

From demography to cognitive measurements. A personal journey

Lívia Markóczy
Anderson Graduate School of Management
University of California, Riverside

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When trying to understand people's actions in an organization or trying to fathom what lay behind strategic decisions we want to know what is going on in people's minds. Not everybody makes the same decisions and not everyone acts the same way in similar circumstances. So we want to have, at least a guess of, some idea of what is going on in an individual's mind when making a decision.

There was a time when I accepted the argument that was presented by Hambrick and Mason (1984) that demographic characteristics are acceptable proxies to capture differences in the thinking of individuals. Looking at individual characteristics, such as age, sex, level of education, country etc seemed like faulty but plausible and reasonable proxies for what kinds of values, beliefs attitudes, etc people had. After all, since the minds of individuals are shaped by their experiences and since these in turn are reflected in demographic characteristics, those external characteristics appeared to be acceptable proxies for individual cognition.

My views have changed for a variety of reasons from the acceptance of demography through the use of cognitive mapping techniques to their current position that well designed questionnaires and experiments of the sort used by cognitive psychologists are the best way of getting at individual cognition. By the end of this essay, I will describe very briefly some of my current projects and ask the reader the rhetorical question: "Could the cognitive depths I am getting at through my current methods be even approximated by demography (or even by cognitive mapping)?"

1 In the Beginning

When I accepted the demographic approach in the early 1990s, there was a great deal of literature justifying this sort of stereotyping as long as it was done in a statistically responsible way instead of making strong predictions about a single individual based on the stereotypes (e.g., Hitt and Tyler, 1991; Wiersema and Bantel, 1992). What also made the fact that this was stereotyping less obvious was that the actual contents of the stereotype were never visible, only the claim that people with similar backgrounds thought the same way.

While even Hambrick and Mason (1984) and Michel and Hambrick (1992) acknowledged that demographic characteristics are rather rough proxies for cognition, they suggested that the field should accept these as surrogates for cognition instead of trying to capture cognition directly, which is “unobservable, [and] impractical to measure directly” (Michel and Hambrick, 1992, p. 16). Rather than going to the trouble of capturing cognition, they advocated the advantages of demography, such as objectivity, parsimony and possible replication” (Michel and Hambrick, 1992, p. 16). Another clear advantage of demography – though less talked about – is that data on demographic characteristics are available archivally. That enabled researchers to conduct statistically rigorous, large scale and longitudinal studies.

Whatever the motives behind accepting individual characteristics as proxies for cognition, one thing needs to be acknowledged: Researchers have found connections between top management team demographics and organization behavior or results, although we still do not know the underlying mechanism for this relationship. So, whatever criticisms are made here in this symposium of the demographic approach, a question must be addressed to the critics: “If demography is as bad as its critics say it is, how come it works?” I don’t have a solid answer to that question (although I could speculate about the direction of causality being “wrong” in some studies), so the particular criticism I make in this paper remains incomplete.

At any rate, to go back to my original point, I was taken by the idea of demography at first. I did have a terminological quibble, however. I always thought that the word “demography” is used for describing populations or collectives, not individuals. I preferred to speak of “individual characteristics” instead of “demography” and in what I say and write here, I will tend to use the two interchangeably.

2 From demography to causal mapping

My question, back in 1991 for my thesis (Markóczy, 1995a), was how much does a particular individual characteristic – nationality – matter in comparison to other individual characteristics? I went into five companies in Hungary that had recently been acquired by western companies. Using a specially designed intensive causal mapping method, I tried to work out who thought like whom in these organizations. Did the Americans tend to view the strategic issues the same way as each other; did the women, bilinguals, or engineers?

What I found is reported in a series of papers (Markóczy, 1995b, 1996a,b, 1997, 2000b; Markóczy and Goldberg, 1998) But what is relevant here are two findings:

1. The relationship between individual characteristics and what people believed about relevant issues was extremely weak.
2. Far stronger than individual characteristics was whether people belonged to the functional area that was being favored by major change and restructuring.

Had I only found weak relations, one could assume that the problem was with the causal mapping method, but the method consistently found strong relations to membership of favored area. In 1997 I published this in an article titled “Measuring beliefs: Accept no substitute” (Markóczy, 1997). I suppose that it is because of what I argued in that paper that I have been invited to join this symposium.

3 From causal mapping to experiments and questionnaires

Both critics and admirers of the causal mapping method that was used in the study (Markóczy and Goldberg, 1995), agreed in one point. Namely, that the causal mapping that was used was convoluted, difficult, tedious and resource intensive. And methods for statistical inferencing that it required were even more convoluted, difficult, tedious and resource intensive.¹ Thus

¹Yes, even the statistical inferencing was resource intensive. Some statistical tests for a recent paper using these methods had to run over the course of a few days.

if faced with the choice of using individual characteristics as a substitute for cognition or using something like the causal mapping I used, I have a great deal of sympathy for those who would choose demography.

But as I was working with the convoluted, difficult and tedious method I'd help develop, I became more conscious of the implications of things that I'd said while developing it: *The meaning of a causal map is a function of how it is elicited*. I was also reminded of a quote from Axelrod (1976)

The cognitive mapping approach is, of course, in no better (or worse) position in this regard than any other procedure that relies on a person's conscious and monitored linguistic behavior to make inferences about his beliefs. [p. 252]

This was combined with the comments of one member of my thesis committee who kept on saying one way or another that if I was going to take 90 minutes of each subjects' time, couldn't get something more usable.

Two other things needed to come together before I arrived at my current position. One was a rereading of much of the demographic literature and taking a closer look at what individual characteristics were intended as a substitute for. These things were often described with expressions like "cognitive base," "knowledge maps" or "mental maps." But on closer examination they really didn't need to be any deeper than beliefs about what was most important for the success of the organization or beliefs about what causes what. Although the terminology used suggested that the researchers were interested in very deep, subtle and inaccessible features of cognition, the actual fact of the matter was that remarkably superficial and accessible things were being sought.

This lead me to half of my current position: For most things that the demographer and cognitive mappers have actually wanted (once we no longer allow ourselves to be misled by the terminology), could be better achieved with a simple questionnaire. There is no need to use convoluted methods when a questionnaire will get as deep, and more to the point of this symposium *there is no need to use individual characteristics as a proxy for what people believe when you can just ask them in a questionnaire*.

The second thing that has lead me to the other half of my current position came from a wider reading of the cognitive psychology literature. When cognitive psychologists want to get at subtle and inaccessible things about how people's minds work what do they do? They do not engage in cognitive mapping as known by management scholars, *and cognitive scientists certainly*

don't use individual characteristics as proxies for subtle cognitive features. Instead they perform experiments.

4 Experiments and questionnaires: Four projects

I am currently engaged in four projects. Each project involves trying to get at how people think or make certain decisions while these particular aspects of thinking may not be immediately available to introspection. I will provide a very very brief discussion of each and an even briefer mention of what methods I'm using to get at the ways of thinking in question. And finally I will offer up a challenge.

4.1 Project 1: Twelve motives for (non)cooperation

I am arguing in this project (Markóczy, 2000a) that there are a wide variety of different motives for cooperation or non-cooperation in social dilemmas, not just the handful of things like “cooperativeness” or being raised in a “collectivist” society. From my reading of the literature and my own work I have identified twelve candidates. Examples are things like “mass participationists” who wish to be a part of some big movement, in contrast with “elite participationists”, who want to help get something moving, but lose interest if the collective effort takes off. Other examples are concerns that it would be unfair for others to contribute while you don't, and there is the flip side of that that it would be unfair for you to contribute if others don't. Other motives, such as fear of being a sucker, come from the experimental economics literature. All in all, I've collected twelve of these and reviewed the literature on each. My belief is that different people are more susceptible to some of these motives than other motives.

To try get a clearer handle on these, I am trying to develop and validate a questionnaire which detects how much each motive plays a role in people's thinking. The general way the questionnaire works is that people are presented with a plausible real-life social dilemma and are asked to think about it carefully. Then they are presented with a series of questions asking them how much certain sorts of “thinking” came to mind when they were puzzling over the problem. The certain sorts of thinking include phrases like: “well, if

I don't do it, why should I expect others to," "my contribution would just be a drop in the bucket" and so on.

At this point I'm only going for a subset of my 12 motives and have a tentative questionnaire with three to four items per tested motive. It is still at preliminary stages, but certain patterns are emerging from trials, strongly suggesting that at least some of the motives I am after can be captured with this type of questionnaire.

4.2 Project 2: Everyday Kantianism

One of my on-again/off-again projects Goldberg and Markóczy (1997) is a detailed exploration of one of the motives for cooperation in a social dilemma. It is some times called "the voters' illusion" (Quattrone and Tversky, 1984) or sometimes "everyday Kantianism" Elster (1989). It is the reasoning behind "well, If I don't cooperate, it is likely that likeminded people won't cooperate either, so if I want them to I should cooperate." That reasoning is based on a causal fallacy. When you have a choice and your choice is correlated with someone else's choice, you can get a false causation. The phenomenon is subtle and not readily available to introspection. Indeed, my experience is that those who are subject to it most strongly are also most blind to it.

I am getting at this through a series of experiments that attempt to manipulate things so that the false causality is either accentuated or suppressed.

4.3 Project 3: Cognitive laziness and reference groups

Long research, particularly in marketing, shows that people's cognitive laziness (or bounded rationality) will lead them to be highly influenced by what others decide in similar situations (Simon, 1990). In work that I am doing with Sha Yang we are looking at modeling the extent to which individuals' judgments are influenced by other people's judgments, including the judgment of the majority, the judgment of those who are demographically similar to the individual in question and the judgment of those who share the motives of the given individual. We also look at individual differences in receptiveness to social influence, and differentiate between information and normative influence of others on the individual's decision.

For capturing these, we are conducting experiments that are based on

subtle manipulation of information given to subjects and then careful analysis of their revealed preferences.

4.4 Project 4: Meaning of money and zero-sum thinking

In a number of places over the years (e.g., Markóczy, 1998), I have speculated that in some societies, particularly those with no history of a functioning market economy, people view all money transactions as zero-sum games. As such they believe that if they are to come out ahead, someone else must lose. Thinking that way means that they cannot distinguish between legitimate (if risky) investments and pyramid schemes.

People can see that there are differences in preferences while trading goods, so they can easily see barter as a potentially win-win. But with money for money transactions (such as financial investments, loans, saving) the nature of money obscures the fact that there can be different preferences at different times, and so people see the transactions as zero-sum.

For this, I am trying to develop some tools to get at zero-sum or win-win thinking about different sorts of transaction. This is extremely preliminary at this writing.

5 The challenge

Each of my four projects described involve getting at aspects of things that people might call “cognitive base” or “mental model.” Indeed, what I am after in these projects appears to be even deeper than what people have used those phrases for.

While there are certain to be many design problems with the approaches that I am taking to the problems, I would ask whether using demography would be a reasonable alternative to the approaches of questionnaires or experiments.

5.1 Is my challenge fair?

Is it fair for me to say that just because demography is absurdly inappropriate for the four projects I’ve listed that it is a generally inappropriate method? After all, there are plenty of perfectly legitimate methods which

are inappropriate for the projects I've described. I think that the challenge and critique is fair just as long as demography justifies itself by saying that the individual characteristics used by demography serve as a proxy for aspects of a person's cognitive base. The claim that individual characteristics serve as a useful and meaningful proxy is absurd on the face of it. And, as I have tried to show, it is absurd not just on the face of it, but also absurd upon closer examination. Even if it served as some proxy (which I doubt), it is certainly not a useful proxy as there are direct and practical methods (questionnaires, etc) to get at cognitive bases more reliably.

6 Is demography useful at all?

If we remove the pretense that demography is a proxy for cognitive base, is demography still useful? I have no answer for that question, but I hope that some views will emerge from the symposium discussion. It must be noted that work on demography "works" in that it helps support theoretically motivated hypothesis. Demography cannot be ruled out until those results are explained in some other way. But I hope that enough doubts have been raised that it would not be someone's first choice of method. And that the reason given for why it works by the demographers seems implausible.

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