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Learning the Craft of Organizational Research

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This essay *proposes that scholarly research is a craft and that significant research outcomes are associated with the mastery of craft elements in the research process. A tentative framework of the research craft is proposed, which includes error and surprise, storytelling, research poetry, nonlinear decision making, common sense, firsthand knowledge, and research colleagues.*

What research techniques can be used to obtain significant new knowledge about organizations? Many of us would answer by referring to what has become known as the natural science model of research (Behling, 1980; Popper, 1964). In organization textbooks (Behling, 1980) the natural science model typically is associated with good research and is exemplified by precise definition, objective data collection, systematic procedures, and replicable findings. A milestone in the use of systematic procedures in organization studies was Campbell and Stanley's (1963) work on experimental design. The natural science model is sometimes called quantitative research (Morgan & Smircich, 1980). This approach assumes that social reality is a concrete, measurable phenomenon. Advocates of this approach stress the importance of reliability, validity, and accurate measurement before research outcomes can contribute to knowledge.

Others of us would answer that significant new knowledge about organizations is the result of qualitative procedures. Qualitative research is concerned with the meaning rather than the measurement of organizational phenomena. Qualitative research techniques were highlighted in a special issue of the *Administrative Science Quarterly* (Van Maanen, 1979). Organizations are assumed to be enormously complex social systems that cannot be studied effectively with the same techniques that are used to study physical or biological systems (Daft & Wiginton, 1979; Pondy & Mitroff, 1979). Qualitative research procedures assume that organization realities are not concrete, but are the projection of human imagination (Morgan & Smircich, 1980). Those who prefer qualitative research techniques argue that direct involvement in organizations and the use of human senses to interpret organization phenomena are necessary for discovering new knowledge.

A few of us would suggest yet a third answer. This answer would not make the distinction between natural science and qualitative research techniques as separate avenues to significant research outcomes. Organizations are complex, multidimensional entities. A range of techniques can be adopted to pursue effectively a range of research topics (Daft, 1980). Indeed, qualitative and quantitative approaches can be used side by side, as in the natural sciences. The qualitative method of "direct observation" (Mintzberg, 1979) is similar to watching cell matter under an electron microscope or sending Voyager II out for a first hand look at Saturn. The qualitative notion of "organizational stimulation" (Salancik, 1979) is similar to feeding large doses of artificial sweeteners to mice or treating cell cultures with chemicals to observe the response. Perhaps at a superficial level, research in the natural and social sciences seems to call for different approaches. But in many ways research in these fields is similar. In his address to the American Psychological Association, Oppenheimer (1956) proposed that we are all in this together, facing similar problems, suffering the same human limitations, trying to probe into the apparent randomness of a vastly complicated physical and social world to see patterns and make sense of it.

What techniques can be used to obtain significant new knowledge about organizations? Those who do not answer in quantitative or qualitative terms would argue that significant new knowledge is the outcome of something deeper. Research involves basic attitudes and ways of thinking. Research is a craft. Like other crafts, activities are not analyzable (Perrow, 1967). Cause-effect relationships are not clear. Unexpected problems appear. Procedures are not available to describe each aspect of research activity. The learning of craft skills may take years of trial and error. Through practice one learns how to ask research questions, how to conduct research projects, and what to strive for when writing a research paper. Significant research, then, is the outcome of a way of thinking that can be called craftsmanship.

The dilemma for the field of organization studies is that the technical, methodological aspects of the research process are taught to aspiring scholars in graduate school. A professor once told several students, "You will need at least 5 years to outgrow the effect of your dissertations." At the time the students could not appreciate the meaning behind what the professor said. They were captivated by the power of newly discovered research methods. Elegant and sophisticated techniques went into the design of dissertations. What was there to outgrow?

What many of us discover after graduate school is that research techniques taught in graduate school are not enough. Only the formal side of the research process can be transmitted effectively through textbooks and the classroom. One cannot learn to perform significant research by following a textbook anymore than one can learn to be a good writer by studying the rules of grammar. In one sense, significant research requires new learning beyond what is learned in graduate school. As a craft, research is interesting, exciting and satisfying. The challenge for researchers is to get beyond sheer techniques, whether quantitative or qualitative, and to interject the craft attitude into the research process. The purpose of this paper is to explore more fully those elements that make up the craft part of the research process.

The Research Craft

Sketched below are seven elements that form a tentative framework of research craftsmanship. Each element is briefly explained and contrasted with the formal, prescriptive approach to research that is frequently taught in graduate school.

Build in Plenty of Room for Error and Surprise

Training for the study of organizations, as most scholars experience it, reflects a rather traditional approach to scientific analysis. One learns about scientific rigor, experimental control, planning, and the anticipation and removal of uncertainties that could upset the research blueprint. The research challenge is to plan the work so that it comes out as predicted.

The problem, of course, is that this approach assumes that investigators know a substantial amount about the phenomenon under investigation. Knowledge beforehand makes for clean, tidy, hypothesis testing research, but the knowledge return typically will be small. If we have a good idea about what the research answer will be, if we understand the phenomenon well enough to predict and control what happens, why bother to ask the question? If we are to acquire knowledge that is really new, then we do not know the answer in advance. The significant discoveries, the good science, require us to go beyond the safe certainty of precision in design.

Lewis Thomas (1974) said that good basic research needs a high degree of uncertainty at the outset, otherwise the investigator has not chosen an important problem. One should start with incomplete facts, with ambiguity, and plan experiments on the basis of probability, even bare hunch, rather than certainty. Then look for surprise. Quality of work is measured by intensity of surprise. The greater the astonishment, the greater the knew knowledge about the world.

Those of us in organizational behavior and theory often seem to have it backward. Books on research design, courses on research methodology, and comments from journal referees lead me to believe that many investigators desire absence of surprise in their research. Hard logic and previous evidence should justify every step. A journal referee once insisted, "You can't use that hypothesis because there is no previous evidence to support it." A successful project is believed to be one in which everything comes out as predicted.

The myth that successful research comes out as predicted, probably more than anything else, restricts the discovery of knowledge in our discipline. Reviews of landmark studies in the behavioral and organizational sciences indicated that they tended to be loosely done (Daft & Wiginton, 1979; MacKensie & House, 1978). The significant studies often approached the problem as an open-ended question to be answered rather than as an hypothesis to be tested (Lundberg, 1976).

The notion of building uncertainty into research has been a big discovery for me. It is okay to ask research questions without the answer in advance. In one sense, all scientific progress is due to errors and deviations. New knowledge is a surprise; it changes how we see things. If experiments are perfectly designed and the results come out as expected, then they probably are a waste of time. We must take chances, we must make mistakes, to be good scholars.

Research Is Storytelling

Graduate school teaches that research procedures include designing a project, collecting data, counting things up, looking for relationships, testing hypotheses, and reporting the findings in a journal article. These steps certainly are necessary in an empirical science.

The craft side of research is not like this at all. Research is storytelling. The scientific method is more like guess work, the making up and revising of stories. Storytelling means explaining what the data mean, using data to describe how organizations work. Stories are theories. Theory need not be formal or complex. Theories simply explain why. The "why" is important, and researchers should be creative and ruthless in pursuit of it (Weick, 1974). The why, not the data, is the contribution to knowledge.

Data collection and analysis are integral parts of the research process, but they are intermediate points between an initial hunch and the final story about the organizational world. Data do not stand alone. So many papers miss this essential point of research. Data are treated like so many playing cards to be shuffled, reshuffled, and dealt around. Research often is viewed as if it is naming the game and calculating the probability of each hand. Emphasis on method and calculation misses what the data represent. Human behavior and processes in organization are what we care about. The data alone are not enough, no matter how sophisticated the techniques for collection and analysis.

Geologists, for example, are storytellers (McPhee, 1981). They take observations from outcroppings, roadcuts, tunnels, maps, and drillings. These are geological datapoints, which are collected and analyzed rigorously. But geologists do not report only the data. They use the data to construct wonderful stories about geological history. They describe the appearance of lakes and oceans, the wearing down of mountains, and the ecological systems of animals and plants that inhabited the earth. These stories provide insight and understanding about the earth's history. Geologists make up stories and continue to revise and elaborate the stories with subsequent research projects. In much the same way, craftsmen in organization research use data to tell stories about the behavior and processes within organizations.

Design Research as a Poem, Not as a Novel

The logic of research, as I learned it, was to reach out for more variables whenever possible. Multivariate analysis was one key to success, and still seems to be. Most review papers recommend that future studies incorporate additional variables as the path to uncovering true relationships and greater understanding. Journal referees enjoy pointing out how operationalization of additional variables would make a study better, perhaps even publishable. To the extent that variables represent characters in a story, then the approach often recommended would result in a novel, with many characters, a complex plot and almost infinite relationships.

I no longer accept this approach. Poetry seems to have greater applicability to organizational research. Poetry means a research design that includes only a few, perhaps two, three, or four variables. But they must hang together in a meaning unit, a coherent framework of sorts, that explains some aspect of organizations. A research poem also must have depth. The meaning unit must take a deep slice into organizations and convey a rich conceptualization to others.

These two ingredients—a few variables that form a coherent whole and depth of meaning—constitute an ideal research framework. Most of the significant ideas in our field are poems. Theory X and Theory Y is a poem. So is the notion of differentiation and integration. For me, Perrow's dimensions of task analyzability and variety constitute a poem. *Organizations in Action* (Thompson, 1974) is a book of poetry. Thompson (1967) expressed several important models in simple two-variable contingency tables.

The thread common to all of these concepts is simplicity in the sense of only a couple of key variables, but the ideas hang together in a unit to explain some dimension of organization. The ideas have depth. Differentiation and integration summarize a cluster of behaviors that may be found in organizations. The concepts have layers of meaning that enable one to understand a complex notion in a single thought, much like a metaphor. The concepts have roots that run deep into organizations.

Human organizations are enormously complex, so how is it possible to understand them with simple models? Two reasons. First, good research doesn't try to answer all questions about an issue. It doesn't pretend to. The best research provides an utterly imperfect model of organization reality. One goal of research is simply to understand a tiny piece of organizational reality. The insights provided by a simple model can be used to raise new questions for future research. Second, Simon (1981) argued that one does not have to measure system complexity to model it. Everything in organizations may be related to everything else (Boulding, 1956; Pondy & Mitroff, 1979), but a model of two or three key variables can still be accurate. The model provides a basis for a deeper story. A hundred variables may be involved, but substantial insights about organization relationships can be uncovered from an assessment of a few key dimensions.

Writing poetry in organizational research is extremely difficult. Successful poems can be the result of genius or of chance. This does not imply that every study should be limited to a small number of variables, only that we should not expect a large number of variables to produce great insights. We can strive for simplicity in our research. Simple means fundamental, not trivial. The addition of variables should not substitute for careful thinking about organizations or for searching out key dimensions.

Research Decisions Are Not Linear

If any activity should be characterized by rational, logical decision processes, certainly it would be empirical research. The rational model begins with a carefully formulated research problem based on a thorough literature review. Next, the research design and methods are chosen. Some sort of triangulation may be possible. Data are collected and analyzed, and the results are used to support or confirm specific hypotheses.

The craft decision process is much more random and messy. After evaluating research decision processes, Martin (1982) proposed that the garbage can model serves as a better description of a random, chancy process. Campbell, Daft, and Hulin (1982), based on a retrospective interviews with prominent organizational scholars, discovered that most significant research did not follow the rational model. The original decision to undertake a project resulted from the simultaneous convergence of several events, such as the discovery of a new research technique, the availability of a research site, and the appearance of a new idea. The investigator spontaneously grabbed the opportunity. The Campbell et al. interviews also found that when research decisions were rational and linear, the research findings tended to be less significant. Research undertaken as a logical next step tended to produce outcomes that were routine and dull.

Another side to the decision making process concerns intuition and feelings. If the rational research model can be characterized as left-brain activity, then many research decisions are made in the nonlinear area of the right brain. The Campbell et al. (1982) study found that investigators cared about their research. They felt passion for their studies. Investigators couldn't explain it, but the significant studies felt good from the beginning. Mitroff (1972) reported that research objectivity among the scientists he studied was a myth. Scientists are not free of bias, opinions, or convictions. They care deeply about their work and have a stake in the outcome (Watson, 1968).

Another nonlinear attribute by which a research project can be judged is beauty. Kaplan proposed that esthetic quality is one way of validating a theory. "A scientist sometimes needs the courage, not only of his convictions, but also of his esthetic sensibilities" (1964, p. 319). Mintzberg (1982) wrote that if an idea is not beautiful, then perhaps it will not be useful either. The decision to undertake a study is based on a symmetrical hanging togetherness that pleases the beholder. The right brain has an important role in the craft side of the research process. Significant research is not a logical next step, is not the outcome of a strategic plan, is not calculable. The best time to undertake a research project is when the investigator suddenly realizes, "What a lovely idea!"

Relate Ideas to Common Sense

We have all heard or used the argument that research ideas are on the frontier of organizational knowledge. The concepts may not make sense to those not involved with the research, especially managers. The evaluation of research findings must be objective. One learns to distrust gut reactions and other indicators of common sense. As scientists we expect to seek a higher proof.

Perhaps there is some truth to this idea because the common sense of laymen and scientists may differ (Davis, 1971). On the other hand, I have gradually come to realize that common sense—of both the investigator and his colleagues—is the best test, the ultimate test, of our theories. I am beginning to understand what Oppenheimer meant when he said, "Science is the adaptation of common sense" (1958, p. 129). Scientists simply look for aspects of experience not visible in daily life by using instruments such as telescopes or questionnaires. Oppenheimer went on to say, "We come from common sense, we work for a long time, and we give back to common sense refined, original and strange notions that enrich what we know. We come to new things in science with what we already know" (1958, p. 129). C. Wright Mills (1955) found it essential to integrate what he was doing intellectually with what he was doing as a person. To trust one's own experience, Mills said, is the mark of a mature scholar. In a sense, one cannot deal with new scholarly findings except on the basis of the familiar and old fashioned.

One way to embrace common sense is to use analogy and metaphor in scientific descriptions. Huff (1980) writes that metaphor makes the strange familiar and it allows recognition and learning that links an idea to previous experience. Metaphor and analogy provide a vehicle for relating new ideas to what is already known. Without this linkage the new idea has little value, little impact, and provides no means to elaborate on previous experience.

Other fields, especially the natural sciences, make use of analogies (Dreistadt, 1968). Analogies are not perfect representations in any sense, but they provide a basis to make the new familiar. Oppenheimer (1956) argued that one cannot be surprised at a discovery unless one has a view of how it ought to be. A recent paper argued that biological metaphors of organizations are inadequate (Keeley, 1980). Of course they are inadequate, all analogies are. But biological and other types of analogies still help in communicating the essence of new ideas.

The final point about common sense concerns the notion of proof. Ultimate proof of an idea or theory is its acceptability to common sense. An important test of validity is liking an idea, feeling right about it, being able to use it to throw light on a previously hidden aspect of organization. Objective proof seldom will exist somewhere outside one's self that will demonstrate correctiveness or validity. No statistical test will do this for us; no amount of replication will make acceptable an idea that does not square with experience. Even if the organization reality studied is hard and objective, we are not. We cannot obtain knowledge independent of our own judgment and social construction. (Morgan & Smircich, 1980).

The notion of differentiation and integration in organizations is an example of concepts that are useful to experience although they are not provable in objective fashion (Lawrence & Lorsch, 1967). The scientific measurement of these concepts has been challenged (Tosi, Aldag, & Storey, 1973), yet the ideas continue to flourish because they are useful and acceptable to students and managers of organizations. These ideas make sense at a deeper, nonstatistical level.

Learn About Organizations Firsthand

This idea seems so obvious, but it is not stressed in many Ph.D. programs. Organizations are so rich that anyone who actually observes them, who goes out for a look around, will find sufficient puzzlements to last for a productive career. For some reason, direct contact with organizations, firsthand learning, is not given high value. Collecting data is stressed, and so are running correlations and reporting statistical coefficients. As a reviewer of papers, it becomes painfully clear that many authors have never seen or witnessed the phenomenona about which they write. Authors cannot give an example to illustrate a point. They have an enormously difficult time thinking beneath the correlation coefficients to discuss what the coefficients represent in terms of organizational activities and processes. Authors typically report very thin descriptions of a large number of relationships, never touching the why of the correlations, dealing only with the fact that variable *Y* is related to variable *Z*, as if that constituted everything.

The difficulty that many authors have developing interesting and insightful theories about organization probably is explained by the lack of experience with organizations. G. R. Grice admonished his students who were trying to understand animal learning: "No matter how much research money you may have, or how many assistants you may hire, always handle your own rat" (Hackman, 1982). If those of us in organization studies would handle our own rats, the supply of important research problems and new theoretical insights could be quickly increased.

Organization studies is an empirical science. Mintzberg's strategy of direct research on managers (1973) and decision making (Mintzberg, Raisinghani, & Theoret, 1976) illustrates how powerful first hand knowledge can be. If we look, really look, at our subject of study, we cannot help but see things that will inform us about organizations. Staying in one's office and mailing out questionnaires may have the appearance of research, but often it reduces the opportunity to learn about organizations.

One of the unexpected discoveries from interviews with leading scholars by Campbell et al. (1982) was the importance of real world contacts. Significant studies often began through direct contact with organizations—perhaps a training session with managers, a consulting job, or a puzzlement encounter during field interviews. On the other hand, studies that turned out to be less significant were not originated in organizations. These studies originated in a more academic fashion, from one's university office, perhaps based on a journal article and the perceived opportunity to make a small modification that would yield a quick publication.

Armchair theorizing and other forms of noncontact with the organization also can be helpful, especially if they probe into organizational ideas in a speculative way and provide a fresh perspective to guide empirical research. But even armchair theories have to be informed by contact with organizations somewhere along the way. Contact either in the form of visits and observations or perhaps through descriptive case analyses provides the intellectual raw material for useful theory.

Many Colleagues in Our Discipline Really Care about Quality Research and New Knowledge

The need to publish papers becomes apparent to most of us during graduate training. Many, many people in our field seem preoccupied with the idea of publication. They do whatever is necessary to have a paper published. They will send it to any conference or seminar or journal to get publication credit. In the worst cases, people cut up their data or trade authorships to increase the number of publications listed on their résumé.

So much career progress is based on publication that attention gets distracted from the content of our papers. In a publication environment, failure to publish means failure in an academic career. Hence a large proportion of us are seduced into this process without realizing that there is another game to be played. There are fewer players at the other table, but a serious research game is being played out right now in organization and management theory. There are many colleagues who count the content of a paper first and publication second. Among these scholars, an unpublished working paper will have impact if it adds to the developing knowledge base. A working paper can influence the thinking and research of others. Formal publication is anticlimactic. Individuals can be known by their ideas, not by the number of publications.

I cannot specify the boundaries of this research orientation or identify very precisely the players, only that the game is played, and that I have experienced the thrill of sitting at the table. The concern for content is a welcome haven from the publication wars, and far more productive. The machine gun fire of referee criticism is replaced by positive words of encouragement and support. The bombshells of journal rejection are replaced by collegial advice, intellectual exchange, and a desire to get to the truth. Interchanges with senior scholars that did not have publication as the ultimate goal had a profound impact on my intellectual development. Publication is a fact of life for all of us. We all feel the pressure. But there is tremendous support within our discipline for high quality empirical and theoretical work. We do not have to do mindless research if we choose not to, and publication will take care of itself.

Conclusion

What research techniques can be used to achieve significant new knowledge about organizations? The answer proposed here is that formal research techniques—quantitative and/or qualitative—as taught in graduate school are not sufficient. Significant research grows out of experience and mastery of the attitude and frame of mind that make up the research craft. The research craft is enhanced by respect for error and surprise, storytelling, research poetry, emotion, common sense, firsthand learning, and research colleagues.

The elements of the research craft described above are neither fixed nor complete. Every scholar can add characteristics that help lead to significant outcomes in his/her own research. Scholars can progress through their own stages of learning and develop their own guidelines. The important thing is that the craft perspective be mastered and used to build upon the techniques of science taught in graduate school.

What troubles me is that many of us seem never to have discovered or acknowledged the craft aspects of scholarship. Formal techniques and method dominate in most manuscripts and journal articles that I read. The authors act as if there is only a single approach, which includes measurement precision, perfect prediction, dispassionate analysis, and many variables. Authors often eschew real organizations, storytelling and common sense.

How can we facilitate the learning of research methods to include craft characteristics? We can convey to our students that research is a craft as well as an exercise in methodology. Formal techniques are easy to teach in the classroom, but the craft attitude and way of thinking are learned through experience. Students can be told that there is an uncertain, emotional, human side of research, and research that incorporates these properties can be science at its best. Even more important, we can experiment with these elements in our own research and show them to students firsthand. A great scholar such as Kurt Lewin used apprenticeship to pass the research craft to his students (Marrow, 1969). By showing students how to design studies on the basis of anticipated surprise, beauty, firsthand experience, emotion, and storytelling, we can be role models for the kinds of things that go into significant research. We can ask students to learn formal research techniques in class, and then invite them to join us in the research adventure.

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