Toward a Theory of Motivational Interviewing

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Much of my career I've run afoul of colleagues for being too atheoretical - a pragmatic dustbowl empiricist. When pressed I can usually come up with a reasonable theoretical rationale, but the truth is that I usually start from curiosity and experience, and from a general interest in finding what works best for people in pain. With enough experience, I start coming around to theory development.

Thus I have devoted perhaps too little time and attention to developing the theoretical underpinnings of motivational interviewing. As you know, MI did not evolve from a theory. It was drawn out of me. In a style much like that which I would be writing about, my Bergen colleagues had me demonstrate what my clients had taught me, and then helped me to unpack the unspoken assumptions and decision rules behind the method so that it could be communicated to others. As I wrote the resulting descriptive article that was published in 1983, I tried to ground the method to behavioral science findings and constructs, as well as a few metaphors. (This was long before Jeff Allison's elegant dancing-versus-wrestling.) I drew on Festinger's concept of cognitive dissonance, which eventually gave way to the broader and less baggage-encumbered concept of discrepancy. I used what Hal Arkowitz had taught me about Daryl Bem's self-perception theory. There was a natural fit with the then-new transtheoretical model of change, and with health belief models. I even threw in a wacky electrical wiring diagram that with a little tweaking could now grow up to be a structural equation model, complete with mediating and suppressor variables.

Since then I have been busy doing, developing, testing, and teaching MI, and haven't contributed any further steps toward a theory to understand it. I may not even be the right person to wrest (or dance) a theory from the data. I am getting curious, though. We seem to have a method that works with surprising consistency across problem areas, contexts, therapists, and cultures. Two decades ago it would have been hard to convince me that a single session of anything could reliably trigger a change in stubborn addictive behaviors. Yet it seems to happen - not always, of course, but with enough statistical reliability to be replicated by
numerous investigators in reasonably small samples, with effect sizes averaging somewhere around six tenths of a standard deviation. We seem to be able to show other therapists how to practice MI, so that they can replicate its effectiveness. We're even training trainers!

Yet with all of that we do not have, in my view, a satisfactory explanation of why and how motivational interviewing works. I've taken to asking MINTies how they explain it. Perhaps that's the nominal group brainstorm topic with which we will start off MINT-7 this year. If we stick to our style, what we will do is eventually elicit the theory from each other, and I hope that MINT-7 will be just such an opportunity. As a small step forward, I offer the following findings, which I think, need to be accounted for and incorporated into any theory of motivational interviewing.

1. MI seems to work. Behavior patterns that have been stuck for some time seem to get unstuck. How/where was behavior "stuck" before MI (this is where Steve and I have speculated about ambivalence), and what unsticks it?

2. It works in relatively small doses. There are numerous demonstrations of single session interventions (not all of them explicitly MI) being a reliable catalyst for change. Whatever it is that happens, it doesn't take much.

3. The effect is relatively large. It's enough to produce large effect sizes from a single session added to an outpatient program or a 21-day inpatient program. In Project MATCH, MET held its own against two 12-session outpatient treatment methods. Whatever is happening in MI, it seems to be enough to produce change.

4. The efficacy of MI seems to be enhanced by (or at least is most evident in the presence of) negativity. If anything, its relative advantage is with less ready, less motivated people. In MATCH, it worked better with angry people. Client attributes often regarded to be markers of poor prognosis seem to be less serious obstacles with MI.

5. It seems to work by reducing negativity. In Miller, Benefield and Tonigan (1993), we found that what predicted change was not a high level of clients saying the right thing (though that did happen with MI), but rather a low level of client resistance. If the therapist behaved in a way that did not elicit resistance, change followed. It is also noteworthy that client resistance was a relatively low frequency behavior; small numbers of occurrences predicted a lack of change.

6. If Paul Amrhein's reported psycholinguistic findings in MIDAS hold up to replication, self-motivational statements (SMS) do make a difference. What he is finding, and what may have eluded us before, is that it is not the absolute level of SMS that predicts outcome, but rather the slope of commitment language during an MI session. If commitment language (what we call SMS) is going up over the course of the session, the client is likely to show behavior change. If the slope is flat or negative, the client is unlikely to change.
7. Therapists differ in their efficacy using MI. Even under intensive training and monitoring conditions in Project MATCH, designed to minimize therapist differences, therapist effects on outcome persisted after removing variance accounted for by sites, treatments, and client characteristics. MET was the one condition where we could not account for such differences by eliminating outlier therapists.

8. Accurate empathy, defined as reflective listening, seems to be a strong predictor of therapist efficacy. I am enthusiastic that in MISC we have a research tool with finer resolution, which may let us get closer to identifying process determinants of change. Straying further from the data, I would add these intuitive observations.

9. There is something about this Menschenbild, the underlying positive assumptions about human nature, the living-as-if seeing of possibilities in the other. This may be harder to measure, but I believe that the efficacy of MI has something to do with communicating - even taking for granted - hope, profound respect, esteem, possibilities, faith in the person, freedom to change. "Other-efficacy," perhaps.

10. There is something about self-esteem. The literature doesn't show up self-esteem, as usually measured, to be a strong determinant of outcomes, and perhaps it's because it involves interactions. In my original wiring diagram, self-esteem has the potential to drain off motivational juice at the point where both discrepancy (importance) and efficacy (confidence) are present. If I am doing myself in with my behavior, and there is something I could do about it, I still might not take action if I think I'm not worth saving. Self-esteem in itself doesn't seem to drive change; it may even do the opposite in some circumstances. Yet I think there are conditions under which it is the missing ingredient. Lacking self-esteem, our clients borrow our esteem for them.

11. There is something about acceptance. The paradox that Rogers highlighted is that when one feels unacceptable in one's present discrepant state, one cannot change. When one feels accepted or acceptable, then it becomes possible to change. Against the reflexes of the heart, the motivational interviewer does not insist or even believe that a client must change. I also agree with Rogers that this is a reciprocal process - not that the client accepts the therapist (although I think it happens, and that Monty Roberts is onto something here) - but that one's ability to extend such acceptance to others is related to and enhanced (or limited) by the extent to which one experiences that same forgiving acceptance of self. The good news is that practicing one seems to enhance the other. The very act of listening reflectively to another also changes the listener.

12. There is something about love. In America it is out of fashion for psychologists and researchers to talk about love. We also mix up its multiple meanings. I have found particularly helpful a little C.S. Lewis book called The Four Loves in which he distinguishes among four ancient Greek nouns, all of which are rendered in English as "love." One (eros) is erotic, sexual love. One (storge) is attraction love, like my own love for chocolate. One (philia) is familial, close-bond loving. All three of these are things that therapists are not supposed to do with their clients. Then
there is agape, a kind of selfless, other-directed, encompassing but nonpossessive love, likened to God's love. Its sole interest is in the well-being and growth of the other. There is a mystical sense of oneness with the other, as though at least for this moment we were not separate beings.

Twelve. That's a good number, a good place to stop for now. You take it from here.