

A Muckraker Battles Alienation in Organization Studies: Introducing Culbert's "Transorganizational Muckraking: Method and Style"

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Abstract

For years Samuel Culbert has been providing penetrating analysis of how modern organizations are mismanaged, and the dysfunctional assumptions managers make. Presented are two papers describing his phenomenological investigatory approach. The first places his methodology in a broad intellectual framework, and describes its avoidance of social science strictures. The second is an autobiographical account of the experiential and intellectual paths that led to Culbert's choice of methodology, and the reasoning that guides his use of it. It presents a detailed account of how he collects data, analyzes it, goes about validating his conclusions, and trying out remedies suggested by the theoretical frameworks he constructs.

Keywords

alienation, applied behavioral science inquiry, clinical skills

I was pleased to be invited to introduce Samuel Culbert's "Transorganizational Muckraking: Method and Style" (from here on, I will refer to the article simply as "Muckraking"). It is a wonderful read and provides intriguing understanding of how the mind of one of the most insightful and outside the box-living students of

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organizations works. Sam has long been one of my favorite organizational scholars. Over the years, his work has provided my students and me with meaningful and exciting insights into organizational and human dynamics. I have long admired and even envied his ability to understand people and organizations—he sees so much that I and many of my colleagues often miss. In the following article, he helps us understand what enables him to do the research he does. An underlying theme I discovered from reading his article helped me see the unity in his work I have missed before—he has been fighting major sources of alienation in organization studies.

In this introductory essay, I attempt to analyze his work as objectively as a longtime admirer can do. I try to describe what I find exciting in his work and what are some things about it that likely limit its academic impact. “Muckraking” helped me understand his work in new ways.

“Muckraking” reveals that his life’s work has in large part been directed at overcoming what he saw as two deficits in organizational inquiry: (a) its failure to uncover the organizational processes that prevent human beings from reaching their potential and (b) misdirection of social science methodologies. I see both of these concerns directed at overcoming alienation. Please note use of the word “alienation” stems from my interpretation. I do not recall Culbert using the term in any of his major work.

When I use the word “alienation,” I mean it in the sense Marx (1908) seemed to have in mind when he spoke of the Fetishism of commodities. When human beings are influenced by this fetish, rather than exercising conscious control in their lives, humans are being controlled by their own creations. In the remainder of this introduction, I will use “alienation” to reveal an underlying thread of “Muckraking.”

Alienated Organization Processes

Culbert’s major concern is that the nature of the organizations human beings have created leads to humans being controlled in ways that interfere with them reaching their potentials. This concern is evident not only in “Muckraking,” but throughout his work. In the second paragraph of “Muckraking,” he writes that the major focus of his research has been identifying organizationally constructed obstacles to people doing their best work and to related errant assumptions about human nature that are associated with these obstacles.

These concerns were grounded in the work of major scholars including Paulo Freire’s belief in the importance of raising consciousness of sources of cultural oppression. Interestingly here and in most of his other work that I am familiar with, Culbert does not use the academic term “alienation.” In fact, he seldom refers to academic work and seems to avoid the use of academic jargon.

I suspect that Culbert’s decision to avoid academic jargon has had mixed consequences. Probably, many public scholars have had similar experiences. On the downside, it has meant many doctoral students may not have been introduced to his work. On the positive side, it has meant his work has reached thoughtful practicing managers and students, such as many of those I have instructed in business schools. These people became aware of the alienating effects of modern organizations, without use of the word. Moreover, they learn how to make things better.

In “Muckraking,” Culbert describes a second alienating aspect of organization studies that his work has been directed against. This one—alienated research methods in social science—is less obvious in most of his work than the alienating practices of organizations that I discussed above.

Alienated Social Science Methods

Culbert, guided by an epistemology that differs from that of many other students of organizations, was doing “action-research” before social scientists used the term. He relied heavily on his training as a clinical psychologist to collect and interpret his data. Such methods are not trusted by many mainstream social scientists who in their quest to show they are practicing science, doubt the ability of clinical methods and action-research to produce “objective” information. In my view, their fixation with mythological purism reminds me of a concern of the great psychologist Sigmund Koch (1959). Koch observed that psychology had long been an unusual science in that “. . . institutionalization preceded its content and its methods preceded its problems” (p. 783). In “Muckraking,” Culbert begins with a similar concern, introducing his methodology by pointing to the importance of an axiom that liberates inquiry from the institutionalized control. The axiom is that nature of the phenomenon of interest should determine the researcher’s methodology.

In short, Culbert’s concern with alienation advances our methods. However, his choice to employ clinical methodology and to present his research using limited academic jargon have had mixed consequences. Relative to the work of other academics with his talents, it has made his work more accessible to applied audiences while distancing it from some of his academic peers. This approach reflects the type of scholar Culbert is—he has questions he wants answered. He does what is necessary to get them answered and to be sure his answers are correct, even if it includes developing trusting, cooperative relations with the people who are providing his data. This allows him to check out how well he understands those he is studying. He controls the method, not the reverse.

Note how different this approach is from the commonly used one employed in laboratory experiments where the inquiry relies on deceiving the subject. While I expect Culbert believes that for some questions, we can learn much from appropriately constructed experiments, he would insist that we do not let the method control us. In other words that we should not let some preferred method become so dominant that we ignore potentially significant insights because the way we gained them does not conform to some standard humans created. I suggest that research controlled by such standards is appropriately labeled alienated. If we follow Culbert’s axiom and his more clinically driven approach, such alienation is less likely. (Although of course, it is possible that the norms of clinical inquiry could also become controlling.) Culbert’s description of this method in “Muckraking” reveals some of its merits and limits. The advantages are rather clear but the limits are less so.

In my view, the major limits are how much difficulty it takes to use this method. Whereas most anyone can administer a large scale survey, the skills that Culbert draws on from his clinical training are not so easily acquired. In fact, they are difficult to

describe. As you will see in “Muckraking,” his method is not the type of thing we usually find in textbooks—rather, it is the product of a person who studied broadly and developed himself as an observer prepared to employ diverse perspectives to answer questions that interested him. In this context, it is useful to recall physics Nobel Laureate Steven Weinberg’s (1993) observations about methodology of physicists.

Weinberg (1993) observed there are different types of physicists—he described two: magicians and sages. Each works in a different way and serves different functions. “The sage-physicist reasons in an orderly way about physical problems on the basis of fundamental ideas about the way nature ought to be” (p. 67). Einstein, in founding the general theory of relativity, acted as a sage. In contrast, many theoretical physicists play roles of magicians. The magician-physicist does not appear to be reasoning at all but sometimes appears to jump over intermediate steps and jump to new insights about nature.

In contrast, Weinberg (1993) described Heisenberg’s early work on quantum theory as the work of a magician. The way a magician’s work is difficult to understand let alone describe. Consequently, “. . . authors of physics textbooks are usually compelled to redo the work of magicians so that they seem like sages” (p. 68). It is well to keep the distinction between magicians and sages in mind when considering Culbert’s discussion of his methodology. Although it would be wrong to suggest he is not reasoning, his clinical approach is difficult for textbook writers to capture and at least occasionally leaves one with the impression of mental leaps. For example, while his move described in “Muckraker” from Berger and Luckman’s (1967) ideas to rejecting the face value of people’s utterances makes sense in retrospect especially given the developments in how we view human behavior since Berger and Luckman wrote. At the time Culbert was developing his approach and establishing straight-talk relationships with research participants, it appears to represent a major cognitive leap. In such respects, Culbert reminds me of a magician.

Consequently, as with Weinberg’s magicians, it is not surprising that Culbert’s methodology is not ordinarily found in textbooks. Moreover, teaching it either in textbooks or in the classroom will not be an easy task. Thus, despite the insights it has yielded, it is not surprising that this approach is not more widely used in the field. Moreover, since as he describes in “Muckraking,” it originated from serious questioning of established methods and from an epistemology that challenged what appears to underlie much of modern social science, replacing them with an emphasis on establishing straight-talk relationships. The central role this approach gives to the personal behavior of the researcher adds a flavor of “subjectivity” that alarms many social scientists.

However, Culbert’s success with it in exposing hidden features of organization processes speaks to its potential value in using his method instead of conventional methods to answer the questions one has. This is why I described it as a step against methodological alienation.

Conclusion

Many contemporary scholars seek to think outside the box. I propose that doing so effectively is aided by an ability to free oneself from being controlled by ideas and initiatives human beings have introduced—that is, overcoming alienation. “Muckraking” reveals

the cost and benefits of Culbert's pursuit. It provides insight into Culbert's accomplishments and some of the possible costs and benefits of his approach. The autobiographical content reveals it is not easy to learn to become able to do what he does. Among other things, it may demand a program involving study of materials well outside one's current discipline and limited approval from one's disciplinary colleagues.

With these thoughts in mind, I recommend a serious study of "Muckraking," I suggest complementing this study with reading a few chapters from several of Culbert's path-breaking books, for example, *Mindset Management* and *The Organizational Trap*.

After this study, I urge reflection on the state of organization studies and its prevailing epistemology trying to maintain a "beginners mind" perspective asking whether it may be time to consider the value of more "magician" like methods of the sort Culbert has molded.

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Transorganizational Muckraking: Method and Style

Samuel A. Culbert²

Evolving Appropriate Methodology

Arriving at a Fork in the Road, One Has Little Choice but to Take It

Every thoughtful researcher embraces a version of the methodological axiom that the nature of phenomena being investigated should determine the researcher's methodology—not the other way around. Correspondingly, most researchers readily acknowledge that using a set methodology, regardless of what one is investigating, irrespective of circumstances and situational constraints, greatly increases the likelihood of Type I error. I emphasize this axiom (and corollary) because I cannot think of any field of inquiry where it applies more than identifying organizationally constructed obstacles to people doing their

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best work and to related errant assumptions about human nature associated with such obstacles. It also applies to studying the dynamics produced when people who feel constrained from performing their best, out of frustration, act competitively with the very teammates the company is counting on them to assist. Interconnected, these two issues have been my research domain for 40-plus years.

I began performing research when, as a PhD student studying clinical psychology, I was assigned to learn T-group facilitation skills in a state-of-the-art group dynamics research facility. Immediately, I was fascinated by the therapeutic and life-changing experiences *normal* people could have in short-duration T-groups. In need of extra income, I took an additional assignment assisting in, and eventually leading, a series of hypothesis-testing studies investigating the validity of Rogerian theory stipulated conditions that promote self-awareness and interpersonal effectiveness gains. Group discussions were recorded, raters worked separately in multiple one-way observation rooms, interpersonal-perception surveys were seriously attended to by subjects, and experimental conditions were expert monitored to insure they took place as prescribed. The result was a number of co- and single-authored data-based published studies.

Shortly after completing my PhD, my clinical orientation and research focus shifted. Instead of working with groups of previously unrelated people, I found myself drawn to facilitating the effectiveness of people who were teaming up with one another on projects in the workplace. The world of work became my “laboratory,” and the people I studied were both my informants and coinvestigators. Of course, the companies retaining me did so out of a belief that team building and authentic communication exchange in relationships would positively affect productivity with gains at the bottom line.

Not at all at odds with, but different than my clients expected, I found my intellectual focus was on how, what I was helping individuals, work groups, and companies accomplish, applied to people in work settings more generally. I often found myself performing a mental partial differentiation, stripping away individual and company specifics to focus on what managers erroneously believed, regardless of company and situation, that led to management effectiveness problems existing transorganizationally.

I thought about why the bright managers with whom I worked were not themselves able to see what they were doing ineffectively and mend their ways on their own. Had in-the-moment expedience, and needs for self-advancement, overwhelmed their common sense? Had they been misled by a mainstream culture idea that you get discipline, order, and accountability through control instead of building bonds of trust? Increasingly, I saw the importance of exposing dysfunctional mainstream practices and getting managers to see the human nature misassumptions on which they were proceeding.

I also reflected on what I was doing that allowed me to see what was off and what might be done to remedy it. Certainly, my skill sets gave me an edge. I used my clinical and group process skills to understand the personal relevance of situations managers encountered, and to recognize the basis of the unique and individual ways they pursued work and personal agendas. But when it came to thinking about phenomena that drove groups of people to think and act as they normatively did, it was the systems

analysis and inductive reasoning skills, acquired in undergraduate engineering studies, that I relied on the most. I became aware of why I no longer wanted to conduct single-variable hypothesis testing. Temperamentally, I was much more drawn to analyzing systems-level influences.

Research-wise, I made the transition. I was now in the mid-level theory mode attracted to issues related to unleashing human capacities. This change was greatly facilitated by two paradigm-shifting sets of forces. The first was involvement in, and my instantaneous identification with, an emerging activist discipline, Applied Behavioral Science—where action-research was the investigative methodology of choice, and humanism the explicit value set. My orientation was further expanded, and deepened, by exposure to the social interventionist writings of Paolo Freire (1970), and association with researchers in the Quality of Working Life movement.

The second paradigm-shifting force was exposure to 1960's new-wave sociologists arguing the importance of naturalistic observation, and offering methodologies for interpreting the meaning underlying what people said and how they acted. Their insights provided the perspective and logic I needed for dealing with a research dilemma I had been struggling with—how to deploy my clinical insight and acumen without corrupting data.

A clinician conducting research, I have honed skills in observing people and analyzing for intentionality that are somewhere between difficult and impossible for me to turn off. I am constantly scrutinizing the data I collect and making attributions about its meaning. I am attentive to nuanced expressions and feelings, observant of body language, I naturally connect what's transpiring to what previously happened, I question the reasons for people expressing themselves conventionally, and, when I think the situation will tolerate my doing so, am inclined to question and even challenge people to explain the grounds and the motivations for what they conclude. I always want to know what's really going on in a person's head.

I discovered that getting a deep understanding of what I'm investigating entails more than what informants think or are inclined to say. So, I'm ever alert for discrepancies: between what an individual seems to be experiencing and what that person says; between what an individual says he or she will do and actions taken; between what actually drives an individual's thinking and the justifications the person provides.

Wanting to use all my skills, I feared that interpreting and assigning meaning to what people told me would be viewed as data contamination, tarnishing the scientific credibility of what I discovered. Wary that taking what people say at face value leads to inaccurate conclusions, I was stuck in conceptual no-man's-land unsure just how to proceed.

Sociologists Helped With Methodological Reasoning

Reading sociologists Peter Berger and Thomas Luckmann's (1967) *The Social Construction of Reality* reinforced my belief that failure to probe the meaning of people's words and actions would be an investigative mistake. I found their thesis providing the methodological guidance I needed to do what I believed epistemologically

correct. Using a sociological framework, their thesis built from a human nature fact that made immediate sense to me when I encountered it training as a clinician. Paralleling how cognitive psychologists differentiated their thinking from the behaviorists preceding them, I heard Berger and Luckmann asserting that events do not have unitary meaning revealed to people thinking “objectively” enough to comprehend what is truly happening (my words). In contrast, they argued that people bring meaning to events, and thus, everything a person reports is colored by personal history and motivations in the moment—and I would add workplace agendas embraced. In other words, any individual’s pronouncement about what “objectively” happened is nothing more than a specific individual attempting to get you to believe that what they are alleging is factual, exists outside of themselves, free of distortion, and without spin for personal gain—which, of course, is never so.

Berger and Luckmann’s ideas addressed another of my research concerns, this one bearing on how laboratory generated data can mislead. Performing laboratory research, I was aware of data validity problems due to subjects falsely claiming their phenomenological accounts were “objective” and, more importantly, the investigator knowing this was false but lacking the means for correcting. I was further troubled by inferences researchers made connecting informant accounts in anthropomorphized situations to real-time situations at work. In other words, I believed people react differently to life events than to laboratory induced hypotheticals. I also believed the same behavior holds different meaning for different people, that people often use the same words to communicate different meanings, and take the same actions for very different reasons.

I began to ponder: If no behavior stands on its own, or speaks for itself, then taking people’s actions and utterances at face value is methodologically incorrect. This line of thinking reinforced something else clinical experience taught me: to put a good deal of energy into *getting* each subject sufficiently well to be confident that I am correctly interpreting what they say and, when imputing meaning to behavior that is different from what an informant reports, to present that person with my interpretation, and give good consideration to their response.

This collection of concerns led to my recognizing the importance of establishing straight-talk relationships prior to questioning subjects. I needed informants feeling sufficiently comfortable, accepted, and self-reflectively contemplative to speak openly and honestly with me. I also realized the importance of keeping in perspective what people with limited self-awareness report since informants cannot relate more than they are able to candidly tell themselves, and often feel obligated to maintain company image and personal face.

Berger and Luckmann supported another axiom that I internalized during clinical training: External events are experienced uniquely by each individual. This led me to expect people in the same company, with parallel assignments and the same mandated objectives, will think differently and not to count on them to give even simply stated work-based communications the same interpretation. Along these lines, I believe everyone who has completed the leadership course I have been teaching for 25 years, however long ago, and even those who long ago forgot that my first name is Murray[*sic*], can pass an exam question I always give, “What’s the difference between ‘the founders reality,’ ‘the dominant reality’ and ‘the individuals’ reality?’” The answer: the

“founders reality” is how things were originally supposed to be and needs to be repeated when explaining the company’s mission and purpose to outsiders; the “dominant reality” is how company insiders have, through implicit and explicit processes, agreed to talk about issues and refer to events when justifying their actions; and the “individuals reality” is what an individual actually sees, believes, and inwardly knows when feeling sufficiently secure to speak honestly to one’s self. The takeaway: Accessing what an individual truly believes and actually drives his or her actions requires a relationship in which that individual feels sufficiently free from judgment to be open with the interviewer.

The theory-building approach described by Glaser and Straus (1968) explicitly distinguishes between hypothesis-testing and hypothesis-generating research, and provides important methodological guidance about how to formulate theory. Arguing the appropriateness of addressing big picture issues using an investigative, hypothesis-generating, theory-building approach, Glaser and Straus stipulate methodology for excluding idiosyncratic variance leading to Type II error mistakes. I found their “data saturation” ideas consistent with the investigatory protocols used in projective testing (e.g., Rorschach and Thematic Apperception Test) where essential knowledge is gained, with little data corruption introduced, by the interviewer first soliciting and recording answers to tabula rasa phrased questions, and then, subsequently, asking increasingly “leading the witness” questions to address suspected omissions and distortions.

Glaser and Straus also offered a method for getting beyond conclusions the researcher had drawn prior to conducting even one interview. They argued the benefits of researchers performing a front-end comprehensive articulation of all going in views and biases, “bracketing” their thinking, to provide interested parties a means to judge for themselves whether researcher beliefs materially affected the validity of conclusions drawn. In other words, Glaser and Straus saw bias-denied far more likely to corrupt a researcher’s findings than bias-acknowledged, and provided a method that freed researchers from the pretense of noninvolvement. Think about it, why would anyone conduct a study unless that person had some going-in beliefs about what the study might yield?

The Impact of Interventionist Scholars

My exposure to the political ideology and methodological practices of social activist, philosopher, and educator Paulo Freire was a great match with my systems, big-picture, see-a-problem-fix-it temperament and affected me in several ways. I have adopted Freire’s use of constructive muckraking in promoting institutional change. I have embedded a derivation of his consciousness-raising model in the algorithm I use in my formulation of theoretical frameworks; and, consistent with his validation model, I utilize praxis-generated data in offering managers and scholars a means for evaluating the validity of the conclusions I present.

Like Freire, I believe any system constructed by one constituency to govern, control, and organize the actions of other constituencies will inevitably spark resentment. Lacking voice and self-determination, it is inevitable that members of the underclass

feel oppressed. Lacking a viable means for extricating themselves from what they perceive as externally and unjustly imposed, effectiveness-limiting, and subordinating their interests, leads so affected constituents to object causing governing groups to initiate additional controls.

Freire believed that people with the power to impose constraints on others would never willingly relinquish that power. In other words, only the oppressed could free their oppressors from their compulsion to control. He saw the act of domination causing overclass insecurity, and because of this, he believed people in power lack the ability to see what they could gain from relinquishing their domination and living in an increasingly conscious and just system.

Paralleling Freire, my goal has been exposing and correcting misguided, effectiveness-limiting, organizationally imposed modes of operating that limit open-minded thinking, authentic expression, and teamwork in organizations. Accordingly, the remedies I propose always include venues for employees expressing their views openly and honestly—without intimidation or fear of retribution—on matters related to their well-being and the organization's effective functioning.

Kurt Lewin's (1951) publication *Field Theory* presented a straightforward analytic model to use in identifying the force-field elements that drive and restrain behavior. It's an easy to use conceptual scheme for conducting phenomenological investigations leading to institutional reform and change. Included in Lewin's article was a dictum that almost everyone who hears it retains: "If you want truly to understand something, try to change it!" In other words, Lewin saw perturbing the status quo, and then documenting what transpires, an illuminating way of conducting human nature research. Lewin's methodology was exemplified in activist experiments he devised during World War II aimed at getting American families to eat commonly shunned, but nutritious, animal parts. Thus, Lewin became the groundbreaker for a dynamic investigative methodology termed action-research and the inception of an academic discipline, Applied Behavioral Science, focused on humanistic, capacity-liberating, managerial practices, and the processes of "planned change"—organizational, institutional, and societal. A distinguishing feature of action-research is that the researcher does not just identify problems; the action-researcher's assignment also includes coming up with culturally appropriate solutions, learning from attempting to implement it, and then iterating the theory and prescribed application accordingly.

Action-Research as Practiced by Samuel Culbert

At the 2010 Academy of Management Meetings¹, I reflected back on the theoretical frameworks I had developed and how I go about validating what I surmise from the phenomenological data I collect. I saw my research process separable in six interconnected, and usually sequentially performed, activities:

1. Identify a transorganizational management practice that systemically blocks people from being and performing their best.

2. Deconstruct the practice to identify the erroneous assumptions and thinking about people and their work effectiveness, on which that practice is based.
3. Conduct a disciplined investigative approach using robust methods to access the phenomenological experiences of people who perpetrate that practice and think it correct, and of those whose actions and capacities are constrained by it; and access the knowledge and insight possessed by researchers and other parties with relevant experience and expertise.
4. Formulate a phenomenologically grounded, theoretical framework that explains what is wrong (missing or erroneously assumed) in current practices, and provides theoretical justification for conceiving an alternative that is both human potentiating and workplace effective.
5. Field test the alternative, assess its impact, and make theoretical framework revisions as deemed appropriate.
6. Publish findings and prescriptions, and publicize them for purposes of informing the academy, and effecting workplace reforms (muckraking). In short, the methods I use dovetail with my career-long goal: making the world of work more fit for human consumption.

Working to overcome blocks to individual and workgroup effectiveness, I am always thinking about the transorganizational manifestation of what has been managerially imposed locally. I say “managerially imposed” because I believe the core managerial function is staging for the effectiveness of others and that once a systemic obstacle to employee effectiveness is identified, I see getting that obstacle removed to be a fundamental managerial responsibility.

More Specifically, Here Is What I Do

My methodology utilizes a Glaser and Straus prescription: Investigators should avail themselves of every resource at their disposal and use all their thinking power and expertise. I believe *researcher objectivity* a false construct—especially at the beginning of an investigation where subjectivity is engrained in the researcher’s choice of topic and what he or she hypothesizes. Action-researchers always have outcome goals and inevitably choose investigatory tactics distinctly suited to their resources and proclivities. I diligently follow the Glaser and Straus prescription to bracket personal views and accumulated knowledge. All preliminary thoughts written down, to return to, which frees me to practice *tabula rasa* inquiry.

The decision to expose a particular mismanagement practice is one that emerges over time. By the time I decide on a topic, I have accumulated a sizeable folder with notes—written on post-its, napkins, take-out menus, scraps of papers, lecture notes, book margins, and anything else I happened to have to write on when something I think, or someone says, strikes me as investigatory relevant. Reviewing my notes for themes, I conduct a thought-searching “core dump.” I write down everything I know, trying to make declarative and increasingly succinct statements about issues and forces

bearing on the phenomenon I am out to understand, putting at least temporary labels on groupings of statements.

Initial thinking chronicled and organized to be continuously revised, I access topic-knowledgeable informants—people who by virtue of phenomenological immersion or topical expertise have acquired an informed vantage point for viewing what I am out to understand. At the beginning, I solicit their views without revealing mine, although some informants are, by virtue of previous conversations, familiar with my ideas and personal sentiments. With them, as with all others, I try to convey the impression that I am solely out to record their phenomenon-relevant experiences, and how they think and what they conclude based on them. Recognizing that trust is the key to the depth, candor, and comprehensiveness an informant reports, I try to conduct each interview in the context of a budding relationship. I avoid suggesting there is a right or wrong answer, and I often ask for visuals, and examples to test whether I am getting the meaning that underlies an informant's words. At all times, I try hard to communicate that I am out to factually record what this person actually thinks and has experienced, with no judgment being made about what that is.

Unlike a laboratory experiment where each subject's data are collected and await analysis until the subject pool is exhausted, I review, interpret, and integrate what is revealed in an inquiry as soon as I have time to fully consider it. While I usually go out of my way to avoid redundancy, for this part of my investigation, I do my best to patiently listen when people repeat what I have concluded, or state what I believe misguided. I gain some efficiency by being selective about who I interview, having a preference for people who have had unique experiences with the phenomenon, or who I find self-aware and topically insightful. When someone presents a viewpoint I have not previously heard, I am likely to present it as a counterpoint discussion topic to the next informant who relates something appreciably different. Not wanting to add error variance, I am inclined to exclude data from informants who have difficulty staying on the topic about which I am inquiring.

Gaining data coalescence, theoretical framework emerging, I spend less time inquiring about rudimentary phenomenon and more time inquiring about specific situations informants relate that appear to vividly illustrate what I am investigating. When I get to the point where I am not hearing anything different, or learning anything new, what Glaser and Straus call data saturation, I am likely to probe anything that I find at odds with what I am concluding, but only from informants I find subject matter knowledgeable.

As I get framework clarity, I transition how I speak to informants. I begin disclosing aspects of how I have the phenomenon formulated to test its persuasiveness in causing an informant to modify their views. If it does, I then ask questions aimed at ascertaining specifically what proved sufficiently compelling to cause that informant to change his or her mind.

Depending on an informant's temperament, and what seems contextually appropriate, I next change my demeanor from trying to understand to challenging views that differ with my emerging, iterated formulation. For instance, I might say something like: "I get a different meaning out of what you just related."

Then, referencing precepts of my in-process formulation, I provide an alternative interpretation explaining what theoretically derived insight led me to conclude as I did. If I sense an informant becoming defensive, I become supportive. For instance, I might say: "Of course in the end it's your experience and your way of seeing things that I want to hear so there's no need for you to agree."

When I believe an informant is holding back and think I have not heard everything an informant knows, I get more assertive. For instance, I might ask: "What needs to be added or subtracted from my formulation to get you to change your mind?" I liken what I am out to accomplish to solving for the first differential in calculus. In my mind, I am conducting implementation research, searching out what statement of incoherence, or what demystification, or what reconstruction of a commonly occurring phenomenon might interrupt an informant's attachment to what he or she formerly believed that I think the framework I have constructed supersedes.

When possible, I begin an implementation discussion referencing a phenomenon-laden situation the informant and I have experienced in one another's presence. Lacking such a reference, I may mention several company practices I believe the informant will recognize as reifying the phenomenon addressed in what I have formulated. Alternatively, I might bring up a situation exemplifying the phenomenon that I believe the informant finds personally troublesome. Wherever I begin, my goal is to bring up a situation that I see epitomizing the phenomenon and its dysfunction to find out if the informant believes that it does. Only after I am convinced an informant can visualize the phenomenon do I sketch out the transorganizational framework I have conceived. Then, after I have laid it out, I keep quiet. Of course, I will clarify and be appropriately interactive, but now my focus is on how the informant reacts to my reconstruction of a familiar situation that person has been viewing differently.

I precede roughly the same with informants with whom I have no previous experience, but only after first presenting them a phenomenon-steeped scenario. Then, I inquire whether they have experienced a situation with similar dynamics to make sure what they are referencing fits with what I want to hear them discuss. Only after determining the informant and I are on the same page do I invite the informant—now acting as coinvestigator—to describe their experiences and, where possible, how it illustrates the framework I have been inviting them to critique. In all instances, I am looking to minimize hypotheticals by asking informant-coinvestigators to vicariously put the framework I have formulated to a self-reflective experiential test.

Most action-research is organization specific with validity determined by enhancements in individual, project team, and organization-wide effectiveness. Usually three criteria are invoked (Argyris, 1974): (a) Were the action-research findings sufficient to reconcile the performance limiting issues prompting the investigation? (b) Is it reasonable to expect that problems solved will stay solved, that improvements made will endure? (c) Did the action-research activity, in and of itself, enhance the organization's capacity for identifying and coping with effectiveness-limiting issues that subsequently arise?

Determining validity is a bit different when action-research is performed transorganizationally. It is illogical to think there is a one-size-fits-most standard remedy or fix for people working under different conditions in different settings, in different industries, with different backgrounds, financial circumstances, and so on. To the contrary, transorganizationally stated, each application is expected to be different. Each replacement action will require local sponsors, culturally appropriate formatting, and participants adjusting the model in response to what they learn when trying it out.

The compass setting for action-research is always praxis advancement. Wary of travelling intellectual roads that fail the application test, I am constantly looking for an appropriately stationed manager willing to host a demonstration project that I can guide, learn from, and reference in facilitating an application somewhere else.

Research Location

As previously indicated, my “research laboratory” is the world of work with a “workshop” at the university. My reason for preferring naturalistic settings parallels bank robber Willie Sutton’s apocryphal explanation for robbing banks: “Because that’s where they keep the money.” The world of work is where the phenomena I study take place. It is where I can learn by conducting and reporting on real-time field experiments, trying out theoretically conceived “remedies” as field-site specifics direct.

Of course, not all populations are equally immersed in all phenomena and some work settings and personnel are more accessible than others. Notwithstanding the open-minded eclecticism I like, whenever possible, I attempt to access populations whose business activities and modes of operating exemplify people steeped in the phenomenon I am studying.

For example, when, at the time doctoral student and now Cal Poly SLO Professor Jean-Francois Coget (2004) wanted to investigate how leaders reconcile the dysfunctional dynamics they incite with emotional outbursts and/or their mistreatment of personnel, I asked J. F., “Can you think of a work setting or industry where people in leadership roles are known to scream and shout and emotionally lose it?” Top of the head, J. F. answered, “Film directors on location movie shoots” which I thought methodologically ideal. Whether or not his choice was “scientifically derived” is debatable. Most important was the fact that it was a place where he could engage the phenomenon he was out to study, and it was accessible due to his living in Los Angeles sharing an apartment with well-networked film director. Of course, he also needed a disciplined methodology. If you want a treat, I recommend you read his study. On the other hand, if he had conducted this conversation in Madison, Wisconsin instead of Los Angeles, he might have nominated members of the Wisconsin State Legislature, who at the time were engaged in vicious partisan fighting. Along these same lines, another appropriate phenomenon steeped set of informants would have been college football coaches under alumni pressure to field a winning team.

Sources of Data and Presentation Tactics

Always at the top of data sources informing my research are organization events I have personally witnessed where I have a nonthreatening venue for inquiring into what others experienced and the sense they made of it. I also draw on my own life experiences, not as data, but only as illustrations of the phenomenon I'm describing. Whatever I choose, my criterion is helping readers recollect a personal situation of their own to self-authenticate the validity of the theoretical framework I am presenting.

Oftentimes, there are world events and national news items that serve as recognizable examples of the phenomenon I am investigating. For example, when investigating barriers to authentic expression and employee dissembling in boss/subordinate relationships, I came across a video clip of then Secretary-of-State Donald Rumsfeld pretentiously stating: "The buck stops with me," as if to stand accountable for the Abu Ghraib fiasco. I followed that clip with a second one showing the operatives entering a stockade and then, 6 weeks later, a third clip of *Mr. Buck-Stopper* receiving the Medal of Freedom for his leadership in Iraq. Seeing it as an example of boss dominant, one-sided accountability, I showed it to my fully employed MBA class. Then, listening to student comments I realized a missing dimension in the in-process formulation of what I considered the desirable, good management alternative—two-sided, reciprocally accountable, nonhierarchical relationships. The addition added clarity to the entire framework I had formulated to the extent that the nearly everyone who reads the published model instantly gets the problem and essential point. This student's input, and others like it, lend support to my belief that people usually know a great deal more about human nature than they seem able to practice or even talk about until prodded.

Scientific journals and books play an important role in my research but only after I have an issue in focus. Long ago, I subscribed to Paolo Freire's pedagogical belief that lessons related to what is current and meaningful in one's life are far more impactful than knowledge "banked" to be withdrawn when an individual has the need. When I have got an issue in focus, I want to stand tall on the shoulders of what is already known. Of course, there are topics of personal interest that I continually follow. For instance, after Jack McDonough and I cowrote a chapter critical of performance reviews in our (Culbert & McDonough, 1980) book and then a take-to-task exposé (Culbert & McDonough, 1981), I began keeping an eye out for articles and books alleging reviews could be conducted "objectively." Subsequently, I included a chapter criticizing the mainstream practice of combining pay and performance review discussions in my Culbert, 1996 published book. I recall the literature providing helpful background reading for that chapter—it was vacuous which I interpreted as validation for my conclusion. On the other hand, performing a data dump after deciding to investigate issues relating to "speaking truthfully to power" routed me directly to the trappings of hierarchical relationships. I learned I had some catch-up to do and spent the next 2½ months reading up on this topic. What I learned added conviction to my original belief about the gains people and their organizations can realize from extracting hierarchy out of boss/employee relationships.

The second occasion was the search I performed on the benefits of performance reviews. That research clued me in to a glut of what I saw as bogus information—ready to disorient researchers. I found scores of books alleging needs served and benefits received and step-by-step advice for successfully conducting reviews. But I could not find a single credible argument or data-based study to substantiate that this mainstream practice accomplishes anything I considered positive. Moreover, by breaking performance reviews down into their social psychology studied component elements, I discovered that the research literature would predict their having the exact opposite effect.

Of course, every sensible person understands that even statistically substantiated results and high confidence levels can and will be readily discounted by people with personal motives to believe differently. Convincing to me was the Mitroff and Mason (1974) study of 42 scientists holding divergent beliefs about the origin and composition of the moon. Given moon rocks collected by Apollo Mission astronauts, NASA scientists expected theoretical convergence. What they received, however, was the opposite. Each recipient found additional justification for his or her distinctive theory. My takeaway: Even in the physical sciences, the researcher's going in beliefs affect scientifically substantiated conclusions. Unlike these physical scientists, the methods I use require remain conscious of my going in biases.

Working on transorganizational topics, I have found serendipity a reliable friend. For instance, my *Wall Street Journal* (Culbert, 2008) article criticizing performance reviews produced over 1,200 unsolicited letters to the editor that were forwarded to me as they were received. Recognizing I was receiving a trove of passionately expressed, unsolicited self-statements, I responded to almost every one engaging in give and take follow-up e-mails with, rough-guess, well over half the people who wrote me. Also useful was the interactive blog where in excess of 350 people published heartfelt online statements where I posted my comments to themes. I kept every e-mail and blog exchange and had a student researcher perform several tabulations of the content which I cited in the comprehensive theoretical framework I published 1½ year later. The article also evoked letters from several researchers who sent data they thought I would find interesting.

Writing Style and Verifiability

A career in academia—conducting research, guiding and evaluating the research of others, and reading scholarly articles—I have grown critical of academic jargon. I have long felt communication best when ideas are expressed simply and, while admittedly I have much to learn here, it has always been my intent to speak straightforwardly without pretense.

Writing up my findings, I attempt to make the phenomenology referenced in the theoretical frameworks I construct as tangible and visual as I can to give readers a concrete view of what I am out to explain is off and any remedy, implied, or described.

1. I illustrate every construct and almost every dynamic referenced with descriptions of situations I have witnessed.

2. I give all people portrayed an opportunity to review and comment on actions I describe them taking. This serves as a self-check that I “got them correctly.” I consider this axiomatic action-research methodology. When a subject disagrees with how I described a situation, I discuss it and either correct my interpretation or explain the reasoning that led to my different interpretation. When we cannot reach agreement, I often acknowledge our disparate views in my write-up.
3. The criteria I use selecting illustrative vignettes is neither modal nor random. The main criterion is what I believe best evokes reader identification. As I have been emphasizing, I want people recollecting personal experiences to determine whether the theory I am offering sheds light on what they have and will experience. I refer to the process of having readers personally validate the intellectual plausibility of what I have concluded as “second-order phenomenological validation.”
4. I promise anonymity to all informants. I rely on a very simple coding system to help me remember who was involved.

Formulating Theoretical Frameworks

To date, I have produced eight book-length exposés, each on themes I considered relevant to making the world of work a place where people can be their authentic selves, form meaningful relationships, and realize their inner capacities. When my children were young, thinking about jobs they might eventually take, I began a project write-up with a question: “How many jobs do you know of that you’d wish on someone you love?” I wanted my research adding to that number.

Very affected by social activist exposés of mind-set determining hidden persuaders, I saw something analogous taking place in organizations. People were being offered choices that, as an outsider, I saw variations of a single alternative. I wanted to see what was needed for people to have real alternatives in making work decisions that determined the course of their lives. Individuals were always at forks in the road, I did not see most having the means to know enough about the variables involved, or sufficiently conscious of their inner needs, to be making constitutionally valid choices.

At the time, I had consultancy access to several organizations struggling with system-wide problems caused by people dealing with variables they could not grasp. There was a Catholic Teaching Order running high tuition schools to make expenses having staffing problems caused by defecting monks. It took an outsider for them to see the obvious incoherence. Monks had chosen a life of poverty, celibacy, and obedience to teach the poor. Then there was a company with high-value employees making bad-for-the-family decisions by taking overseas assignments that they thought essential for promotion. And I encountered others.

The book-length report I published (Culbert, 1974) was aimed at showing people how to raise their awareness of the force fields affecting their thinking and to speak in an increasingly authentic voice to people making decisions that deeply affected them,

illustrated by a long case study illustrating the use of prototype methodology at the Procter and Gamble Company.

The next two projects exposed and investigated the politics inherent in workplace relationships with a coinvestigator (Culbert & McDonough, 1980) and (Culbert & McDonough, 1985). We began our investigations thinking the mainstream mind-set of eschewing work politics was naïve and problem creating. Too many people were under the misimpression that politics could be avoided and that all politics were bad. We thought that viewpoint needed correcting. Our investigations allowed us to create a data-based theoretical framework that explained the inevitability of self-interested pursuits and the political dynamics that ensued. It also provided guidance for learning the basis of others' pursuits and promoting increasingly authentic team-play.

Looking back, I now see a common purpose to my next four muckraking efforts. Each was aimed at prodding managers to be more other-directed in their focus and to become collaborative teammates with their reports. The first project was a response to a deficiency I repeatedly observed managers having in just about every company in which I consulted. Too many managers thought it legitimate to insist that reports think and behave as they expected, with expectations based on a version of their idealized self. When I faced them with the impossibility of this happening and my belief their attempts to continue would be relationship defeating, the backup tactics for getting compliance that many gravitated toward was to manipulate and bribe. Of course, they did not call it that, they called it "incentivize" and "motivate."

In my mind, managers needed a crash course in approaching people psychologically and that is what I set out to provide. Starting with a very basic psychological fact, "Reality is an artifact of the mind that views it," I set out to produce a theoretical framework that both documented the folly of insisting people view events as a manager views them and the managerial advantages of learning enough about the other person to approach them on terms they personally relate to. For this project, I drew on a trove of short case descriptions and consulting interviews that I had been collecting for 20-plus years. I titled the resulting book-length report *Mind-Set Management* (Culbert, 1996) and accepted the publisher's commercialized subtitle. Also included in this report was a questionnaire, and detailed instructions for how to use it in learning the mind-sets of others. By the way, to this day, I much prefer impractical subtitle used in the report I submitted—it was far more accurate: "Making Management a Psychological Science Instead of a Manipulative Art."

My fifth book-length investigation (Culbert & Ullmen, 2002) dealt with the dysfunctional infusion of hierarchy into managerial relationships. It was a response to complaints managers make about employees' lack of candor in sharing their views and deficiencies, and to stand accountable for their errors. To me, this was a classic case of blaming the victim. How could employees be up front with the person who has all the power? Conducting interviews and reviewing an extensive literature to learn more, I eventually concluded that hierarchy serves many important structural purposes but is toxic and inappropriate in the context of give-and-take relationships—which is what I thought companies needed. I set about constructing a framework that would educate managers to the appropriateness of two-sided, reciprocally accountable boss/direct

report relationships. Moreover, in the process of listening to informants, I realized that most managers have an inappropriately limited view of what accountability means. My report includes a more constructive definition.

The next topic, number six of eight book-length investigative reports (Culbert, 2008), was prompted by my reading philosopher Harry Frankfurt's essay on "bullshit" (Frankfurt, 2005). Differentiating lying from bullshit, he made a point that dramatically struck home. Lying requires thinking about the truth and making a conscious decision to say something else. In contrast, speaking bullshit requires no such thinking. One merely thinks about what the other person needs to hear to go along with their agenda. Mulling this, I came to a personal revelation: Bullshit is the communication etiquette of choice for corporate communications. In a flash, I realized how much I have been swimming against the tide in my career-long consulting. So much of my work was involved helping people constructively discuss what for them had been undiscussables. I set about collecting data. I learned more deeply about the role bullshit plays in the workplace one could not be viable without it. I also collected data about the advantages of forming what I called straight-talk relationships and enough understanding about how to go about forming them.

I owe serendipity the credit for my seventh book-length report (Culbert, 2010). Wanting to attract readers to my *Beyond Bullsh*t* publication, I wrote an article on what I had long thought the most blatant bullshit organization practice of them all: the annual pay and performance review. In response to my submission to an academic journal, I received a request that I also allow it to be simultaneously published, in boiled-down format, in another journal with which they were collaborating. The result was publication in the *Wall Street Journal* mentioned earlier. It appeared full page in 11 million printed papers worldwide. I have previously described the focus group data set created by that article.

Contacted by a big-time publisher to write a book, I took this as an opportunity to elaborate my argument, and my performance PREview alternative, data documented and illustrated. The book led to other requests for articles, numerous media events, two *The New York Times* requested op-eds, another for the *Los Angeles Times*, and many consultancies and short articles. My code terminology for all of this: I am out to lead the million person march on Washington to get rid of performance reviews.

Finding myself in the autumn of my career, I felt compelled to ask the deeper question. It is one that is been shadowing me in every investigation. What is going on that causes management mischief whack-a-mole? Get rid of one bad practice and another pops up. Why does so much bad management behavior emanate from well-intentioned people? What is going on systemically that throws so many people off? This was a question I raised for 18 years in weekly Sunday morning breakfasts with my dear friend Warren Bennis—who is no longer around to read my conclusion. On this endeavor, I have had thousands of informants and scores of hypotheses and up until I forced myself, I could not converge on an answer.

Pushing myself, striking out on a write-up, not knowing where I would land, I have spent several years working out a theoretical framework. I did come to a conclusion and it is one that I know Warren would have applauded. But it is not a conclusion he

would have reached. Why? Because Warren saw the wonderfulness in leadership and studies and wrote about leaders and their virtues. My conclusion is the opposite, I see leaders needing to step up and most too self-serving to do so. And I have the data (Culbert, S. in press).

Conclusion

I have spent many years identified with two societally important social movements that I thought would humanize the world of work: Applied Humanistic Psychology, most notably the NTL Institute;² and the Quality of Working Life/Socio-Tech Redesign. While each movement's impact continues today, neither has the societal prominence it deserved. My legacy? Producing evidence of what has been going on amiss. I lived my life as an optimist pointing out what needs fixing, counting on people finding the means to make situationally and personally appropriate use of it. A great deal needs to be done. I see wonderful opportunities awaiting researchers out to make the world of work a place where people can be their authentic best and realize dreams.

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Notes

1. A symposium titled "Culbert's Influence: Demystifying and Deconstructing the Leadership Process," Academy of Management, August 9, 2010.
2. I spent 2 years working for them, reporting to Charlie Seashore and Leland Bradford, 1967 to 1969.

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