were several bands that played every day in the capacious central courtyard. There was a Latin band, a small jazz band, a large jazz band—all kinds of bands. We had a function on the women’s unit and this woman got up to sing. I had never heard anything like her! It was just the most powerful... moving experience. I had listened to some good blues singers, but ...

‘to be even a modestly successful narcotic addict, you had to be very clever and very smart and you had to have your wits about you at all times’

A: *You seem very moved by this. It seems very deep in terms of how you reacted to these people.*

FG: What became clear to me is that these were good people. Oh yes, there were exceptions, but for the most part, these were good people who had enormously difficult lives, partly on their own initiative, partly not. They were despised and scorned and unable to get treatment, and they had to come a thousand miles to get any help at all, and they were very likable and they were intelligent and they were fun. If they could only turn their intelligence and their charm and their ability to good effect, what a wonderful thing it would be.

A: *How did the Lexington experience influence your career plans?*

FG: The other important thing that happened to me at Lexington was on the professional side. It turned out to be possible to get analytical training while you were at there, but very difficult. I considered it but put it off. Then an article came out in the *American Journal of Psychiatry* by an analyst, Van Buren Hammett. He said (about being trained as an analyst) 'You probably think that the real roadblock is the money, but that isn't the real problem. The problem is time. If you go into analytic training, you will not have time for anything else. You have to devote yourself completely to being an analyst and that is all you can do.' And it was not a negative article.

A: *It was reality.*

FG: It was reality, and it was the first time I became aware of this aspect of analytical training. I had begun to read the literature on narcotic addiction. Lexington had a remarkable library, and was the home of the Addiction Research Center, which has evolved into the intramural program of the National Institute on Drug Abuse. There were excellent researchers at Lexington who had made seminal contributions to the field, such as Jack O'Donnell, John Ball, Bill Martin, Abraham Wickler and Harris Isbell, and I had begun to learn from them and from the library. After I had been there for a while I began to think, this is such an interesting field, I do not want to get out of it. I do not want to give it up to become an analyst.

A: *Other influences from that time?*

FG: There were additional turning points, sometimes stemming from the visitors who came to

Lexington. At my very first staff meeting Warren Jurgenson, the Deputy Medical Officer in Charge, said, 'We have a special visitor today'. We watched as a large chauffeured limousine came driving up. A very elegant woman with gloves, hat and a graceful manner emerged. This was our speaker for the day. Her message was: 'I am now very wealthy. I am the president of my own electronics firm. But I was a patient here at this institution, not once, not twice, but eight times. And what I want to tell you is, don't ever give up on anyone. Because it takes time.' Another visitor was David Deitch. David had been a heroin addict for 15 years, had gone through Synanon, and was then the Executive Director of Daytop Village in New York. This was my introduction to therapeutic communities. I had heard the term before in connection with Maxwell Jones, but what David was talking about was completely different.

Philadelphia years and a growing interest in treatment systems

A: *You moved to Philadelphia in 1966, and went back into general psychiatry then?*

FG: Yes. One of my teachers in residency, Al Gardner, was made head of the Community Mental Health Center at Temple. He recruited a number of people who had gone through the residency program. The idea of getting back together and starting this new adventure was compelling. I was put in charge of the partial hospitalization program, about which I knew nothing.

A: *Another experience with doing things you knew nothing about.*

FG: Exactly, but why not? I continued to be interested in alcohol and drug problems. I began going up to New York to visit Daytop Village to see how it worked. I was very impressed with it and even set up the partial hospitalization program as a therapeutic community. Then there was a major political schism at Daytop. A lot of the staff and patients left, and we invited a group of them to come to Philadelphia. That was the beginning of Gaudenzia (Glaser, 1971), the therapeutic community.

A: *Where did the name come from?*

FG: In Sienna, Italy, there was a famous horse race continuing from medieval times called the

Palio della Contrede. One of its legends concerns a horse, Gaudenzia, who threw its rider, but who nevertheless finished first and was declared the winner. The idea was that the members of the community would succeed in spite of major difficulties. It was an apt name.

A: *What was your involvement?*

FG: I was a member of the board and Chair of the Research Committee. We performed some research, directed principally at trying to figure out what the retention rate was and why some people stayed in and others did not. It turned out that the split rate was enormous, something like 95% in the first month. We developed data that suggested it was related to the season of the year. There were more splits in the summer than there were in the winter, and I thought that maybe it had something to do with temperature. Foolish me. It turned out that the drug market was far more active in the summer. So that was no doubt the reason there were more splits in the summer. I came to like and respect the people I worked with and began to feel that the therapeutic community was a very significant innovation. I have been connected with it ever since, in the United States and in Canada.

A: *It seems to me that somewhere in these Philadelphia years your interest began to shift from the individual patient to systems of care. That has been a big focus for you since then.*

FG: A psychologist who had been on the faculty at Temple, Richard Horman, was appointed Director of the Governor's Office of Alcohol and Drug Problems. Rich asked me to be the principle treatment consultant to that office. I was able to bring in John Ball, who was an experienced epidemiologist. When we met with Rich, he said, 'I don't have any clear idea of what we're doing in treatment in this state. Help me to describe the treatment effort.' John came up with the simple but brilliant idea that the best way to do this would be to visit all the treatment programs to see what they were doing. We developed a uniform interview schedule, and visited all of the eighty or so programs. What became apparent was that there was no actual treatment system in terms of a planned, coherent effort. Treatment had just evolved, without any constraints or guidelines. Although many of the people involved in treatment were excellent and did very good work, there was no overarching

rhyme nor reason to what they were doing collectively. For example, we found that there would be more than one program in the same building, and they would not know of each other's existence.

'Although many of the people involved in treatment were excellent and did very good work, there was no overarching rhyme nor reason to what they were doing collectively'

A: *Literally?*

FG: Yes, absolutely, and when we would inform them, one of the programs would say 'Well, it doesn't matter.' We looked at whether there was any relationship between the prevalence of alcohol problems in a given county and the prevalence of treatment programs (Glaser & Greenberg, 1975). There was none. Most of the treatment programs were in or near large cities. There were areas where there were no large cities, in which there was an enormous problem and no treatment. It became apparent that there was no treatment system as such, and that evoked the notion that perhaps there should be. Perhaps there should be rational way of allocating treatment resources and of tying all of this together, and not simply leaving the provision of treatment to chance. The other observation was that there was no cross-referral. There were enormous animosities between certain types of programs, for example between methadone maintenance and drug-free programs. There were disparate philosophies of treatment and there was no idea that there are many paths to a good result, that the trick might be finding the right path for a particular individual. There was no mechanism whereby that could be done, other than trial and error. We began to think about developing a rational system of care that would try to deliver the most appropriate treatment to each individual. We surveyed the drug programs and then the alcohol programs and we found exactly the same situation in both instances. It was these experiences that led to the development of some things I did in the future.

The core-shell concept is created

A: *Was the core-shell concept part of what came out of that experience?*

FG: Yes (Glaser, 1974). In the very first book that was published, on the treatment of alcohol problems, the core-shell system was proposed, and we proposed it again in the second book on drug treatment programs. I think what also helped in my further thinking was that I spent a year as a Robert Wood Johnson Foundation Fellow in Washington, where I had a chance to look at the health system generally, as well as the system of treatment for people with alcohol and drug problems.

A: *What was your vision of the system?*

FG: We were in the midst of the first survey. For some reason, I was thinking about the physicist, Neils Bohr, and his conception of the atom. All of a sudden it occurred to me that maybe treatment should be modeled after that. There might be certain nuclear or basic therapeutic functions that everybody needed and then, in terms of formal treatment, people probably needed different things. So I began to think of a core of universally applicable functions and, surrounding that, a shell of discrete treatment entities, which would be very different from one another, and which not everybody would use. No treatment would be universally applicable. In order to decide which of these treatments would be most appropriate for a given individual, they would first have to be carefully assessed. Thus, assessment was a core function.

A: *Did you have a sense that you were proposing something that could happen, or something you saw as an ideal?*

FG: Both, really. George Orwell had proposed the concept of doublethink, meaning that one could hold two things in mind that were contradictory and believe both of them. On the one hand, I believed that this was the way to go, and that given the appropriate leadership, it could be accomplished. On the other hand, I knew that it would be very difficult thing, and indeed it has turned out to be extraordinarily difficult, and has not been accomplished. I believe that there is movement in that direction, but for very different reasons than I thought would ever be the case, largely the result of managed care. In a sense we are doing the right thing for the wrong reason, moving toward a systems approach not

to provide better service but to save money. In the beginning I really had no conception of just how difficult it was going to be. It seems so logical and reasonable. Initially, I had the hope that we would do it in Pennsylvania. But it turned out to be politically infeasible and could not be done.

A: *What were your thoughts about the kinds of things that would have to occur before it could have happened?*

FG: I felt that if somebody in a position of sufficient authority said, 'This is what we're going to do', it could be done. The problem was finding a venue in which that could happen. That *did* happen in Canada at the Addiction Research Foundation (ARF). When I went there, however, I thought that all of this stuff about treatment systems was behind me. I was to be Head of Psychiatry. After I had been there for a few months, the Director of the Clinical Institute, Dr James Rankin, said in a staff meeting that he had some concerns about the overall thrust of our treatment effort and asked if anybody had any ideas about how we might improve it. I said, 'Well, as a matter of fact ...' and so he asked me to put some ideas on paper. I did just that, and the papers were well received. Jim gave me the go-ahead to put together a team and we implemented the core-shell system for a period of several years.

Toronto—radical treatment ideas and resistant clinicians

A: *What was it, specifically?*

FG: It was a little different from what I had envisioned in Philadelphia (Glaser, 1988, 1994, 1995). I had come to appreciate the concept of case management during my time in Washington. So one of the elements of the core became case management. We called it 'primary care' to differentiate it from treatment, because we felt it was more fundamental than treatment. Many of our patients were in survival mode. They lacked such fundamental things as food, clothing, shelter and gainful employment. They were often sick, both mentally and physically, and it was clear that before engaging in any sort of definitive treatment, these more pressing needs had to be met. I knew that treatment professionals were not well-equipped to do that, so we

conceptualized having a cadre of primary care workers who would excel at this. We set up the system so that the initial contact was with the primary care worker, who had overall responsibility for the case. We also decided to make any treatment contingent upon an assessment. No assessment, no definitive treatment. If people did not want to be assessed, that was their decision. They could continue with their primary care worker forever, but they would not have access to definitive treatment. The third element that we put into the core was follow-up. We felt there was an ethical obligation for us to know the outcome of every case we had treated, because if the treatment had not been effective for that particular individual, it seemed our responsibility to offer something else.

A: This was done in a sufficient amount of time that a team could triage a person into another treatment?

FG: Yes. We also felt that if we were going to assign people to treatments carefully, the treatments had to be clearly specified. We had to have written descriptions of all aspects of the treatment process. There had to be a delimited course of treatment, specified in terms of the number of sessions and the overall duration; a given treatment could not go on indefinitely. We felt that if after X amount of time there was no discernible effect, it was probably not the right treatment for this individual and we need re-assessment and reassignment. Of all the things that we tried to do, getting therapists to specify treatment was by far the most difficult.

A: You were using existing resources in the community for the treatments?

FG: We largely used internal resources in the Foundation. We were able to identify some 21 discrete treatments within the Foundation that we could offer. In order to be eligible for any particular treatment, a person had to meet the specific selection criteria for that program: scoring such and such on a measure, subscribing to treatment goals which were consonant with what was done, and so forth. We also asked the patients what sort of treatment they thought was appropriate, and found that three-quarters of all patients said they wanted psychotherapy. But on further inquiry, virtually none of them had any idea of what psychotherapy was. Prospective pa-

tients know less about alternative treatment methods than we usually think.

A: You said something which struck me as being reasonable but also probably being very radical—that if someone did not do well in one of the treatments it was incumbent upon the treatment system to think about what else the person might need. Whereas the traditional view of treatment is the person was unmotivated, not ready, resistant, etc. Did you run into resistance to that concept?

FG: Oh, absolutely. In fact, we ran into resistance to virtually everything we wanted to do. But especially to that concept, because it quickly became apparent that most of our therapists believed that their treatment method alone was effective, and that if a person did not get a good result from them, it was not the therapy, it was the individual. We felt that was unreasonable.

A: It seemed as though your experience in looking at so many different treatments and programs, and hearing so many clinicians with that passionate belief that their treatment was right, gave you the opportunity to have a perspective that was different than a person only working in one place.

FG: The experience of going around and talking to different treatment programs was an utter revelation. We began to think that enthusiasm was an essential ingredient for therapeutic success. It followed that therapists would jump at the chance to deal exclusively with a carefully selected group of people who were very likely to benefit from what they did. That turned out not to be the case, because they ‘knew’ that whatever it was they did, it would benefit everybody. So yes, it was an eye-opening experience. We also learned that most therapists had no idea whom they were treating, in terms of their demographics as well as other characteristics.

‘most of our therapists believed that their treatment method alone was effective, and that if a person did not get a good result from them, it was not the therapy, it was the individual’

A: *This was within ARF?*

FG: Yes, in ARF as well as elsewhere. Another basic notion of the core-shell was that it would be self-correcting. If we treated people and we looked at results, we would know whether they needed further treatment. If a treatment program was highly successful, that would be some evidence that the assignment criteria we used were accurate, but if success would be mixed or limited, then we would have to retool the criteria. We could learn whether there were segments in the larger population that were not coming to treatment. We could also find out what proportion of the treatment population was using program X, Y or Z. If the proportion was very small, then perhaps that was not a program that we wanted to keep. Rather, we might want to develop a new program that would be particularly appealing to a segment of the population that we were not attracting, or to a segment we were not doing well with despite our armamentarium of treatment alternatives.

A: *Over time, did the clinicians become more positive, or was it more like pulling teeth throughout?*

FG: It really was the latter. And it was not just the clinicians; the researchers objected, too. What the clinicians largely objected to was they did not get to pick the people with whom they did their therapy. But the clinician's input was crucial in constructing the selection criteria. So we would say to them, 'No, you really are picking the people, because you told us the characteristics of those you thought would do well, and we used the information when we developed the assignment criteria.' But they could not see it. They wanted to pick people directly. They did not want to delegate control of that activity. Subsequent experience suggested that they wanted to use unspecified criteria of their own in selecting individuals to treat.

A: *And the objection from researchers?*

FG: Researchers had a different objection. We looked at the treatment system as a way of facilitating research, since every patient could be screened routinely for every ongoing research project. Previously all the recruiting had been done by each individual project. We showed that recruitment proceeded more effectively and efficiently if it was done centrally. It turned out researchers did not like that. Why? They felt that

since one research project was such an enormous part of their career, they had to have direct control over the personnel doing the assessment. Well, assessment workers worked for the system *per se*, not for the researchers, and they did not have that control. They did not like that. Again, an issue of control. It turned out that they, too, wanted to use unspecified criteria, and would regularly violate their stated criteria to enroll larger numbers in their projects. The short and long of it was that although those of us who participated in it really thought highly of the treatment system, neither the researchers nor the clinicians participated enthusiastically, and it eventually went by the wayside.

Matching still matters

A: *Part of what you are talking about is patient-treatment matching, in an empirical, rational way, and it makes sense. Let us pause for a moment to talk about matching. We now have this big clinical trial in which, in a sense, matching did not work. Do you still believe in the concept of the patient-treatment matching process?*

FG: Yes. I very much do (Glaser 1977, 1980, 1993, Glaser & Skinner, 1981).

A: *Why?*

FG: The people involved in Project MATCH did a tremendous job. But, as one swallow does not make a summer, one study alone does not prove or disprove anything. I think there were some fundamental design flaws in Project MATCH. I was concerned that its results would be a signal to the Federal funding apparatus to avoid this area completely in the future. That has not been the case. If anything, my impression is that the feeling about matching is much stronger now than it was, and data are increasingly coming out that suggest there is something to it. I think it will ultimately become clear that matching is extremely important. Is it the only thing? No. I think there are a lot of factors that make for successful treatment, but I continue to think matching of patients to treatment is one of them. Many interesting things were learned from Project MATCH. There were some matches that seemed to hold up and some of them took a lot of time to become evident. Not all of its results were negative.

'But, as one swallow does not make a summer, one study alone does not prove or disprove anything'

A: You talked about matching prospectively, where you are saying, 'Here is this person with this set of characteristics and problems. How do those constellations—what are the most important sets of factors in terms of deciding on treatment?' As opposed to the Project MATCH model, which was retrospective matching, because they did not change the treatment based on what the patients received.

FG: No. They randomized the patients and carried out a mathematical analysis of what variables predicted outcome. Handling the data in this way treats each individual variable as if it existed in isolation. My guess is that the variables we use in matching are closely coupled to other variables. Each variable may well be a proxy for a host of related variables that are extremely important in determining outcome. Hence the effects of matching are more likely to become apparent if you actually match people to treatment programs, which did not happen in Project MATCH. A mathematical analysis simply will not cut it. You will not find the matches because the variables we use are only the tip of the iceberg. Human beings are enormously complicated, and the number of variables that are salient is probably almost infinite. Fortunately, we now have computers that can take a large proportion of those variables into account instantaneously. To me, the potential use of this technology to predict treatment outcome is a truly exciting prospect.

Broadening the base

A: Let us go forward a little bit to Broadening the Base of Treatment for Alcohol Problems. I think people know the book, but the process to get to the book—how did it evolve? It seemed that it was enormously complex.

FG: It was quite complex, but a wonderful experience. I had been a Robert Wood Johnson Foundation Fellow, which was a program administered by the Institute of Medicine. I had performed some subsequent work for the Insti-

tute and had become acquainted with Fred Solomon, a member of the staff. Congress had become concerned about what was going on in the treatment of alcohol problems in the United States, and as part of an omnibus bill, commissioned the Institute of Medicine to do a study of the treatment of alcohol problems in the United States. The National Institute of Alcohol Abuse and Alcoholism was instructed to fund this as part of their budget. When the Institute began looking for a study director, Fred Solomon thought of me. It seemed rather daunting, but I thought about it and decided, why not? The Institute's standard procedure for a study was to appoint a member of the Institute as the chairman of the steering committee for the study. In this instance they appointed Dr Robert Sparks. Bob was by far the best committee chair that I have ever worked with. He and I began recruiting other staff and committee members. I think we put together a committee that was superb. We had the good luck to hire Herman Dieneshaus as our Associate Study Director. Herman had a remarkable knowledge of the ephemeral literature of the field, materials that had not been formally published or widely circulated. This kind of material turned out to be crucial in developing the study.

A: How did the project actually work?

FG: The committee got together with no really preconceived idea; there were few precedents. We had a charge, indeed more than one charge, but the tradition of the Institute of Medicine has always been that the committees are completely independent. It was ultimately up to us to decide what we were going to do, and we did. The study process was a remarkably collegial interaction between the members of the committee, the staff and a whole host of consultants. We were able to set up task forces which drew on people who were not members of the steering committee and it all seemed to come together very well. There were of course differences of opinion, but Dr Sparks's leadership allowed us to deal with them creatively. Herman and I did all the drafting of the study, and it just evolved. When it came out, the report (Institute of Medicine, 1990) was criticized by a number of people as simply being 'the core-shell treatment system writ large'. Not so; many of the ideas from the core-shell were there, but they were only there because the entire committee endorsed them,

and many of the ideas went well beyond the core-shell. We did not do anything that was not unanimously agreed upon by the committee, and this was not necessarily easy. For example, I can remember Herman and I spending 3 hours on the telephone with a committee member who objected to a section of one of the chapters that we had written. What we learned from that experience was that it was not the content, it was the expression. We had used terms that were too stark; what this committee member wanted was to tone the rhetoric down. We worked with him and we were eventually able to come to an agreement about the wording.

A: Were there any other major things that made it wonderful or difficult?

FG: One of the things that made it so good was that we were all working together. It was a highly cooperative venture, much more so than I had experienced in most academic settings. It was a peak experience for me. But it was not an easy time for me and my family personally. Our daughter, Sarah, passed away during the course of the study. The study was dedicated to her, and to Mansell Pattison.

A: Institute reports go through a review process?

FG: The Institute has an agonizing review process. Nothing goes out over the Institute's name until an entirely separate committee looks at the report and determines that its recommendations are supported by the available data. Some reports had never come out because they had not gotten through review. So the review process was an unsettling one, if in retrospect a prudent one. Fortunately, we passed with flying colors. We also very much wanted to avoid a minority report, an option under Institute guidelines. We felt the impact of the report would be blunted if there was major unresolved dissent among the committee, and this had actually happened to another IOM report in the recent past. The issue that seemed likely to provoke a minority report was controlled drinking, so we were very cautious about that. The thrust of the first RAND report had largely been missed because of the controversy over a small section on this subject. We did talk about controlled drinking in one section, where we construed some controversies in the field as resolvable if you used a matching perspective. We said, 'Maybe it is the case that controlled drinking is suitable for some people,

especially those with mild to moderate alcohol problems, but not for others who have substantial or severe problems.' That was all we did, and the committee was worried about it.

A: Do you think the report changed anything?

FG: I had hoped you would ask that. I do not know. My inclination is to think not. There are two ways of looking at the report. One is that it was a path-making, visionary, idealistic, perhaps even radical depiction of where the field ought to be. The other is to suppose that it captured the *Zeitgeist*, that it reflected what a lot of people were thinking about, and where the field was headed anyway. If that was the case, then I do not think it caused change as much as it was a reflection of changes that were already under way. I think that progress in this field, as in any other field, is largely incremental; so-called paradigm shifts are probably the end result of a multitude of incremental changes. We may have contributed to and or encouraged the ongoing evolution of the field, but I think we largely reflected the thinking of many people as to what the future might be. We gave expression to some of the aspirations in the field, but they already existed. I doubt that we caused anything to happen. We felt very good about the report. It has now been 11 years, and I continue to feel good about it. I do think that in some respects things are moving in the directions that we endorsed, although, to be sure, there is a long way to go.

'We may have contributed to and or encouraged the ongoing evolution of the field, but I think we largely reflected the thinking of many people as to what the future might be'

A: I think, again, we are talking about a commitment to pluralism that is reflected in the report, in terms of: what is an alcohol problem? What is the treatment? What could be the nature of treatment and how do we allocate resources at different levels? It is a perspective that is accepted more in treatment now. Did the report make that happen? The report

certainly has become a base from which people can draw.

FG: I think—I hope—it is a touchstone. People who agree with it can draw upon it and say, honestly, this represents the opinions of a group of people who are knowledgeable about the field. As you know, our field is highly diverse, and there were some reactions to the report at fairly high governmental levels that were strongly negative.

A: Can you give me examples, or are they unspeakable?

FG: Our view of alcohol problems as a continuum, rather than an all-or-nothing phenomenon, was not acceptable to everyone. And many people felt that the only legitimate treatment for any kind of alcohol problem was to join AA and to stay with it, and those people were highly offended by the report. They need not have been, because we were very positive in the way that we dealt with AA and other self-help groups. The fact that we even talked about other self-help groups was seen as a slap in the face. We were also very clear that we did not think that either AA or anything else was *the* answer, and that offended people. Others felt that the report was not sufficiently biological in orientation. But on the whole it was well received and was a good basis for discussion of where we might go in the future.

A: What about the other end of the spectrum, which focused on problem drinkers and moderation and had more negative views of AA—were they critical because you did not exclude the AA and disease end of the spectrum?

FG: No, or if they were, we did not hear about it. I think by that time most felt that controlled drinking was not for everyone, and that there were certain people who were not suitable. Most of the people who had a cognitive-behavioral orientation were very favorable to the report. In essence, the report strongly underscored the validity of those approaches, particularly screening and brief intervention. You know, it is curious: you actually get very little feedback from anything you write. But we had a reunion of the committee that prompted me to look up how frequently the report had been cited, and we were really impressed with the number. It is hard to pick up a general paper in the field now that does not at least mention the report. I think we

are far from realizing the vision embodied in the report. I do not think it has happened, but I think we are moving in that direction. For example, one of the things that I am most happy about that happened since I came here to North Carolina was that we have established a Drink-Wise program here.

A: One of the things that strikes me is your ability to be interested in and involved with things that are so disparate and typically contradictory. You have a whole series of papers over time about AA (Ogborne & Glaser, 1981, Glaser & Ogborne 1982), and now you're doing DrinkWise. On the surface of it, those are completely non-overlapping universes. How do you hold these in your mind or your values?

FG: One of the insights for me that came, at least in part, from my experiences at Lexington, was the enormous diversity of the people who have these problems. They are as different as night and day. They may have certain things in common—they all use more alcohol and drugs than they should—but beyond that they are very different, and I think it follows that there have accordingly got to be a whole range of treatment approaches. All of them are valid in their own way, but not for everybody. Nothing works for everyone, but everyone can benefit from something.

A: In a sense, you are completely non-doctrinaire in a typically doctrinaire field.

FG: I try to be. I am certain I have my prejudices.

'Rabbi Tarfon used to say, "It is not your duty to finish the work, but neither are you at liberty to neglect it"'

Paris in a deserved summer

A: Tell me about retirement. You have been in this field for 36 years. You retired in the Fall. What are you doing? Are you doing professional things or ... ?

FG: Not really. I thought I would. I had the fantasy that retirement would at last give me the time to devote to studying the literature and keeping up with things. What I have found is

that it is time for me to lay that burden down, both in terms of my obligations to my family and to myself. I am interested in a lot of things and I have had to put a lot of them aside in order to concentrate on things in the field. I do not regret what I did, but I remember from studying the Talmud that Rabbi Tarfon used to say, 'It is not your duty to finish the work, but neither are you at liberty to neglect it.' I have not neglected it, but I am not going to finish it. Others will have to do that and I, myself, will be able to spend time with my wife and my son, to read what I want, to listen to music, to travel, to play with my dog, and to do many of the things that I realize increasingly that I have missed very much. Others will take up the good fight. I have said what I needed to say. Others will decide whether they want to take it up or not. I intend to really retire. This summer my family and I will spend the whole summer in Paris. That is something I have wanted to do for a long time, and there will be other such things. My son has now become a Little League baseball player and, in eastern North Carolina, that is an enormous commitment, not only for the child but for the parents. To be at Adam's games and practices seems very important to me. I have worked all my life, and it is sufficient. I have done my share. It is time for me to turn my interest to other things.

References

- GLASER, F. B. (1971) Gaudenzia, incorporated: historical and theoretical background of a self-help addiction treatment program, *International Journal of the Addictions*, 6, 615–626.
- GLASER, F. B. (1974) The treatment of drug abuse in the rural South: application of the core-shell treatment system model, *Southern Medical Journal*, 67, 580–586.
- GLASER, F. B. (1977) The "Average Pack of Help" vs. the matching hypothesis, *Journal of Studies on Alcohol*, 38, 1819–1827.
- GLASER, F. B. (1980) Anybody got a match? Treatment research and the matching by patients, in: EDWARDS, G. & GRANT, M. (Eds) *Alcoholism Treatment in Transition*, chapter 11, pp. 178–196 (London, Croom Helm).
- GLASER, F. B. (1986) Alcohol and drug problems: a challenge to consultation liaison psychiatry, *Canadian Journal of Psychiatry*, 33, 259–263.
- GLASER, F. B. (1993) Matchless? Alcoholics Anonymous and the Matching Hypothesis, in: MCCRADY, B. S. & MILLER, W. R. (Eds) *Research on Alcoholics Anonymous. Opportunities and alternatives*, pp. 379–395 (New Brunswick, New Jersey, Rutgers Center of Alcohol Studies).
- GLASER, F. B. (1994) Slouching toward a systems approach to treatment, *Alcohol*, 11, 467–470.
- GLASER, F. B. (1995) A systems approach without a system design: a commentary on six papers on the Ontario experience, *Contemporary Drug Problems*, 22, 137–150.
- GLASER, F. B. & GREENBERG, W. (1975) Relationship between treatment facilities and prevalence of alcohol and drug abuse, *Journal of Studies on Alcohol*, 36, 348–358.
- GLASER, F. B. & OGBORNE, A. C. (1982) Does A.A. really work? *British Journal of Addiction*, 77, 123–129.
- GLASER, F. B. & SKINNER, H. A. (1981) Matching in the real world, in: GOTTHEIL, E., McLELLAN, T. A. & DRULEY, K. A. (Eds) *Matching Patient Needs and Treatment Methods in Alcoholism and Drug Abuse*, pp. 295–324 (Springfield, IL, Charles C. Thomas).
- INSTITUTE OF MEDICINE (1990) *Broadening the Base of Treatment for Alcohol Problems* (Washington DC, National Academy Press).
- OGBORNE, A. C. & GLASER, F. B. (1981) Characteristics of affiliates of Alcoholics Anonymous: a review of the literature, *Journal of Studies on Alcohol*, 42, 661–675.