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Continuity

Janet Beavin Bavelas

Department of Psychology, University of Victoria

Victoria, B.C., Canada

Q: Whatever happened to Janet Beavin?

A: She became an experimental psychologist, a Professor in a traditional psychology department, conducting lab experiments with "normal" strangers. She is not now and never has been a psychotherapist (although some of her best friends are therapists).

Given these accurate biographical facts, most people would see only discontinuity between my past and present professional lives and would assume that, shortly after the publication of Pragmatics, I started a new career with a new name in a new country, thereby ending all connection to the Palo Alto Group. In my view, however, there is a strong, clear, and direct connection between the way of working then and now. Indeed, there are such important debts to the Palo Alto Group that we call our research team the "Victoria Group" as a direct tribute. There are at least four guiding principles which I learned then and still try to follow today:

First, stay inductive--stay focused on the phenomenon. I am particularly indebted to John Weakland for encouraging me to look (and see) for myself, that is, to watch how a behaviour actually happens. To do so requires putting aside the lenses that are other people's ideas of what a behaviour "should be" or "really" is. These borrowed lenses most often take the form of widely accepted theories and concepts. For example, Weakland (1967) pointed out that preconceptions about "good" communication were a barrier that actually prevented accurate observation. It is very tempting to get lost in the tidy and often beautiful logic of theories, which (not incidentally) we ourselves create and control. But nothing new can come out of applying pre-existing concepts; new concepts and theories can only arise from new observations. "Theory-driven" research is inherently conservative; it preserves established, authoritative views. The alternative is to respect and to learn from the data.

The inductive approach is often scary, like leaving a good road to explore an apparent wilderness. You have to trust this wilderness, but you quickly learn that you can and that you do not have to pave and destroy it in order to enter and explore. You need only respect the data and refrain from making it into something else. This is a profoundly reassuring experience: Countless times, I have found myself in a concept-induced muddle

and have felt as if the data took me by the hand and led me forward.

Sophisticated, constructivist readers may be doubting the possibility of pure unbiased observations or induction, but that is not what is being proposed here. Of course we make choices about what to observe and what we are looking for, but these need not blind us unless we choose to be blinded (that is, unless we design our studies to avoid risk). If I were trapped by my own mind and could only see what I expected to see, I would never be disappointed, frustrated, or just plain wrong about the data and (contrary to what the following article shows), the original propositions in Pragmatics would be unchanged. To continue the exploration analogy, we can choose where to explore without determining what we will see there.

The second legacy of the Palo Alto Group is about where to explore: look at behaviour as communication and at communication as social interaction. There is an alternative to using behaviour solely as evidence about the mind, as a route that only goes inward. We are not limited to explaining behaviour from the inside, as a by-product of mental processes. The intrapsychic focus that we eschewed in Pragmatics has its roots in the 19th century, in the origins of our disciplines in philosophy and mentation. Like the small-windowed houses of that era, the view

is closed-in, inward-looking, agoraphobic.

The alternative is a new, open vista, looking out to where the behaviour goes, to the other people out there. Behaviour goes to other people; it does not simply drop off into space! Communicative behaviours connect people and thereby create new phenomena, which are separate from either person. Just as modern physics shifted from the study of separate planets to the study of the field between them, we can shift from studying individuals to studying the relationships between them. Those relationships exist in observed interactions, and if we look there, we will see new, unexpected phenomena.

The third lesson I cherish is to collaborate. You will notice in the publications of the Palo Alto Group that these talented individuals appear many combinations as co-authors: Bateson, Jackson, Haley, and Weakland; Watzlawick and Jackson; Weakland and Fry; etc. The best researcher and colleague is one who has a strong sense of his or her own ability and a fierce desire to achieve but is also open to collaboration and generous with ideas. As a 21-year-old with only a brand-new undergraduate degree, I benefited greatly from this open atmosphere. Everyone helped me learn, came to accept my contributions, and especially Paul Watzlawick insisted that I be co-author when appropriate. Today, the vast majority of my publications are also co-authored,

and the collaboration goes deeper than authorship.

Intellectual collaboration in the development of ideas and in writing is a real "high." I do not understand how people work alone; it seems a grim alternative with too much emphasis on status, on who has a right to have ideas or to get credit for them, and (worst of all) treating ideas as personal property to be guarded or stolen. These sad people fear that their own ideas are limited or rare, so they build walls that make this true. In fact, the best way to generate ideas is to share them openly.

Our research team is a mixture of graduate and undergraduate students and some practitioners from the community. Research is conducted through frequent, non-hierarchical meetings. It is not simply that each individual brings ideas to those meetings and transmits them to others. Rather, the discussions generate the ideas. It is not possible or desirable to trace who "had" the idea, and it is an illusion to credit the idea to the one who merely articulated it. The person who argued most strongly against the idea contributed equally to its development.

Finally, I learned all those years ago to stay off-beat, even off-balance in relation to the Establishment in one's own field. People are always surprised to learn that, at the time, the MRI might as well have been on the moon (or in Victoria!). It had virtually no connection with Stanford, unless scorn can be

called a connection. The group was considered very deviant, with few "proper" degrees among them and with very strange ideas. (Indeed, when I went on to graduate school at Stanford in 1966, I learned very quickly to keep my MRI work and my strange ideas to myself--while financing my studies, in part, from the royalties on Pragmatics.) Yet compatible people in the wider world knew of our work and came to visit, as we did them. We were on a virtual migration route for like-minded people from anthropologists to psychiatrists.

It was only a few years ago that I realized that I was recreating the best aspects of this curious mix: I study non-psychological phenomena in a traditional psychology department, which is pleased but puzzled by whatever it is that I do, by the variety of journals I publish in, and by my collaborations with other departments. I seldom attend psychology conferences but appear, instead, as the only experimental psychologist at gatherings of communication researchers, family therapists, discourse analysts, or language scholars. It is essential to me to be always slightly out of place. It is also essential to make connections with the like-minded, to constantly seek out people who may or may not know that our approaches are similar. And, in particular, I visit my old friends, colleagues, and mentors in Palo Alto, where Weakland, Watzlawick, Fisch, and Beavin talk

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more about what we are doing now than about the old days.

Reference

Weakland, J. H. (1967). Communication and behavior: An introduction. American Behavioral Scientist, vol. 10, no.8, 1-4.