Why Appreciative Inquiry is Unlikely to Generate Research Publications

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What is the role of Appreciative Inquiry in research? Has it fulfilled the definition proposed by Cooperrider and Srivastva in 1987? In what ways has AI been used in research studies in the last twenty years? Is it possible for AI to become a main-stream academic research methodology. Is it desirable?

Consultants and managers engaged with Appreciative Inquiry (AI) tend not to appreciate how AI was initially described as an alternative research method. In Cooperrider and Srivastva's original article on AI (1987) they state 'Our effort here is but one in a small yet growing attempt to generate new perspectives on the conduct of organizational research, perspectives that can yield the kind of knowledge necessary for both understanding and transforming complex social-organizational system… As a holistic form of inquiry, it asks a series of questions not found in either a logical-positivist conception of science or a strictly pragmatic, problem-solving mode of action-research. Yet… its aims are both scientific (in a sociorationalist sense) and pragmatic (in a social-innovation sense)…'

This was a bold and provocative statement, and one that got many academically oriented organization development (OD) people excited. In the 90s it was popular in North America for graduate theses in organization development to use AI as their research methodology. The popularity, however, seems to have waned. I've been around close to a dozen graduate theses related to AI and they all fall into two categories: studying AI as a change process or using the output of the Discovery Phase of AI for a qualitative, grounded theory-building type of study. The latter would be the closest I've seen AI used as a research method and the closest to Cooperrider & Srivastva's vision of a science that values generative theory. For example, one of my master's students did a thesis on how executives learn leadership. She participated in an AI process intended to change the training and development of leaders in a telecommunications company in which the top 45 executives were interviewed about their peak experiences of leadership and how they learned to be leaders. She mined their stories for ideas, compared them to the research and professional literature on the topic and articulated 3 ways leaders learn leadership that were not found in the literature.

It's instructive that while over 100 AI related theses exist in North America alone, only one, to my knowledge, has been published in a peer-reviewed journal (Jones, 1998). I am not aware of any blind reviewed publication using an Appreciative Inquiry methodology that studied some substantive content. The closest would be Reed et al's 2002 study of patients leaving hospitals. Using a variant of the AI process described by Bushe (1995) they engaged members of a variety of agencies in studying positive experiences of leaving hospitals. While the study lists 'provocative propositions' that emerged from the AI, the article is mainly about how AI might be used to bring multiple agencies together to collectively study and

change something. Yoder (2005) published a study of emotional intelligence and leadership that claimed to have used an AI as its methodology, but in practice merely used AI-style interviews and then content analyzed them. It does not appear the study was interested in organizational change. How 'peak' stories are used and how this supports change is an area of AI practice that has not been well explored or described. I have argued that in an appreciative inquiry concerned with organizational change, simple content analysis of interviews does not lead to much change and is probably not how the stories ought to be used (Bushe, 1995). Ten years later I am more convinced of this than ever (Bushe, in press).

The lack of published research using AI as a method should not be surprising. Appreciative Inquiry is rooted in a rejection of the positivist standards and basic assumptions most research journals espouse. It is based on Gergen's socio-rationalism, which declares that the aim of science should not be about finding social laws allowing for prediction and control. He argues that instead, we should aim to create a social science focused on its 'generative capacity'. Gergen defines this as the 'capacity to challenge the guiding assumptions of the culture, to raise fundamental questions regarding contemporary social life, to foster reconsideration of that which is 'taken for granted' and thereby furnish new alternatives for social actions' (1978, p.1346).

When Cooperrider and Srivastva argue for refashioning a social science of vital significance to society, they say that 'To do this, we need a bold shift in attention…'. I would put emphasis on the word bold. In arguing that theory and research should no longer be judged in terms of predictive capacity, but instead judged in terms of how much it presents provocative new possibilities for social action, they are striking at the deepest, and in many cases unconscious, fears of the academic mind. Moore and Gillette (1993) demonstrate that the 'shadow side' of the archetype that governs the life of scholarship, what they call the 'magician archetype', distrusts action of any kind and particularly distrusts calls for action based on ideological grounds or claims that something is better than something else.

On the other hand, Moore and Gillette show that at its most developed, the magician archetype is concerned with using its knowledge for healing. That does leave some room for optimism that academic conceptions of research might evolve in the direction Gergen, Cooperrider and Srivastva envisaged. I find Gergen's call for emphasizing the generative capacity of science as stimulating and attractive a vision today as I did 30 years ago but little has changed in the world of theory and research. One thing research is, is the process of evolving the shared understandings of people through inquiry and imagination. All academic research is aimed at fellow academics and only they can judge its contribution to their understanding. Appreciative Inquiry is mostly aimed at members of a social system, and only they can judge its contribution to their shared understanding. Because these different audiences require different forms of inquiry, I don't believe that outcomes of appreciative inquiries that are, first and foremost, organizational change efforts, will ever be treated as research findings by the academic community.

That doesn't mean that some of the underlying theory and practice of AI couldn't be used for research purposes. Cowling (2001, 2004a) has done the best work on conceptualizing, for other academics, how Appreciative Inquiry might be understood as a research method, but his specific application, unitary Appreciative Inquiry, is not interested in changing social

systems nor does it focus people on the best of their experience. Instead, he seeks to uncover the wholeness, uniqueness and essence of human existence. He has published one peer-reviewed study on despair using this technique (Cowling, 2004b) but I doubt most people involved in AI would recognize his methodology as an appreciative inquiry. If an appreciative inquiry is not concerned with changing the system in which it is employed, I wouldn't call it AI. If AI does influence research, I think we are more likely to see researchers adopt an 'appreciative stance' to more academically grounded methodologies like narrative research (e.g. Schall et al, 2004) than to actually engage a system in Appreciative Inquiry.

Does that mean AI is not a practical or useful research method? On balance I'm inclined to say it's not, though there is at least one provocative possibility, embedded in the idea that AI can be used to study the social innovation potential of a social system (Cooperrider & Srivastva, 1987) – what has more recently come to be called the 'positive change core' of an organization. This evokes an image of researching 'what could be' – not something one normally associates with the word research but still close enough to possibly gain legitimacy. I have, however, not seen this kind of study presented or written up. If AI ever comes to be accepted as a research method by the research community, I'd expect it to be only the Discovery phase of AI that gets used as some kind of grounded theory-building process, much in the way Cowling (2004b) and Schall et al (2004) describe.

I want to conclude by stating that it may be better not to think of AI as a research method. Because the early work in AI described it as a new form of action research, too many people assume AI is action research with positive questions. My early work (Bushe, 1995) was guilty of this. Even contemporary OD textbooks describe it that way (e.g. Cummings & Worley, 2005). They don't recognize that AI is based on an entirely different paradigm than action research (Bushe & Marshak, 2007). My research and experience indicate that when AI is used within a conventional action research structure, change processes and outcomes similar to conventional OD-style action research occur. When people use AI this way what tends to happen is that they treat Discovery like data gathering and only a few people actually do the data gathering. Then Dream is used like problem formulation and Design like solution gathering and these too are usually done with small groups of consultants and organizational members. Finally, Destiny is run like implementation. This can produce some improvement but in contemporary organizations, where such processes are already well absorbed into their cultures, it cannot produce transformational or cultural change (Bushe, 2007).

Thinking of AI as a research method probably helps to keep this image of 'AI as action research with a positive question' alive. The kind of AI that can amplify and accelerate culture change (Bushe & Kassam, 2005; Ludema et al, 2003; Whitney & Trosten-Bloom, 2003), however, has little resemblance, in practice, to traditional action research and even less to what academics would consider research. Instead, it invites members of a system to shift their mental maps and the prevailing discourse(s) in their system through a kind of inquiry that has no interest in validity, reliability and generalizabilty – the kinds of things science values. Instead, in an effective AI the inquiry results in statements (provocative propositions) that are only generative in the system in which they are constructed, and their generative potential has as much or more to do with the level of engagement by all system members,

and the quality of dialogue evoked, as with the actual 'findings' and provocative propositions themselves.

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