

# Interview

Fifty years of the Interactional View – an interview with Janet Bavelas

**Interview by Mark McKergow**

Janet Bavelas is an active Emeritus Professor of Psychology at the University of Victoria, Canada. She was co-author of *Pragmatics of Human Communication* (1967) with Paul Watzlawick and Don Jackson and has conducted experimental research on face-to-face dialogue and communication for half a century, authoring dozens of papers, chapters, and two further books. Janet was at the Mental Research Institute around the time of the birth of Brief Therapy and is now again producing groundbreaking research in this area with SFT colleagues, doing microanalysis of therapeutic conversations. She was an invited speaker at the SFCT research conference *Orienting Solutions 2013*, at the University of Hertfordshire, UK.

**You are one of our last links back to the beginnings of all this stuff at the Mental Research Institute in Palo Alto. How did you get involved in the MRI in the first place?**

It's a weird and totally true story. I was an undergraduate at Stanford, majoring in psychology and taking anything else I could – it was a good liberal arts programme and psychology was in a very boring stage at that point, all stimulus-response research with (at Stanford) a layer of Freudian theory – every course was like that. It was pretty awful [laughs]. But I liked the science part of psychology, and in one term, we had a visiting professor who taught the Adolescent Psychology course – he was a psychiatrist from San Francisco. It was a really interesting course: I don't remember if he had a

textbook, but we read *Tea and Sympathy* and the *Diary of Anne Frank*, and all kinds of neat stuff. He assigned actual articles to read, and one of the articles was by Don Jackson. I was in the reserve room, reading it, and it was like a light came out of the ceiling – because somebody else saw what I saw, which is the people around the individual and the fact that they’re interacting. That, of course, is missing from psychology entirely. So it was just fantastic that this guy had noticed it. Much later I read it again – it’s a terrible article [laughter]. It’s called ‘Guilt and the Control of Pleasure in the Schizoid Personality’ (Jackson, 1958). It’s just awful. But that’s how starved I was, that this was somebody who said, ‘there are people around the schizophrenic (or schizoid, or whatever), and this person is interacting with those people, and they’re influencing him’. That stayed in my mind, and when I graduated, I was looking for work in the Palo Alto area. Stanford had a good employment programme, which had two possibilities for me: one was a really well-paid job with General Foods, doing computer programming – which believe it or not I used to do – and the other was as a sort of glorified secretary/research assistant at this place where the director was Don Jackson. So I took that, for a pittance, just because of that article. It was that simple. I was there full-time from 1961 till 1966, when I decided I’d better grow up and go to graduate school and couldn’t be a child prodigy forever. I was still part-time at MRI and working with Paul Watzlawick a lot till 1970, when I got my PhD and left for the University of Victoria.

**What were you actually doing there – what was your role?**

Well, it evolved. I think they initially called me Executive Secretary, part-time, for the Family Therapy Training Project, which was Virginia Satir’s training project. But that was easily organised into a much smaller job. The other half was as a research assistant. One of my jobs was to be in charge of the reprints – these guys were so prolific that already in ’61 they had a file cabinet of reprints of what they’d been doing, so I

read all of them. That was what I did besides the filing, and then increasingly did the research assistant part, and I guess they found it useful, because eventually they hired another secretary and I was a full-time research assistant. I was, I guess, the utility player. I did things early on like taking minutes at all their meetings, and making sense out of them – and Don Jackson particularly liked that. So he would give me a bunch of notes for some of his early articles (Jackson, 1965a,b), and I was his ghost-writer – staff writer, whatever sounds best. John Weakland and Paul ‘outed’ me (Watzlawick & Weakland, 1977, pp. 2 & 21) and said that I’d had a hand in writing them – but it was a fantastic opportunity and experience, to do professional writing, to have somebody think you could do it and could understand the concepts well enough to do it. It was an unbelievable opportunity.

So I did a lot of that kind of writing and editing. I was really free, pretty much, to do whatever needed doing. I did an analysis of data with Jay Haley, which ended up being a Markov analysis of speech sequences in three-person interactions. I spent a lot of time with John Weakland – he and Paul were my main mentors. I helped John edit a special issue of *American Behavioral Scientist* (1967) on this new look at communication, one that was not electronic and technological – beyond Shannon and Weaver. John was doing film analysis, hypnosis, and all kinds of stuff that was interesting to talk about. Paul and I were systematically watching a lot of therapy sessions. The MRI building had a lot of therapy rooms with observation mirrors and reel-to-reel audio recordings, so anytime anything was going on, I could go watch it. It was amazing. Paul and I did a lot of watching, especially of Don Jackson. We were interested because he was an amazing intuitive clinician. He would do things in a session, and Paul and I would look at each other and go, ‘What? Where did that come from?’ Listening to the tape later, we could figure out that he’d picked up a whole bunch of things, very subtle things, and was building on them. He had an amazing ability to do that – to read interactions, in a way, and make a lot of really interesting analyses of them. So I always say – and it’s

true – that *Pragmatics* started because we tried to get Don to say how he did it, and he hated that [laughs]. He said, let's write a book [laughs] – and we did.

When I first got to MRI, it had a lot of money, as it was very popular with public and private funding organisations, but that began to close down by the mid-60s. It was the beginning of biological psychiatry taking over the funding. There were a lot of political things going on with the funding (mainly attacking what they called 'family blaming', so our funding was beginning to close down. Don thought it would be a good idea to gather the people who were mostly at the MRI as well as the separate Bateson Group and to draw those ideas together. There are two books (Jackson, 1968a,b) that are collections of all the publications published between 1956 (when the Double Bind was published) and less than 10 years later: two volumes of papers that they'd written on families, on communication, on the early approaches to Brief Therapy – it was an absolutely amazing group. So Don thought that this ought to be integrated somehow; it's something that Paul and I would've been very interested in at the time, so that's how *Pragmatics* started formally – and Don, once he knew we'd started, as usual went off to other things, which was fine [laughs]. Paul and I divided up the chapters; I've outlined that in the tribute to Paul after he died (Bavelas, 2007). I still read parts of *Pragmatics* and I cannot tell in some places which of us wrote it. It was a great, great writing relationship, really. We're both 'OCD,' and I have found only one typo in the book [laughs]. So that was *Pragmatics*, which took a couple of years of work there, starting it in about 1965 when I was 25 until it was published in 1967. Then with funding closing down and that project finishing up, I decided I'd better get a PhD.

I'll mention one thing before I go on about where things went after that: MRI was really marginal. When people do this kind of historical interview, where I serve as their historical artefact [laughs], they always think it was really important, as important as it became and is now – but we were really, really off the beaten track. We were off-off, probably. Don had an

appointment in Psychiatry at the Stanford Medical School, and he lectured there. I think eventually Paul and John did, too. But that was pretty much it; we were really ‘that weird communication group in Palo Alto’ – which was great. If you have a good idea, and it’s off-centre, excellent. Really excellent combination. Missing either of those is not good. Very few of the people at MRI had regular degrees. They were brilliant people, such as John with an MA in Anthropology and a Chemical Engineering degree, and the odd psychiatrist to keep things legitimate (because we were a part of the Palo Alto Medical Research Foundation, within a world-famous medical clinic). Later the MRI broke that connection, but there was still that advantage in having a psychiatrist.

So anyway, so I decided I’d better get a PhD – not just for the credentials, but in order to become a researcher. Everybody else there was a therapist, and although there were some attempts to do research, they were not capturing things we were proposing – not just the axioms in *Pragmatics*, but the approach, the whole idea of looking between. You and Harry (McKergow & Korman, 2009) have pointed it out too – and it still needs pointing out, right? The space between. And I thought it’d be good to have a research method that could try to capture these things. I wasn’t sure what research method, but I figured that if I did a good PhD, I could find one. So first I went into Stanford’s Communication Department for one year, but it turned out just to be about mass communication, so I moved back to Psychology and social psychology. I quickly discarded surveys, interviews, correlational studies, and psychometrics, but fortunately landed on experimentation. Very few people say ‘I love experimentation’ [laughs], but I do. For me it’s an artistic thing – I’m not talking about statistics or statistical design, but about designing an experiment connecting these abstract theories to things people actually do on the ground – figuring out a way to make something visible and to be able to reach some kind of conclusion from it. It’s something I still hugely enjoy,

and I had fantastic teachers. None of them in this area, but they taught me about the elegance of experimental design. That is really what I've worked at, basic research in lab experiments, that's what I do.

So that was my PhD. One of my examiners said at the orals, 'If you take what you've done and try to apply it and extend it to interaction, it will just blow out of control'. He imagined this decision tree in which, at every point, there would be new things, and new branches, and those branches would tangle. I was just polite, because what we'd pointed out about systems theory in *Pragmatics* is that the whole point of a system is its limitation. It's that people don't do 'anything or everything.' The whole idea of systems theory is that there are constraints, which form patterns. And those patterns are general, at least within a culture, because I can go into a shop here in the U.K and say, 'Do you have travel-size toothpaste?' to a stranger who speaks a dialect I'm still getting a little used to – and we can have a conversation. These patterns of dialogue are there all the time, and the idea was to try to find experimental ways of capturing them, and that's what I started to do.

When I finished my PhD, I was very fortunate to move to Canada (mainly for political reasons; I'm a pacifist and a Canadian citizen now, and have been since I was able to change almost 40 years ago). I came to a good university, a growing university that was only two or three years old when my husband and I came in 1970, which was attracting people, punching above its weight, as they say. It was attracting people who were very good in research in a lot of areas, because it's such a great place to live – it was really doing very well, quite open to new things. So I spent a few years honing my craft, and then, in the mid-70s, started slowly creeping up on actually studying interaction. That's what I've spent my research life on – with a great research team – and what is still my main activity – doing basic research in the lab on how face-to-face dialogue works. How do people manage to do it so brilliantly?

**Can we just go back to *Pragmatics* for a minute. It's one of these books that everyone has. Has that book done what you had hoped it would do, did it open up the things you hoped it would open up?**

Mostly, yes. It's now in Japanese and Greek, too, it's in eight languages. I think it has had an influence in the therapy world (though I would expect not so much with recent generations). But enough people read it, and say, 'Wow, that made a difference to me' – I still hear that. It was like my experience 'Oh, somebody sees what I see'. So I think that in the therapy world, people probably tend to know it. And the other big area – actually much bigger – has been in the discipline of Communication, which is a huge academic discipline in the US, and there somebody said it was one of the top three original books in the field of interpersonal communication, one that got that field started and where it's still well known. I think interpersonal communication's become much more social psychology, unfortunately, but *Pragmatics* has had a big influence, at least in the sense that most people have had to memorise parts of it!

In therapy, I'm always surprised at how many people know me by that book (especially since I managed to change my name afterward), so I think it's had an influence. The qualification is that it has not always been a good influence, sometimes because things are misinterpreted, and sometimes because we just said some wrong things. I dutifully try to publish those [laughs]. For example, the whole thing about 'one cannot not communicate' – we said that, and it's true, if you keep it in context. But the 'all behaviour is communication' is a nightmare. We were not the first to say it, but we put it out there memorably, and it's led to all this 'reading body language' and 'anything I think you were communicating, you were communicating,' which are really wrong, fundamentally wrong. A few years ago, I published a logical analysis of why it's wrong and what might be right (Bavelas, 1990).

## So what should it have said?

That in a social situation, one cannot not communicate. You will do some kind of signalling, which makes it trivial. For example, if I'm in an elevator, and I don't have a speaking relationship with the other people there, I will still communicate that I'm not communicating with them, and those will be a deliberate actions. But that really doesn't get us closer to dialogue, which is what we wanted to talk about in *Pragmatics*, the in-between. So I think it's not a very helpful principle, even when it's accurately qualified. However, our later work on situations that evoke equivocation (Bavelas, Black, Chovil, & Mullett 1990), which was called 'disqualification' in *Pragmatics*, did come directly out of that – when and why people 'try to say something without really saying it'.

The problem is with 'all behaviour is communication.' I would now be more precise about the nature of communication, especially about automatically accepting all non-verbal acts as communication, when only some of them are. I've spent a great deal of my career looking at 'co-speech actions,' so in fact I don't even use the terms 'verbal versus non-verbal' any more, I talk about 'audible and visible' and about those visible acts that are closely related to speech. Specifically, the hand gestures, the (non-emotional) facial gestures and gazes that occur in face-to-face dialogue are the that ones we've shown empirically are very tightly tied to speech; together with words, they form an integrated message (Bavelas & Chovil, 2000, 2006; Bavelas, Gerwing, & Healing, 2013a). All the other stuff about what it means when I'm crossing my legs or my arms is just garbage [laughs]. I feel strongly about this. What's usually taught about 'non-verbal communication' just drives me mental. My students used to tease me by giving me those supermarket booklets on 'reading body language,' and 'how to know if someone wants to sleep with you,' and all that stuff – just to drive me nuts. So that part I would change. It was what everybody thought at the time, and *Pragmatics* was never a research book.

If you actually look more closely and do research, that verbal/non-verbal dichotomy doesn't hold up. Something much more interesting appears – namely that you can sort out which



visible acts *are* communicative and which are not. Sorting that out experimentally has been very important to me. For example, we did an early experiment (Bavelas, Black, Lemery, & Mullet, 1986), on a phenomenon called ‘motor mimicry’ (first noticed by Adam Smith in 1759), which is when you hurt yourself and I wince. That’s always been interpreted as a reflex or as an expression of empathy – all kinds of intra-psychic interpretations. We got the impression, mostly just by watching it a lot, that it was communicative. So we did a study in which the experimenter apparently injured himself very badly (dropping something on his hand) in two different experimental conditions. Right after the injury, he either turned away from the participant and toward the other experimenter, or instead turned toward and looked directly at the observing participant. In the eye-contact condition, the participants started wincing (and/or saying ‘Ow!’) within a second or so. Without eye contact, they either started to wince and quickly dropped it or didn’t wince at all. In other words, the injured person had to be able to see the participant’s wince, because it’s a message showing him that the participant understood that it really hurt. That effect was a breakthrough for me in a lot of ways, showing that *some* visible acts are very precisely communicative – that they depend on another person seeing them.

Interestingly (and not necessarily as a digression since we’re talking about the effect this approach has had), a few years ago I did a citation search on that experiment (Bavelas, 2007), and found about 50 refereed articles that had cited it, and only a handful got it right. It’s a real lesson. The rest of them re-interpreted it as an intra-psychic event – for example, as empathy. They reported the procedures backwards or wrong and eliminated the effect of eye contact! So people don’t read carefully, but the more interesting thing for me is that the changes all made the findings conform to their individualistic theory – and got rid of the communicative evidence, which is what it was about. So it’s a lesson in one’s long-term impact!

There was another thing I wanted to mention about *Pragmatics* and it’s also about the verbal/nonverbal distinction,

which as I said I don't think is a good dichotomy. But there is a part that's actually revived in our recent studies: the digital/analogic distinction, that is, whether a communicative act has an arbitrary, conventional meaning or actually looks or sounds like what it means. Bateson got that from cybernetics, but it's a widespread distinction: de Saussure talked about symbols and non-symbols, Grice called them non-natural (conventional) versus natural terms; Peirce said symbols versus icons; and Herb Clark, my colleague and friend at Stanford, called them describing versus demonstrating. I can *describe* something in words with conventional meanings – which would be digital – or I can *demonstrate* something. Many demonstrations are visible, like hand gestures and facial gestures: I can look quizzically at you, for example, demonstrating that I'm sceptical about what you're saying (or demonstrate motor mimicry, as I mentioned earlier). But demonstrations can also be audible, such as onomatopoeia or quoting someone. We have sort of stumbled on the idea that demonstrations are tied to dialogue, so that they are significantly lower in monologue. Our experiments have shown that relationship for hand gestures, figurative language, facial gestures, and direct quotations (Bavelas, Gerwing, Sutton, & Prevost, 2008; Bavelas, Gerwing, & Healing, 2013b). Participants in a dialogue use demonstrations at a higher rate than when talking alone about the same material. All four of those demonstrations are what we would have called in *Pragmatics* analogic communication. But notice that two are 'verbal' and two are 'non-verbal,' which it makes that dichotomy useless, whereas the digital/analogic dichotomy is predictive. The interesting thing now is, why does this happen? What is it about dialogue that increases demonstrations? Or perhaps, what is it about monologue that suppresses them?

The other thing about *Pragmatics* is that we said contradictory things about systems theory (Bavelas, 2011). I should say 'I' instead of 'we', because I wrote those chapters, so I'll wear it, and one of those things is wrong. What's right is the idea of systems as mutual influence – and constant mutual influence - which is what dialogue is, and that's what I study.

But what happened, because we applied it to ‘this kind of family system’ and ‘that kind of family system’, is that systems became frozen into typologies. So ‘this family has a fixed pattern’ – and that pattern was determined by the presence of a diagnosis of a certain kind. Reifying a system in that way, I think, has been a real mistake. I wince when I see ‘system’ in that sense, but it’s very widespread, mostly thanks to *Pragmatics* and the earlier articles, which I also contributed to. In short, I think *Pragmatics* has mostly has been a good influence. It needs correcting and fine-tuning with subsequent research, but the basic message about interaction needs saying as much now as it ever did.

### **You mentioned Clark? . . .**

Herbert H. Clark. Get his book, *Using language* (1996), or look at his website. He started in the early to mid-80s, at about the time that I was getting into being able to do experimental research with two people in the room freely interacting. About the same time, he started doing that, too, and he’s done some brilliant experiments on interaction. He’s a psycholinguist – off the beaten track also – who has developed a collaborative theory of language use in dialogue. It is exactly everything I agree with, and in fact, I gave up systems terminology and adopted his – because it’s clearer. Systems terminology was designed for biology, and his terms for describing dialogue are designed for language use, and they work a lot better. He has conducted experiments showing that we shape meaning together in dialogue and has done some amazing experiments on this (e.g., Clark, 1992).

**You were saying that the messages from *Pragmatics* still need saying. One of the things that strikes me is how much it still needs saying. Is there a sense in which you’re surprised, or disappointed, or whatever, by the pervasiveness of individualistic approaches?**

Not really, it’s a hard point to make heard. I remember in the mid-80s, saying to a graduate student, ‘If people are noticing

this in 25 years time, that's what I'm aiming for'. And I was a lot luckier than that: I've been very well honoured within my field, and within Canada, and elsewhere for it, so I didn't have to wait 25 years to get my own recognition for our research. However, that didn't mean that the paradigm changed. There's so much that produces a reductionist mind set, especially the cultural individualism of the western world, and especially of the U.S., which tends to dominate research approaches. (That's not just a cliché; one of the things I love about Canada is that it's less individualistic.)

But there is also the scientific notion that the natural unit of study is the biological unit, the individual. I've written against and about this a lot, because I think it needs deconstruction (e.g., Bavelas, 2005). Danziger (1990) has shown how carefully the myth of the individual has been constructed. And that focus on the individual is not just in psychology and psychiatry, it's in all the other counselling and helping professions as well. The belief that you start with individuals is not just a cultural idea, it's also a scientific belief – a very mistaken notion about what reductionism requires. It's very hard to fight the notion that, to do good science, you must go to the minimum unit. But it's just not true, and most good scientists know that it's not true. I mean, the neuropsychologist Luria (1987), who was by no means a rabid social psychologist or interactionist, said people are getting it wrong. What you have to do is to reduce to the minimum *meaningful* unit. He gives a great example: if you want to study the nature of water, you don't break it into hydrogen and oxygen. There's an appropriate level that you don't go below. But I don't think people understand that; they still have the belief (and often say it very explicitly) that if they get down to the individual, then they will really understand what's going on. That one's very hard to get past. People think in terms of this biological package we're in, and not the invisible layer between people.

However, we're beginning to have studies that show that you get different things in interaction from when you study individuals – like the effect on dialogue versus monologue on

demonstrations, above. There's a review in press (with my brilliant co-author, Sara Healing, who spotted the pattern (Bavelas & Healing, 2013)) showing that, in a particular experimental paradigm, using a non-reactive confederate to create a quasi-dialogue produces different results from using two real participants in a free dialogue. I think that kind of evidence may have an impact – although when our article is out, I expect that I'll have fewer friendly colleagues in social psychology, because any time I've tried to say that, people get really angry.

I think psychology, psychiatry, the helping professions, even the media, have moved towards cognition, and now to the brain – the ultimate reduction, which presumably causes everything [laughs]. It's all about some area of the brain, the new phrenology – it's crazy. (Even better if they can make up a genetic or evolutionary explanation, because those don't even require data.) So I think there are a lot of barriers to seeing interaction, but there are still people who do see it and who are excited about what they see.

### **How did you reconnect with the solution-focused therapy world then?**

After I left for Victoria, every time I visited Palo Alto, I'd go back meet with the Brief Therapy group. (By the way, that's what Weakland, Fisch, and Watzlawick always called themselves, 'Brief Therapy' in capitals, never 'Strategic' or 'Systemic' therapy). I'd watch them work, and we'd talk about language in therapy. They really started it all (e.g., Bavelas, 2010), about using language precisely, with terms like reframing to articulate what they were doing in therapy. I probably crossed paths with Steve and Insoo around the MRI, but I never had a chance to ask them whether we actually met then or not. I had the sense I knew them, and they had the sense they knew me. But some colleagues near Victoria started inviting world-famous therapists to do workshops there, which was great. People like Michael White, Alan Jenkins, Boscolo and Chechin, and of course Steve and Insoo. Two of the group

inviting them were my graduate students, who had this idea that when these folks came to do their workshops, we would give them the opportunity to spend a research day at the University, hearing about all the research that we were doing in the lab, which was completely experimental work. Well, most were not interested, but Steve and Insoo lit up. It was a reunion of minds. They understood immediately – although we weren't doing any therapy research at all, it was only about language and interaction. It was very exciting.

That happened in 1996, and Insoo immediately started sending us videotapes. My students started doing microanalysis with them. We had developed microanalysis for experimental work, so it was natural that that's what we'd do, and that's why Steve and Insoo were excited too. The first graduate project was by Bruce Phillips (1998, 1999), who did his thesis on formulation in mediation sessions (including Insoo's *Irreconcilable Differences*). That kind of thesis work increased, and eventually an introduction (Bavelas, McGee, Phillips, & Routledge (2000) and studies were published: McGee (1999; McGee, Del Vento, & Bavelas, 2005); Tomori, (2004; Tomori & Bavelas 2007). (By the way, Steve was the Dean's External Examiner for Dan McGee's doctoral dissertation. That was really cool!)

Then Insoo really turned on the burners. Around 2005, 2006, or so, she started very actively pushing me forward, especially at SFBTA. But probably the single most important event was the memorial tribute to Steve in Amsterdam in 2006, which was the first microanalysis talk I'd given to an SF group, and the reception was amazing. So she got a lot of our work out much more publicly in Europe and at SFBTA.

Then a number of people, including Sara Smock, came up to stay in Victoria for a while to learn. Harry Korman and I started meeting when I was in Europe. Insoo, Harry, Peter De Jong, and I agreed to do a workshop together here in Victoria, and she expanded it but tragically died before we could do it. So my group held that first workshop anyway, in Victoria in August 2007, which led to our ongoing workshops on microanalysis for practitioners, now mostly in Europe and at

SFBTA, teaching practitioners and trainers microanalysis. Thanks to Skype, we now have a cohesive working group of Harry Korman, Peter De Jong and Sara Smock Jordan (she's using her married name now). We're a very dedicated and productive group, as shown in our special journal sections in *Journal of Systemic Therapies* on SFBT research, listed at the top of the Reference section here. We continue to work on projects – in fact, we've made an improvement on Clark's theory of grounding (Bavelas, De Jong, Korman, & Smock Jordan, 2012), which is leading to my next basic research project here in Victoria.

### **Are you still using the Steve and Insoo tapes, or do you have some newer ones now?**

Well, they made such a great library of tapes, they will be a source forever. We have also compared SF to Cognitive Behavioural Therapy and Motivational Interviewing, but in those approaches there are so few tapes available of real sessions. With private tapes, there are restrictions because of confidentiality considerations: if you're publishing something, the data should be publicly available. So we've taken the tactic of looking at best practices in every approach. That is, we're analysing tapes that show what the best people in an approach (who are teaching it, or are founders, or established teachers) are doing. We examining whether their practices are congruent with the theory they're working in and whether they're consistent with others within the same theory. I think these are legitimate questions. All of that work is just coming out in the special section of *JST* that I mentioned earlier. Among ourselves, we have analysed some tapes of Harry's and some of Peter's, but we cannot distribute them; it's a real restriction. The other problem is that most videos reveal an individualistic bias—the camera is on one person at a time. I'd love a Master Series, with either one camera on each person or one camera recording them both, so we could have publicly available videos to show and analyse. That'd be great. I wouldn't know how to organise that, but it would be a great thing to have.

It has to be video. If you don't have video when the two people were talking face to face, it's like taking a novel by Jane Austen and randomly deleting a whole bunch of words or sentences. You're just not getting it – because there are more obligatory or non-redundant co-speech acts (to use the technical terms) than one would guess.

**You were there in the MRI watching through the mirror with John Weakland and Paul Watzlawick and Don Jackson ...**

And Virginia Satir and Dick Fisch

**So you've watched them, and you've watched Steve and Insoo a lot, and you watch Harry, and Peter, and presumably other people, as well. What strikes you about how things in this field have moved over 50 years?**

Oh, I think it's a wonderful evolution. As I pointed out earlier, the Brief Therapy people were introducing and implementing these ideas of using language as an instrument, not making intra-psychic inferences, not interpreting, and not diagnosing. Their idea was 'We're going to look at what people do observably, and we're going to do observable things that we think will influence them, and we'll look to see if it influences them'. Just shedding all that old stuff. And so, to me, and I'm certain to John and Steve and Insoo, the SF Brief Therapy was a natural evolution. Granted that it took one principle and rotated it 180 degrees – but John was delighted at anything that was 180 degrees off. And there's a short article by Steve and Insoo (Berg & de Shazer, 1991) that shows the close connection – how they just changed the order of what to consider first. To me there's a straight evolution of the Brief Therapy approach to the solution-focused brief therapy approach. It's still 'Let's take it seriously; let's take seriously this non-pathologising, non-inferential, not making up things going on in people's heads just so we can diagnose them – just leave the old stuff out'. There's a kind of discipline to that thinking that



I find really appealing, and SF has absolutely gone straight ahead with the same intellectual discipline. So I guess I'm a really right-wing solution-focused person [laughs].

### **By right-wing, you mean . . .**

The only place in my life that I'm right-wing! 'Left-wing' in relation to the research fields I work in, but in terms of SF, it horrifies me to hear that 'SFBT is just like CBT or positive psychology', or whatever. That's not being open-minded; it's sloppy thinking.

We have to be clear about the differences and not try to blend in to be accepted. There's been huge progress for SF, and it's growing, but it isn't going to take over the world, because there're a lot of professionally and economically established systems like the American Psychological Association, which has become a commercial corporation that promotes one thing and sells it as evidence-based. It doesn't make me proud to be a psychologist. So we have a new and different approach, this kernel of thought that was in *Pragmatics*. To me, the best of that went into becoming Brief Therapy and, as it developed, very naturally shifted over to the SF approach. Remember that Steve and Insoo named theirs the Brief Family Therapy Center, marking it as a really clear connection to the Brief Therapy Center where they trained in Palo Alto. I'm always suggesting that colleagues ought to cite something from those Palo Alto guys, because they often said it first. We can't say everything started with Steve and Insoo, because they wouldn't have said that.

In short, I think it's all evolved, and it's gone a good direction, so you have the SFBT founders who wrote books like *Interviewing for Solutions*, which has real examples on video, or *Words Were Originally Magic*, where there's that same commitment to language. When I talk to SF people, they're excited about microanalysis and catch on much more quickly than others do; they recognise the compatibility.

**We're here at this SFCT research conference which is, I think, the first academically-centred SF research conference with many strands coming. One of the questions that's going to be around in various forms is about evidence-based practice – to play the game or not? There are some people who think we should play the game, and there are those who think it's the wrong game, and we should be pointing that out.**

Both! Of course, I would just naturally cut across that question orthogonally. First of all, we should do it—practice should be evidence-based. Right now, there's a game going on, no question, with randomised controlled trials (RCTs), and that offends the pure researcher in me. Unfortunately, it is a highly-politicised game, because people stand to make money and sell themselves as professionals – and the APA's very, very guilty in the way they're playing this game. They've erected this monolith on CBT's being evidence-based—'the only evidence-based therapy'. That's a myth. So I think it's important to fight that, and people like Alasdair Macdonald (2011), Sara Smock Jordan (Smock et al., 2008), Johnny Kim (Kim et al., 2010), the book that Cynthia Franklin and others edited (Franklin, Trepper, Gingrich, & McCollum, 2011) are doing that very well. We can play the game honestly, saying 'Look, if that's your criterion, we're doing it'. And we have to do it because if we don't, then people remain intimidated by CBT and other approaches, and they'll say 'Well, I still like SF, even though it's not evidence-based'. We have to make clear that there's just as good evidence that SF works as CBT. CBT has a huge (and unfair) advantage, because there're a whole bunch of different therapies with that generic name – I think their website (<http://www.nacbt.org/whatiscbt.him>) names something like seven different sub-CBTs, and they say explicitly that "cognitive-behavioural therapy does *not* exist as a distinct therapeutic technique" (NACBT, 2009; emphasis original). But then they aggregate the evidence for all of them, so they (apparently) have several times as much evidence as any homogeneous therapy like SF!

But RCT's are also the wrong game to be playing exclusively. The definition of 'evidence-based' as randomised controlled trials is just plain wrong. It isn't true to the original people who introduced the idea of an evidence base. They soon wrote very strongly about this, saying, 'Guys, you're getting it wrong – We said the *best* research evidence, not just one kind' (Sackett et al., 1996). But what's happened is that some benighted soul started calling RCTs the 'gold standard.' – but it's fool's gold, because it's one that's easily corrupted. In 2004, the world's most well-known medical journals (e.g., Editorial, 2004) formed an agreement that they would not publish RCTs unless the design was published and submitted in advance. And that's because people change things along the way and even cheat, retrofitting the design (e.g., Piggot, Leventhal, Alter, & Boren (2010)) to whatever results they can get out of the data. It's not the gold standard because it's very vulnerable – probably because this has become more a game than research.

However, even if RCTs are done perfectly, their implications are limited. They cannot tell you *how and why* the effect is occurring. It just an input-output design, which can't tell you anything about how it's happening. For that, you need other kinds of research that complement RCTs. I think the evidence base needs to be a lot broader, much more varied (e.g., Bavelas, 2011). And if you do that, we get out of 'the game', and start actually learning things about doing therapy. For example, if we also do microanalysis as one part of a broader evidence base, then we know more about how therapy works and how to teach it and how to practise it. Another example: Psychology really capitalises on its implicit support in an experimental research base. But we have a strong research base too, in language research like Clark's or our group are doing, among others. I've proposed that there's experimental lab research supporting co-construction and a lot of other practices that SF engages in (Bavelas, 2011). The two special sections of *Journal of Systemic Therapies* that are now in press are devoted to exactly this issue: presenting a much wider variety of research methods that give us more information about therapy – information that is more relevant to practitioners as well.

**You've been in this business a long time. What are you looking to, looking forward to next?**

Oh gawd – just all this good stuff. In the basic research, we're wrapping up the studies on demonstrations and dialogue. That has been my passion for about the past five years, since we discovered that really interesting link, but once it's all published, I figure other people can take it over.

So what I really want to do now is test our new theory of grounding (Bavelas et al., 2012). I think we have made an important modification in the original theory of grounding – and Clark agrees that we have – so I want to see how it works out in a very large set of video data – what are the variations on the pattern? This is very different for me because it's not an experiment – and that's exciting too, to develop a new method. Because I don't know what's going to happen, I find that really interesting.

And what else? I'm continuing to work with therapists, with Harry, Peter, and Sara. There are also other people I consult with from time to time, because they said, 'We want to do a project, will you help us, and consult?' I do a lot of that, and because it's their idea, it's interesting and new (and they do the work!). For example, there's this really insightful grad student in Sweden, who's talking about 'agentive language': all the ways in which clients present themselves, not as passive blobs being acted on, but as agents in the world. She's very good at nailing the language that they use, the ways they talk about things and themselves that – if only therapists would notice it – would show that clients don't need to be 'empowered'. They are already, we just have to recognise it.

But I guess number one right now would be the grounding research, which includes both experimental and psychotherapy data. And I'm writing a lot: articles, chapters, and even a couple of books. I really enjoy writing. I've been doing it professionally for 50 years and always discover new ideas in the process.

**Thank you very much.**

## Special Journal Sections on SFBT Research

### *SFBT Contributions to Practice-Oriented Research. Part I: Microanalysis of Communication*

- Smock Jordan, S., & Bavelas, J. B. (Eds.). (2013). Introduction. *Journal of Systemic Therapies*, 32, 14–17.
- de Jong, P., Bavelas, J. B., & Korman, H. (2013). An introduction to using microanalysis to observe co-construction in psychotherapy. *Journal of Systemic Therapies*, 32, 18–31.
- Korman, H., Bavelas, J. B., & de Jong, P. (2013). Microanalysis of formulations in solution-focused brief therapy, cognitive behavioral therapy, and motivational interviewing. *Journal of Systemic Therapies*, 32, 32–46.
- Smock Jordan, S., Froerer, A., & Bavelas, J. B. (2013). Microanalysis of positive and negative content in solution-focused brief therapy and cognitive behavioral therapy expert sessions. *Journal of Systemic Therapies*, 32, 47–60.
- Froerer, A., & Smock Jordan, S. A. (2013). Identifying solution-building formulations through microanalysis. *Journal of Systemic Therapies*, 32, 61–75.

### *SFBT Contributions to Practice-Oriented Research. Part II: Changing the Language of Clinical Practice. (In press)*

- Bavelas, J. B., & Smock Jordan, S.A. Introduction.
- Richmond, C. J., Jordon, S. S., Bischof, G. H., & Sauer, E. M. (2013). Effects of solution-focused versus problem-focused intake questions on pre-treatment change.
- Beyebach, M. (2013). Change factors in solution-focused brief therapy: A review of the Salamanca studies.
- Sanchez-Prada, A., & Beyebach, M. (2013). Solution-focused responses to ‘no improvement’: A qualitative analysis of the deconstruction process.
- Jordan, S. S. (2013). Asking different questions: Validation of the Solution Building Inventory in a clinical sample.

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