

Article

On Research: Reply to Jim Stephen's first version of this article from 1999

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Abstract

This article has very nicely articulated the criteria used by medical groups to “evaluate” research, and reports on how research on the Feldenkrais Method “stacks” up (not very well). Although the article presents this as if the question is simply a scientific one, what the author has presented so well is the basics of the political mountain we in the Feldenkrais community must climb in order to get recognition from the medical and scientific community. He is, of course, right in this. But there is another side of the coin. The response considers the basic assumptions behind this medical model of what is valid.

Keywords

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ON RESEARCH

By Carl Ginsburg

"The closer one looks at a real-world problem, the
fuzzier becomes its solution."
-Lofti Zadeh

In a piece entitled, "How does the world evaluate research on the Feldenkrais Method," (In Touch, Third Quarter, 1999) Jim Stephens has very nicely articulated the criteria used by medical groups to "evaluate" research, and he has also reported on how research on the Feldenkrais Method "stacks" up (not very well). Although this is presented as if the question is simply a scientific one, what Jim Stephens presented so well is the basics of the political mountain we in the Feldenkrais community must climb in order to get recognition from the medical and scientific community. He is, of course, right in this. But there is another side of the coin.

What are the basic assumptions behind this medical model of what is valid? One can be mesmerized by the seeming rigor and exactness. To look under the façade, there is much that is hidden. First of all is the notion that the world must be described in yes-no, right-wrong, logical terms. What is hidden is that such descriptions never fit an actual living world. Second is the notion that a verbal analysis and description can impart the information that you need to proceed. This works, of course for pills, surgical procedures, and exercise regimens. What is hidden are the instances when the procedure fails or simply doesn't fit the person's situation. Thirdly is the notion that any effect to be valid must result from an actual physical agent, thus the notion that a placebo effect is something in the way and must be eliminated as a factor. There is an accompanying lack of curiosity about the placebo effect and how it may work or how human contact may be a factor in the results. Fourthly there is the notion that the important measure of anything is statistical significance. Thus the intense scrutiny of such issues as sampling, controls, randomization. Statistics can hide huge areas of ignorance of individual situations. Rather than increase the quality of information, statistics set up a betting situation. I can choose this procedure because 60% of the time in similar circumstances it "works." Remember that the Feldenkrais Method was developed (according to legend) when Feldenkrais chose not to bet on knee surgery. Lastly is the assumption that the authority of the process confers safety and validity for the procedures. Note that it is the national medical groups that make recommendations for treatment. The person using such authority then does not need to observe or think further in the situation. When there is failure, it is just a part of the probabilities.

In Feldenkrais we want to be precise about the individual, and of necessity we are fuzzy in our verbal descriptions. This is the opposite of the criteria that Jim Stephens describes. We do not work

with fixed protocols, even in ATM. We guide people to their own self-discovery and away from accepting any external authority. But the outcomes of this precision does not lead to predictability. Heinz von Foerster used the terms trivial and non-trivial machines to distinguish systems that have predictable relationships between input and output and those that have a feedback loop, which generates internal states. The behavior of such systems becomes unpredictable. Fuzziness is then a characteristic of living creatures and their interactions. Von Foerster often noted that human beings had a propensity to try to trivialize the non-trivial. In our Feldenkrais work we need to resist this desire for predictability (triviality). In fact our successes are dependent on staying in an open relation with those we work with.

There is a funny trade off between our verbal imprecision and actual working precision. It is a consequence of the nature of complex systems, which suggests that Feldenkrais is more accurate to our non-trivial life processes. I quote from the engineer Lofti Zadeh who was instrumental in developing the mathematics of fuzzy sets. "As the complexity of a system increases, our ability to make precise and significant statements about its behavior diminishes until a threshold is reached beyond which precision and significance (or relevance) become almost mutually exclusive characteristics." (As quoted in Bart Kosco, "Fuzzy Thinking," Harper-Collins, 1994.) The net consequence is that it is more than difficult to fit what we do into the classic statistical research model. That is not to say that we cannot do research. As I have pointed out before (see my talk "Is there a science of the Feldenkrais Magic," Report of the First European Feldenkrais Conference, Heidelberg, 1995, IFF Publication 100) our work crosses the boundary between what is phenomenological, i.e. experience, and the realm of external observation. What we need is to develop another way of doing science, one that accounts for the way complex interconnected systems behave, and accounts for the relevance of experiential data.

In most of current science and medicine Zadeh's analysis is pure heresy. It puts into question the whole foundation of the research game, which depends upon the mathematics of probability. There are other ways of doing research and operating successfully in the world. Bart Kosco in his book (ibid.) describes how fuzzy logic makes for much more precise control in designed systems such as the focusing mechanism of video cameras. The real shame is how often scientifically valid procedures (i.e. correct based on the so called probabilities) lead to human disaster for an individual. Feldenkrais used the idea of fuzzy before it was named as such, and he heaped scorn on bivalent, cause-effect thinking as well as statistical medicine. Statistical medicine fuzzes up the question of why a procedure works for one individual and not another and hides the ignorance behind the statistical analysis. So what can we do in this situation?

First and foremost, I believe, we need to work primarily with individual instances. Neurological researchers do it all the time, and accumulate information that can apply to many nervous systems. Secondly it seems to me we have to become clear about what we want from research. If it is just to validate the method, I think we cannot succeed. That forces us to attempts at randomized controlled

trials. Our method is not a pill; it is not one clearly definable thing. It depends on the qualities and skills of the person presenting the ATM or FI as well as the persons participating and how they are willing to enter the process. Research can be and should be about what we do not know. It can be about new discoveries about functional interconnections that we observe in our working with individuals or teaching ATM.

Let me give some examples. In the same issue of *In Touch* (third quarter, 1999) Russell Delman in his column writes of his experience in working with fixing and opening the attention and how this relates to experience of pain. He makes some astute observations. Here is a whole area for experiment and research. In some experiments I have done in my classes, it has been demonstrated that an open attention changes the observational ability of students, and more interestingly it changes the quality of touching another person as well as changing something in the feeling of the person touched. Research in an area such as this could contribute to the general body of knowledge.

Here is another example. Some years ago I taught an ATM class for a large corporation for people with back pain. This class was combined with a back care program taught by a PT using the ATM work as a basis. We did a survey that compared personal results with prior treatments the participants received. The results were encouraging, but the survey was not rigorously constructed. Some of the individual stories, however, were fascinating. One man with years of pain became pain free after one particular ATM lesson that helped him recover movement in his pelvis, back and hips. That lesson was for him. But how was that possible? In my years of practice I have found that by feeling the change of quality of movement of say the spine during a lesson, I can predict if a person will experience much less pain after the lesson. Here is an important research topic. What is changing for the person and how is it that some people who seem disorganized in their structure and movement may never complain of pain in their entire life?

There are other phenomena to research. For example, I have observed with a number of people a correlation of some missing parts of the visual field of imagining the hand in moving with body-image disturbances and disturbances in the vestibular system and balance. This could be something really important to explore and could be a discovery of something not known.

The results of our work are not easy to document, and vary from person to person. Primarily, at first, when results are dramatic, they are phenomenological, i.e. they involve a big change in a person's experience. Changes in posture, movement, breathing, behavior are much more subtle and can be seen clearly more easily by a trained observer. What shows up on computerized movement analysis equipment is small and barely detectable in machine terms. Tests such as range of movement are often too crude a measure to find the significant change. And we do not know what we might see with brain imaging techniques. Experience also shows that sustaining changes happen over a long period of practice with the method, not just with a few lessons. This is even more the case with neurological difficulties.

To really look at our work we need to document a lot and over a long time period, to develop the quality of our observing and describing, to create notes, make photographic and video documentation. We also need to continue our own development and learning, and discover in ourselves what makes for real quality and success in what we do. We need to go to the laboratory situation with a clearness of what we are investigating and what our claims are. I have been negative toward the classic model of research. Nevertheless I do support a research effort. Let us begin again with more sophistication and in our own terms.

Reading Suggestions:

Fuzzy Thinking, Bart Kosco. Harper-Collins.

Hare Brain Tortoise Mind: Why Intelligence Increases When You Think Less, Guy Claxton. Fourth Estate (London).

Molecules of Emotions, Candace Pert. Scribner.

Understanding Systems, Heinz von Foerster, and Bernhard Poerksen. Kluwer Academic/Plenum Press.