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Reclaiming "Anthropology: The forgotten behavioral science in management history" – Commentaries

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REVISITING THE PAST

Reclaiming "Anthropology: The forgotten behavioral science in management history" – Commentaries

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Abstract

Purpose – This collection of commentaries on the reprinted 1987 article by Nancy C. Morey and Fred Luthans, "Anthropology: the forgotten behavioral science in management history", aims to reflect on the treatment of the history of anthropological work in organizational studies presented in the original article.

Design/methodology/approach – The essays are invited and peer-reviewed contributions from scholars in organizational studies and anthropology.

Findings – The scholars invited to comment on the original article have seen its value, and their contributions ground its content in contemporary issues and debates.

Originality/value – The original article was deemed "original" for its time (1987), anticipating as it did considerable reclamation of ethnographic methods in organizational studies in the decades that followed it. It was also deemed of value for our times and, in particular, for readers of this journal, as an historical document, but also as one view of the unsung role of anthropology in management and organizational studies.

Keywords: Organizational ethnography, Anthropology and management studies, History of organizational and management studies, Ethnography, Social anthropology

Reflective commentary on
"Anthropology: The forgotten behavioral science in management history"

When Dvora Yanow asked us to reflect on the paper by Nancy Morey and Fred Luthans – originally presented in 1987 at the Academy of Management (AOM) History Division and then, having won a Best Paper award, published in the Proceedings – we jumped at the chance for two major reasons. First, this would be a fitting tribute to the memory of Fred's now departed former doctoral student, close colleague, and environmental activist Nancy C. Morey (1941-1997). In the early 1980s, Nancy came to the organizational behavior (OB) program at Nebraska proudly armed with a PhD in anthropology from the University of Utah, with several years of teaching and research in the field. Taking on the rigors of doing a second PhD, she was obviously very motivated to learn management and OB, but immediately she gave new meaning to Fred's often used axiom that he learns more from his PhD students than they learn from him. Fred fondly remembers one of Nancy's light-hearted comments that one of the best research tools of an anthropologist is a good pair of orthopedic shoes.

Although Nancy breezed through the obligatory OB theory and research methods courses, her heart and perspective remained with her first and true love of anthropology. Unlike some of Fred's colleagues at the time, he strongly encouraged Nancy to lead with her strengths, and this resulted in his co-authoring with her not only this history piece, but also two *Academy of Management Review* qualitative methods papers (Morey and Luthans, 1984, 1985) and a qualitative, ethnographic study of informal organization from her dissertation, published in *Human Relations* (Morey and Luthans, 1991). Frankly, Fred always felt these relatively early qualitative papers in the top management and OB journals have not received the attention they deserved.

The second reason we relish the opportunity to prepare this commentary is to make an assessment of what has transpired over the past 25 years since that paper raised the issue of the forgotten behavioral science of anthropology in the study of management and organizational studies. We note, in addition, how fitting it is that this republication marks the quarter century anniversary of its first appearance.

1. Initial intent and reaction

Nancy and Fred's intent in writing the AOM paper 25 years ago was to highlight that the perceived role of anthropology in general and ethnographic research in particular had been slighted (at least compared to the other behavioral sciences of psychology and sociology) in their contributions to the historical roots of organizational studies. Remembering that the academic field of management and OB is a relatively young discipline (e.g. Fred was hired to teach the first courses in this field at Nebraska in 1967 and is generally recognized to have published the first mainline text in OB in 1973), Nancy and he intended to document that an unrecognized amount of original management thought was strongly shaped by insights from anthropology and that much of the early seminal research used ethnographic methods (e.g. Chapple, 1941; Follett, 1940; Mayo, 1940; Whyte, 1948). However, a quick look at the contemporary organizational studies literature suggests that this auspicious beginning was not sustained through the years.

Although the slow but sure recognition of rigorous qualitative methods in recent years is comforting (especially with the founding of this *Journal of Organizational Ethnography*), focus remains predominantly on either multiple case studies (in search of the generalizability demanded by postpositivist philosophy) or occasionally on grounded theory. Organizational ethnography, on the other hand, has yet to realize the level of importance that its roots would have (should have) inferred.

Looking back at the AOM paper, we are reminded that the early management literature was replete with wide ranging opinions, worldviews, and scholarly backgrounds. Although today we readily acknowledge the influence of other fields, in those days, as was brought out in the paper in reference to the formulation and conduct of the Hawthorne studies, management scholars proactively and continuously asked for input from outside disciplines and intentionally crafted research projects so they could bring together diverse theoretical and methodological backgrounds (Morey and Luthans, 1987). In the early days of the management field, rather than having one dominant philosophy of science, or one dominant method, the pioneers had a diverse set of tools and knowledge claims at their disposal. It is not surprising then to discover how innovative and groundbreaking those early studies were and how much they enabled not just management to progress, but other social sciences as well (e.g. the Hawthorne studies are well known in psychology and sociology) and even engineering (e.g. Taylor's scientific management). The question that lingers, which still bothers us, is why did this inquisitive, yet cooperative, spirit disappear?

2. Danger of the pervasive dichotomy

One answer to what happened to that inquisitive, cooperative spirit of the early pioneers is the age-old tendency (whether in politics, sports, peer groups, or academic pursuits) to form into what we will call the "vs" problem (i.e. "us vs them"). Nancy Morey was very amazed and concerned, coming from anthropology to the management field, about the extreme dichotomies she observed (e.g. objective vs subjective, quantitative vs qualitative, etic vs emic). For example, anthropologically trained scholars and those advocating a human science approach argue that social sciences do not lend themselves to the same scientific methods used by the natural sciences. As German philosopher Wilhelm Dilthey advocated many years ago, research within social sciences may be better served with a focus on rich, deep understanding rather than on proof or prediction. In other words, the human, organizational world is fundamentally complex and often defies the prediction that is given in the natural world. To this end, Kilduff and Mehra (1997) stated that "the scientific method" may be problematic, as "truths" in social science are malleable as well as contextually and linguistically determined, and any attempt at objectivity in an empirical endeavor is in danger of being false. In other words, this side of the "vs" is subjective, qualitative, and emic.

On the other side of the "vs," the dominant belief among most contemporary management scholars is that "good research" equals objective, quantitative, etic research (noted caustically by Morey and Luthans in that 1987 paper). This approach pursues truths that may be generalizable across multiple contexts and cultures. In contrast to the more inductive and anticipatory approach discussed above, this other, objective, quantitative, etic side of the "vs" is firmly grounded in a postpositivist worldview that is characterized by being reductionist and logical, and emphasis is placed on quantitative data collection and analysis, probable cause and effect relationships, and a priori theory testing in an effort to generate general laws of human behavior (Creswell and Plano Clark, 2007). As such, objectivity in empirical and theoretical endeavors is highly regarded. Although somewhat more relaxed compared to the positivism that preceded it, postpositivism still regards the researcher as an expert capable of quantifying the social world into useful categories and offering probable causes and effects of human actions (Phillips and Burbules, 2000).

Unfortunately, disagreements among researchers in the "vs" camps may prevent organizational studies scholars from creating the common ground that we seemed to share historically, which today are trying to be brought together again by mixed methodologists such as our Nebraska colleague John Creswell (Creswell and Plano Clark, 2007). Whereas the 1987 AOM paper suggested the utility for organizational studies of a more qualitative approach associated with early anthropological work, its authors also saw ethnographic methodology primarily as an opportunity for bridging the "vs" dichotomy. In particular, in the 1984 AMR paper, "An emic perspective and ethnoscience methods for organizational research," Nancy and Fred presented a set of ethnoscience techniques drawn from anthropological research which had the potential to bridge inductive discovery and "subjective data of immediate practical utility for practicing managers and researchers [with] data gathering techniques that can be objectified and thus be adaptable to more traditional methodological analysis and conclusions" (Morey and Luthans, 1984, p. 28). In doing so, the authors endeavored to create a common ground between the two camps and provide some opportunity for dialogue and mutual understanding and learning.

3. Where do we go from here?

Unfortunately, a cursory look at the contemporary management field suggests that, rather than the healthy dialogue hoped for, there is still a dominance of traditional quantitative approaches and a continuous silent war on the margins of the field. The reason for this, we suggest, may be traced back to the clash of different worldviews that are associated with different methodologies, and with it, a falsely held belief within the postpositivist arena that qualitative methods lack the scientific rigor of the quantitative ones. If ethnography is to bridge the "vs" worldviews in the management and OB field, what we (and most others) feel is required is for us to conduct rigorous ethnographies. This requires management scholars to familiarize ourselves not only with where we came from, which was the intent of the 1987 AOM paper, but also to "embark on a program of extended reading in cultural anthropology, giving particular attention to ethnographic accounts and examining how different ethnographers have conceptualized and written about different cultural systems" (Wolcott, 2001, p. 156).

At the same time, the management field has to embrace the unique value that ethnography may bring to it, beyond that offered by other qualitative and quantitative approaches. The ultimate value of ethnography to the field of management lies in its ability to explore the idiosyncrasies embedded within human organizations. In the words of Watson (2011, p. 204), even though ethnography is difficult, time consuming, and emotionally draining, "there was no real alternative [to ethnography] if I wanted to contribute in a worthwhile way to the social scientific understanding of how managers manage, how organizational change comes about, how micro-politics operate, and how employment relationships are shaped and maintained."

4. Concluding thoughts

We end this reflective commentary with a call for more dialogue among scholars with diverse philosophical backgrounds and offer ethnography as a method that may facilitate getting back to our roots of an inquisitive, cooperative pursuit of understanding and truth. Ethnography shaped our knowledge for centuries, perhaps starting with the Greek philosopher Sectus Empiricus, who conducted cross-cultural research on the meaning of morality, to the golden age of ethnography represented by the work of Bronislaw Malinowski, Robert Park, and Ernest Burgess, to name but a few. It is not surprising to realize that much of what we know today in the social sciences has its roots in early ethnographical explorations. This is not to say that other methods are irrelevant. To the contrary, we strongly believe that true progress may only be made by embracing diverse perspectives, approaches, and assumptions and cooperating across them (i.e. from the perspective that "we is always stronger than me" in all of our life's endeavors). Shining the spotlight back on ethnography after 25 years in this ground breaking *Journal of Organizational Ethnography* seems an important step forward on the path to progress in our field.

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It's all in the details: Ethnographies of organizational life

In the early 1990s, I was working with a group of anthropologists at Xerox PARC and deciding where to return to graduate school. I knew I wanted to study what it means to work in organizations, and I was planning be an organizational ethnographer. I had dropped out of a sociology department filled with organizational scholars who raised their eyebrows and made a quick escape whenever I mentioned the words organization theory and ethnography in the same sentence. So I asked my colleagues, "Should I apply to anthropology departments instead?" The PARC anthropologists were not enthusiastic; they argued that anthropologists marginalized organizational scholars as much as sociologists marginalized qualitative scholars.

I was reminded of this discussion when I read Morey and Luthans's (1987) piece describing the role of anthropology in management history. In recent years, just as when they wrote their paper, anthropologists have not been thick on the ground at the Academy of Management Meetings. However, there is a community of management scholars who come from a variety of backgrounds and embrace ethnographic traditions. For instance, I studied organizational ethnography in an industrial engineering department with an advisor who was a graduate of a business school. And while some management scholars may casually attribute ethnography's contribution to sociology or psychology, many in the community of ethnographers in management draw on the legacy of anthropology in order to make significant contributions to organizational theory.

Morey and Luthans (1987) remind us that anthropologists influenced organizational studies both methodologically and theoretically. Early ethnographers such as William Foote Whyte and Donald Roy encouraged close, detailed examination of interactions, which created vivid, compelling characterizations of organizational realities. And many of their early discoveries have become foundational truths in organizational theory: a characterization of organizations as social systems, the need to understand the interplay of formal and informal relations, and the importance of relationships across levels of the organizational hierarchy.

For contemporary organizational ethnographers, today's incarnations of Morey and Luthans's themes include concerns about flattening organizational hierarchies, porous organizational boundaries, ever-changing work technologies, and shifts in employment relationships (Arthur and Rousseau, 1996; Cappelli, 1999). These organizational realities have implications for the experience of work, including increasing employment insecurity and an expectation for workers to be "flexible" and adjust to the vagaries of their organizational environments (Barley and Kunda, 2004; Smith, 2001).

Organizational ethnographers continue in the anthropological tradition by exploring these concerns with a commitment to portraying organizational life in all of its complexity. For instance, there is continuing interest in understanding organizational culture, with a particular focus on workers' experiences and feelings of ambivalence (consider Kunda's, 1992; Weeks's, 2003). Scholars have also extensively chronicled how organizational and technological change and organizational members' interpretations and actions are mutually constituted. They describe how people embrace, resist, and contest implementation of new technologies or processes, showing how this interplay can challenge or replicate status hierarchies (Barley, 1986; Orlikowski, 1992; Kellogg, 2011). Others have shown how such changes impact workers' perspectives, triggering concerns about professional autonomy and heightened performance pressures (Perlow, 1997; Mazmanian et al., 2012). Finally, scholars are also exploring the broad changes in the inter-occupational division of labor, such as the growth of service and technical work, and the rise of new forms of work arrangements. These influence workers' interpretations and behaviors, creating feelings of insecurity, prompting constant skill development, and limiting time off from work (Barley and Kunda, 2004; O'Mahony and Bechky, 2006, 2008).

What an anthropological sensibility such as Morey and Luthans describe can bring to organizational studies can be seen in an example from my own ethnography of a crime laboratory. I am studying how the work of forensic scientists is affected by the crime lab's position at the intersection of two very different social worlds – science and law. A consequence of the porous boundaries between the lab and the criminal justice system is that analysts participate regularly in both worlds. This has implications for their construction of their role as forensic scientists and it shapes their occupational practices, which comprise multiple forms of boundary work interwoven with their laboratory work.

I sat beside forensic scientists at the lab for 18 months, watching their work, chatting, and asking questions. I learned how to process my DNA profile from a cheek swab, test fire a handgun, and prepare for courtroom testimony. In order to understand these scientists' place in their occupational and professional worlds as well as within the criminal justice system, I also went to court with them, as well as attended lab management meetings, professional association meetings, and training sessions for attorneys and investigators. My close observation of the social world of the crime laboratory enabled me to recognize repeated patterns of interaction within, across, and beyond the units in the laboratory. I came away with a deep understanding of forensic scientists' work practices, cultural interpretations, and what it means to work in an organization at the intersection of science and the law.
A significant feature of forensic scientists' work was managing the boundary between their scientific lab work and the workings of the legal system.

Forensic scientists spent much of their time writing reports to be used in court; however, <2 percent of cases resulted in a forensic scientist taking the stand to testify. And although these analysts almost never testified, the specter of testimony hovered over the lab. Gossip about testifying was frequent, and I heard mention of testifying almost daily. Most analysts dreaded testifying: they experienced "being sick to my stomach," "losing 10 pounds," and "feeling mortified on the stand." Many complained that testifying was their "least favorite part of the job." They objected to the "horrible" and "intimidating" aspects of testifying, and they worried about being misrepresented and feeling "like a puppet." "When you are on the stand," noted one analyst, "nobody is your friend." Forensic scientists' dread of testifying led them to approach their boundary work between the scientific and legal worlds in specific ways. One tension created by working in overlapping worlds was that while laboratory work was a communal scientific process, analysts were held individually responsible in court for their conclusions. Therefore, analysts first had to take ownership of their conclusions by achieving confidence in the scientific facts. They gained this confidence through membership in a community: they reviewed one another's analyses and conclusions repeatedly, and the lab's practices and protocols were audited by the broader forensic science community. Everyone in the lab obsessively documented their work, for scientific, professional, and legal reasons, as one analyst noted:

> "With science, it has to be reproducible and it has to be credited. […] We write down everything, that way someone can come behind us and get the same results that we do. It is also for ourselves, because if we go to court it can be a year or so later, so we want to be able to jog our memory. It helps me to know that if I go to court, I'm going to feel confident with what I testify to. And also to cover ourselves, because I took a course with a defense expert who says, "I will look for things you haven't written down that could possibly change your results or be a way for me to discredit certain things about your results or your interpretations."

Analysts also worked hard within the laboratory to translate across the boundary between these worlds, which enhanced their feelings of external control. The report reviewing process included checking every written line, making sure that reports adhered to lab and community standards in the wording of judgments and conclusions. Forensic scientists also anticipated potential courtroom reactions when writing reports, discussing how lawyers or members of the jury would react to particular words and images or possibly misconstrue their statements. And analysts tried to "educate" the district attorneys by meeting in advance with them, because "things go more smoothly when they know exactly what we can and can't say."

I never would have realized how important testimony was to the organizational practices of the lab if I hadn't been present day after day, hearing analysts worry about testifying but noticing that they never seemed to go to court. This led me to investigate how being at the boundary of scientific and legal worlds influenced their work. By paying attention to the details of their experiences in context, I gained insights into the importance of emotion in boundary work and the real consequences of perceived legal constraints on laboratory practices. This is what the anthropological approach outlined by Morey and Luthans can contribute to organizational studies: contextualized theory grounded in organizational life. And while we probably are not going to convince many anthropologists to join the Academy of Management, encouraging organizational scholars to adopt this approach would greatly benefit the field.

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Why organizational ethnography is a dangerous game: A plea for clinical research

Reading the 1987 paper by Nancy Morey and Fred Luthans has been a valuable reminder that applied anthropology and sociology have made great contributions to our understanding of organizational phenomena and occupations. What is striking in reviewing this material is the wide range of inquiry methods that have been involved – traditional non-interventionist ethnography, participant observation, action research, interviews, surveys, and anthropologists working as consultants. I want to express a bias toward a certain mode of inquiry based on decades of experience as a consultant to various kinds of organizations.

Organizations are coherent systems, communities, interlocked units, and in this sense differ from occupations. The entry of an ethnographer, no matter how many permissions have been given, is an intervention of unknown proportions into that system. It might be beneficial or destructive, but the point is the ethnographer cannot decipher with his is her own methods what the impact will be. Why should this matter? Because members of the organization will make various kinds of interpretations of the intervention, will talk to each other about it, and, in that process, will launch unknown change forces. All this is ok if the ethnographer acknowledges that it is an intervention and if he or she also has clinical skills to deal with the consequences of his or her presence. One obvious complication that novice ethnographers have difficulty with is the natives confronting the ethnographer with questions about process and results – What have you learned? How are we doing? Will you share your findings with our boss? and so on. If the ethnographer tries to ward off such approaches by hiding behind confidentiality or invoking some prior contractual arrangement that had been made with whoever agreed to the project in the first place, he or she is likely to alienate the natives, who will be more likely to become less approachable or tell only safe things, such that the researcher will get less good or even fudged information. What this reveals is that organizations are more integrated hierarchical systems than merely a collection of occupations, and the ethnographer's presence has implications for who sees what results and what will be done with them.

The way out of these dilemmas and pitfalls is to abandon the purist notion of the ethnographer just describing what he or she finds for the edification of an academic audience and, instead, to define the relationship between the outsider and the insiders as a clinical relationship. By this I mean that the ethnographer is there to help in some way or another, to define the project as connecting somehow with what the organization or some part of it needs, to become an observing participant, to become part of the system so that the intervention itself becomes part of what is observed. This sounds scientifically messy and some purists will say that the ethnographer must minimize his or her impact on the observed system, but the effort to minimize often proves to be a bigger problem because it distances the ethnographer from the natives and prevents finding out why some of the things observed are done the way they are.

My insight into this arose from finding that it was easier to observe and ask about those observations if I was in some kind of helping role. I have seen participant observers like Steve Barley, studying medical systems, acquire key ethnographic data because he was a working member of the team. I have seen how certain mysterious activities in an organization could only be deciphered by my being able to ask questions that would ordinarily be inappropriate, but which I could ask because I was there in a helping role. I realized that many of the key insights in medicine and other fields arose from studying the practice through being involved directly in it.

I called this Clinical Research (Schein, 1987, 2001, 2008) and felt that it was particularly appropriate to organizational inquiry. It means that if the researcher is lucky enough to be called in to solve problems, the ethnographic research can be done in conjunction with the helping intervention. On the other hand, if the ethnographer has entered without that mandate, I suspect that he or she will start to get better data as he or she becomes more accepted and that this acceptance will hinge on the degree to which she or he will be willing to be helpful in some capacity or other. An ethnographer should welcome such requests or opportunities and become both a clinician and a participant observer.

What this means, however, is that the organizational ethnographer must have some clinical and organization development skills. It is not enough to be a trained observer, an expert in taking field notes and content analyzing them. The ethnographer has to combine those skills with interpersonal skills in giving and receiving help and has to be willing to be drawn into the activities of the organization to varying degrees. In my own graduate training at Harvard's Department of Social Relations, I had to do a clinical internship as part of my PhD training in social psychology and only realized much later the immense value of this early organizational experience at the Walte Reed Hospital. I was able to supplement this later with workshop training in NTL programs (the National Training Laboratories Institute – DY), but the graduate internship provided the basic experience of living for a while in an organization in which I had a helping, not a research, role.

Graduate training in most Business School PhD programs, including MIT's Sloan School, emphasizes first the research skills and then sends students out to figure out how to enter and get along in organizations. I would send students out first as interns to learn how to enter an organization with the intention of learning how to be helpful and then teach them observation and analysis skills after they have had the clinical experience. Ironically, for admission to the MBA program we require some organizational experience, but for the PhD program we are much more lax about this. I was fortunate to have had a clinical internship and then consulting experiences, both of which taught me the value of clinical research. I also believe that experienced ethnographers know very well how important it is to be helpful to the tribe or organization they are studying. The question is when we will admit that good organizational research is a combination of both.

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Forgotten histories of the anthropology of organizations

The *Journal of Organizational Ethnography* is doing valuable service in reprinting Morey and Luthans' paper on the role played by anthropology in the early history of organizational studies – not least because it is a source I did not find when researching the history of relations between anthropology and organizational studies (Wright, 1994). But the story they tell is familiar. They start with the Hawthorne studies and extend as far as Chapple and Arensberg's Interaction Theory, Warner and Gardners's contributions to the Human Relations approach to management, and Whyte's earl Organizational Ethnography. However, they cut this story short by ending with Roy's and Dalton's ethnographies of 1959. Whyte, for example, kept working into the 1990s, with studies of Mondragón cooperatives (Whyte and Whyte, 1991) and learning organizations (Whyte, 1991). Writing in 1987, Morey and Luthans claim that from the 1950s there was a move away from "naturalistic observational studies" of organizations, and that organizational studies "play a very small role in modern anthropology." Arguing that their account ends prematurely, I will trace how that early work in the USA was translated into another tranche of studies in Britain in the 1950s and 1960s.While it is important to trace who did what, where, and when in the style of Morey and Luthans, there is an equally interesting history to be told of developments in the research methodologies and research problems not only from the 1930s to 1950s, but also into the 1980s (and beyond).

Manchester shop-floor studies

Morey and Luthans trace the development at Harvard of the Hawthorne factory studies and the parallel community studies. Researchers on these programs met weekly at the interdisciplinary Harvard Society of Fellows, and together developed courses and student projects and set up the Society for Applied Anthropology. What Morey and Luthans do not mention is how these academics also networked internationally. In particular, in 1953-1954, when George Homans, Professor of Sociology at Harvard, was visiting professor at the Manchester department of anthropology and sociology in England, he suggested they carry on the Hawthorne work.

The Manchester department, led by Max Gluckman, gained funding through the Department of Scientific and Industrial Research from Marshall Aid for reviving industry after the Second World War. This funded a series of five industrial factory studies. Tom Lupton (who became head of Manchester Business School) studied Wye's modernized waterproof garment factory, which employed mainly women, and Jay's production of heavy electrical transformers, which employed men (these studies were published together in Lupton, 1963). Sheila Cunnison studied two factories which employed both men and women: Dee's, a small traditional manufactory of waterproof garments (Cunnison, 1966), and Kay's multiple tailoring (unpublished). Shirley Wilson (1963) studied Alvalco, which employed women in valve assembly. In a second phase in the 1960s, the Citro¨en works were studied from three perspectives: Isobel Emmett studied the managers, David Morgan the assembly shop, and Michael Walker the machine shop (Emmett and Morgan, 1982). These eight studies were known collectively as "The Manchester shop-floor studies".

Participant-observation

These researchers took organizational studies in new directions, the first of which was methodological. The Hawthorne studies had tried a number of methods. First, they had taken women out of their everyday working conditions and set up an experimental. Relay Assembly Test Room, in which the researchers were "non-directive interviewers" and more than usually sympathetic supervisors. Second, the Hawthorne factory's own. Industrial Research Division engaged in large-scale interviewing, interacting with 21,126 workers between 1928 and 1930. Third, the anthropologist Lloyd Warner took three teams of men out of their shop floor and set them up as a small-scale society in the Bank Wiring Observation Room. There, one researcher, the observer, tried unobtrusively to keep a continuous record of all activities, while another, the interviewer, remained an outsider to the group, and interviewed the members about their attitudes. These studies privileged "observation." "Participation" was kept to the minimum necessary to get close enough to people to observe their interactions and hear their conversations.

In contrast, in the British studies, both "participation" and "observation" took on different meanings, as did their combination of insider and outsider in "participant observation." The initial five shop-floor studies did not dislocate workers from their everyday working environments and put them in experimental rooms. Instead, each researcher spent at least six months doing factory work on the shop floor. Each one joined in as a worker, even though those around them knew they were researchers. They learned how to do the work, became familiar with the concepts and language the workers used, and gained insights into their perspectives. In the evenings, the researchers made notes of different people's versions of myriad incidents and interactions, gradually unravelling the social processes of the workplace and tracing relations between groups and categories of workers over time. If "participant" meant becoming, as much as possible, an insider, "observer" meant not only watching and recording systematically, but being an outsider with a theoretical understanding of society against which to review the detail of field material. Participation and observation were given equal weight and held in tension.

This new relation between participation and observation had important methodological implications. Out of the tension between the researcher's two roles –between their wider understanding of social organization and the perspectives of workers learned in the field – came the discovery of "problems." Emmett and Morgan emphasize that "problems" are not hypotheses set up in advance. The researcher may start with a general issue, but the gold nugget of a "problem" is only found after fieldwork has begun, and it emerges from this process of holding field data up to current academic understandings. When the latter provides no adequate explanation for the former, the anthropologist has a "problem" on which to focus the rest of his or her study. This anthropological methodology ran counter to the quest for social science to appear objective and "scientific" at that time. Its iterative approach is still out of kilter with the a priori hypotheses and fixed research questions currently demanded by many research councils and ethics approval boards. It is therefore rare to find a description of how research "problems" are actually discovered in anthropology. Emmett and Morgan's (1982) account of the problems that emerged in the shop-floor studies is still one of the best.

Workers' "will to control"

A second important difference between the UK and US factory studies was that the Hawthorne studies' research agenda derived from senior managers. Roethlisberger and Dickson's (1939) account of the studies takes the perspective of managers for whom "problems" lay in workers' "illogical" behavior. For example, managers had designed rules and incentives with the intention that workers would compete against each other and strive continually to increase output, but workers in fact kept their level of output steady instead of acting in what Roethlisberger and Dickson (1939) call "their own economic interests" (pp. 533-534). The Hawthorne experiments had found that workers organized themselves informally in ways quite different to that presupposed by the formal organization of the factory. For example, they helped each other (against the rules) and "fiddled" the records by reporting that they produced the same amount as each other day by day, whereas in fact their individual outputs varied widely. The workers had a shared idea of a standard day's output and carried in their heads complicated records of under- and over-reporting so that the scores evened out between them in the long run. This phenomenon had baffled the Hawthorne researchers, and the five Manchester studies were set up to explain "output norms" and their relation to the informal organization of work groups.

Anthropologists researching in the colonies between the World Wars sought to demonstrate that people's social systems were logical, even if based on different premises than their imperial rulers', but anthropology's radical stance was not transferred to factory studies "at home" in the USA. Rationality remained the sole preserve of managers and researchers, reflecting the top down stance of the analysis. The workers' informally organized output norms, according to Roethlisberger and Dickson (1939), were motivated by non-rational "sentiment." In contrast, the Manchester shop-floor studies did not accept managers' definitions of the problem. Rather, in a way only full developed much later, they treated managers as part of the field of study, no more or less logical than anyone else (Wright, 2011). This opened the space for workers' forms of organizing to be accepted on their own terms. The researchers found that by coordinating their production and "fiddling" their reporting to maintain output at a steady level, workers maintained some control over their working lives and ensured they were all able to "make their wages." The Manchester researchers explained the output norms as the workers' entirely logical "will to control" managers. Managers could not continually raise everyone's output targets to those of the fastest workers or use variable output to pick out and fire individuals. The "problem" that emerged during the fieldwork was that some shop-floor workers exerted this "will to control" quite effectively, whereas other factories were characterized by "militant individualism" where workers competed against each other to get as much piece work as possible and did not keep the flow of work or the level of output even. In those factories, managers could change piece rates and pick off individuals, just as their more organized colleagues predicted. The Manchester researchers had a "problem" in that they could not find any good explanation for why workers in some factories organized output norms and exerted the "will to control" while others did not. They looked for variations between different sectors of the economy, different levels of job security and rates of unemployment, and whether the workforce was predominantly men or women, but none offered adequate explanations – and this probably remains an unsolved problem today.

Conflict

The third big difference between the Hawthorne experiments and their Manchester counterparts was that the former adopted from their founder, the psychologist Elton Mayo, an assumption that managers and workers shared a spontaneous urge to cooperate and work in consensus to achieve the aims of the organization, and if there was discord it was because this urge had been frustrated (Schwartzmann, 1993, p. 14). In Britain in the 1950s the opposite was widely assumed: that managers and workers were divided by class interests and discord was to be expected. The Manchester studies took a more complex and nuanced approach to conflict, inspired by Max Gluckman, the founder of the department, and his anthropological analysis of the everyday events of social conflict in apartheid South Africa.

By following events on the shop floor over a period of time, researchers were able to explore the cross-cutting ties, "multiplex" relationships, paradoxes, and unexpected alliances that maintained the social organization of the factory, its tensions, and inequalities. Cunnison (1982) analyzed the different forms of tentative and temporary "accommodation" reached between managers and workers in the first five shop-floor studies. There were rarely overt class struggles, but the researchers documented how workers used what Scott (1985) later called "the weapons of the weak": singing whilst working, trying to extend tea breaks, playing cards at any opportunity, or maintaining silence when a piece of machinery broke down. The Manchester researchers would not call even apparently acquiescent workers "non-militant." Instead, they described a continuous struggle, with both low-level managers and workers assessing their tiny victories in a "daily running outcome." This approach contrasted with Goldthorpe et al.'s (1969) large-scale questionnaire studies of factory workers, which concluded that peace had at last broken out among the newly affluent British workforce. Emmett and Morgan (1982, p. 154) criticize Goldthorpe et al. for their methods, which missed the detail of this low level but insistent conflict, and for their approach, which dismissed it as not part of the class struggle, with the result that those authors were, embarrassingly, unable to explain when strikes suddenly erupted in the factories they had studied.

Relevance to 1980s and the present

As the above account indicates, by the time Morey and Luthans were writing their paper in 1987, Cunnison (1982) and Emmett and Morgan (1982) had written their syntheses of the Manchester shop-floor studies, showing the ways that anthropology had continued to contribute to organizational studies. In 1981 an organization called GAPP (Group for Anthropology in Policy and Practice) was formed as a UK counterpart to the Society for Applied Anthropology in the USA. GAPP created a national network of about 300 anthropologists employed in academia, companies, public organizations, and consultancies in Britain and internationally, along with a glut of unemployed PhDs and current students. Through the next 20 years, GAPP held workshops and conferences on anthropological approaches to the study of organizations, provided training sessions for graduates who sought to use their anthropology in organizational analysis, and engaged senior managers from the sectors concerned in these discussions (Wright, 2005). The outcome of one of these GAPP conferences held in Swansea in 1991 was *Anthropology of Organizations* (Wright, 1994). The major changes to public authorities and to the whole system of governance in Britain brought on by Thatcherism during the 1980s meant that studies of bureaucracies assumed increasing importance (notably, the anthropological contribution of Britan and Cohen, 1980). Even more than the Manchester studies, which had looked at organizations from the perspectives of workers, the injunction from Nader (1972, 1980) to "study up" encouraged experimentation with ways to conceptualize how people and workers were part of large-scale systems of management and government. The approach of the Manchester shop-floor studies towards conflict as a continual process was taken much further in these 1980s studies. In particular, in the heyday of management studies' focus on organizational culture, which claimed ancestry in Bateson, Douglas, and Geertz, anthropologists critiqued the management studies literature's predominant and continued slippage into ideas of "authentic" culture, as a unitary system of underlying shared values to which all actions and discourses were somehow connected in a self-reproducing totality (Wright, 1998). By treating culture, and organizations, as sites in which both managers and workers were involved in a continual contest over who is trying to define what, for whom, with what consequences, organizations themselves could be seen as the ever-changing outcomes of continual processes of organizing (Wright, 1994).

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Hold the Mayo: Some comments on the origins of organizational ethnography

This is perhaps a Golden Age for organizational ethnography. We have new journals such as the very one you're reading, a noticeable upswing of ethnographic accounts appearing in mainstream journals, a steady stream of high-quality ethnographic monographs appearing each year, multiplying ethnographic subfields in what were once off-limits domains such as finance, law, science and technology, education, markets, entrepreneurship, design and so on. Heady stuff for sure and a signal that the field is indeed vibrant and expansive. Given the current fascination with ethnographic approaches it is perhaps no shock that a lively interest in where all this came from surfaces.[1] To this point, reprinting Morey and Luthans thumbnail sketch of our origins plays a useful role in providing both a historical account and something of an anthropological pedigree for organizational ethnography. My brief remarks here pick up and elaborate on the Hawthorne Experiments as treated, rightly so, by Morey and Luthans as the heralded and much recounted origin tale for the many fields of organizational research, among them organizational ethnography. There are three points worth adding to their sketch.

First, the role of Elton Mayo as the prime mover and protagonist in the canonical Hawthorne story requires qualification. Mayo did indeed secure abundant resources from the Laura Spellman Rockefeller Memorial Fund for the Hawthorne studies and did hire the research associates who carried out the work with considerable aid from Western Electric – the largest Bell telephone company of the day – that owned and operated the enormous Hawthorne plant, some 22,000 employees in the late 1920s and early 1930s. But Mayo himself was much more the armchair theorist, something of a convivial and persuasive dandy, who infrequently visited the plant, content largely to concern himself with the funding, design and direction of the studies from a pleasant – although externally funded and rather marginal – position at the Harvard Business School (HBS). His hand was perhaps most significant in the development of organizational ethnography on two occasions: One was the hiring of anthropologist William Lloyd Warner who, during the late stages of the project, consulted intermittently with the research team at HBS and added a quasi-ethnographic "naturalistic observation" phase to the studies.[2] The other occasion was Mayo's discounting of Warner's (and a few other team members) readings and interpretations of what was going on at Hawthorne, particularly during the Bank Wiring Observation Room phase of the project. This was the quasi-ethnographic period during which the informal organization was "discovered" as marked by the piece rate output norms set collectively and surreptitiously by workers rather than managers.

Second, despite Mayo's widely read just-so story of how the Hawthorne studies evolved and what was learned as a result, there was considerable confusion and disagreement among those involved during and after the studies as to what exactly the research was demonstrating. To wit, the famous Hawthorne Effect, the alleged boost in worker productivity in the early rely assembly room – the T-room – phase of the project, attributed to the mere (unintended) effect of paying kind and civil attention to the workers in the experimental setting, was unrecognized at the time. It emerged as a concept, now enshrined as a scientific principle, a few years later as something of an ad hoc explanation for what at the time defied explanation. To this day, however, the puzzle remains as much of a muddle as it did for the Hawthorne researchers on the spot who had a welter of alternative explanations available to explain the productivity boost – from rest period changes to special compensation to worker fears of losing their jobs in the midst of the Great Depression to statistical noise. But this tangled bowl of macaroni was sorted and straightened out by Mayo as the "surprising" Hawthorne Effect that as we now know stuck as a digestible – if gooey and highly decontextualized – explanation.

Third, when the Hawthorne experiments were completed, Mayo was able to gain the rights to the data (along with HBS where they are today archived). More critically, he exercised almost complete control over the early interpretations given to the findings. The executives at Western Electric were apparently quite pleased and comfortable with this arrangement. When the veritable wave of published papers and books on the Hawthorne studies began to appear in academic forums in the 1930s and 1940s, Mayo's theoretical stance toward the Hawthorne data was prominent in all of them—whether or not he was listed as an author or co-author. Mayo's (1933) book, *The Human Problems of an Industrial Civilization*, was a best seller at the time. In 1941, Mayo appeared on the cover of *Fortune* magazine and was celebrated in the issue as responsible for bringing a sturdy social science to the study of business. A critical literature did not begin to appear until decades later, well after the Human Relations School—so closely tied to Elton Mayo and the Hawthorne Experiments—was well established and widely accepted as a template for both understanding and studying work organizations (with strong vestiges remaining today, particularly in US business schools).

Given these qualifications, questions of why Mayo was so influential for such a long time and why the Hawthorne studies are so fondly remembered as germinal to organizational studies are relevant. Part of the puzzle is solved by the list of analytic and method contributions Morey and Luthans list at the end of their review.[3] Another part of the answer can surely be traced to the obsessive data accumulation and preservation that characterized Mayo's project (e.g. 21,000 interviews transcribed and catalogued). On this matter, William Foote Whyte of Street Corner Society fame said on the 50th Anniversary of the Hawthorne Studies that "(the research project) is still unsurpassed for detailed, systematic observational records of the behavior of work groups." But there is more. Mayo and his associates were prolific. The power of the written word is at work here. Thousands of papers, books, reviews, commentaries were written between the 1930s and early 1960s most of which were distinctly favorable to Mayo's reading of the Hawthorne findings.

This is a reading now well-known that emphasizes a psychological interpretation, edging toward the psychiatric, of individual behavior in organizations, a functional or equilibrium view of organizational life in which conflict is regarded as dysfunctional (in Mayo's term, "psychopathological") but avoidable if employees are well treated by management, and a highly bounded and almost insular treatment of organizations that ignores their interpenetration by a number of other institutions such as class, family, race, gender, and community. While such a reading is thankfully rather quaint contrasted to the organizational perspectives in vogue today, in Mayo's long-standing era, it constituted something of an institutional logic that was – and in many respects still is – attractive if not irresistible to managers.

In short, Mayo and those closely in tune with his views put forth the mid to late twentieth century's most prominent and seductive managerial ideology. What could be more attractive to owners and managers than to be repeatedly told that one's employees or direct reports are really irrational and illogical; that their lack of cooperation is but a frustrated urge to cooperate; that their economic wants mask a need to be consulted and listened to in the workplace; that these needs are best met by a more or less therapeutic regime that pays close attention to the social and emotional needs of employees; and that you – as owner or manager – are charged with a historical mandate (or destiny) to bring social harmony to the workplace? And all this backed up by the extensive and dogged scientific research conducted at Hawthorne. When stripped of its ideological trappings, however, Mayo's practical advice to managers is meretricious and boils down to a rather timeless aphorism: be nice to people and they will be nice to you. Generally it holds and being civil, paying attention to employees, encouraging teamwork, and so forth are undoubtedly all good things and part of what an effective manager must do.

Yet, what Mayo never reckoned with – despite Warner's insistence that there was more to employees' discontent at Hawthorne than simply the way they were treated by their bosses – is that discord in organizations arises from structural and power inequalities as well. Mayo's utopian view was that conflict is irrational and arises mainly from misunderstandings. But, if we put our rational minds to work, such conflict will vanish. That one can be both highly rational and highly uncooperative was a non-sequitur for Mayo. Not so for Warner who went on after his brief stint at Hawthorne to study organizations, occupations, and institutions in a far broader and more thoughtful fashion than was the case at Hawthorne, writing a series of dazzling community ethnographies of Yankee City (Newburyport, Massachusetts) in the period of emerging unionization in the USA. It seems then that if we need a primogenitor for organizational ethnography, W. Lloyd Warner is the one and Yankee City is the birthplace, not Hawthorne.

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The exciting beginnings, subsequent exile, and promising return of anthropology to organizational studies

The paper by Nancy C. Morey and Fred Luthans is intriguing. Unfortunately for me, I was unaware of its existence until my comments were requested. It clearly deserves wider circulation. As someone who has been working on the anthropology of organizations while simultaneously working to transform both academic and nonacademic organizations for more than four decades, I am excited by some of the new (or more accurately, recently forgotten) information this essay contains. I can also confirm some of the arguments from personal experiences and anecdotes told to me by William Foote Whyte and Elliott Chapple about the early time period discussed in the paper, as well as from my current efforts to teach this subject at Cornell University. The Morey and Luthans essay reminds me that anthropology was present at the origins of organizational research, exited this academic scene rather abruptly, and is now back with rapidly intensifying contributions.

The essay well captures the complexity of the interactions between Elton Mayo, W. Lloyd Warner, Conrad Arensberg, William Foote Whyte, and Elliott Chapple in the 1930s-1950s. These stem from a period when the boundaries separating the silos of sociology, anthropology, psychology, and political science were not so clear and when members of the Harvard Society of Fellows could make a late career decision about which of these disciplines they would finally take a PhD in, a world long gone.[4] The combinations of qualitative and quantitative methods and the relative fearlessness with which these individuals approached what now are improperly viewed as "applied" problems are central to the vitality of that period of work, as was their sense that industrial and labor relations could be handled in non-adversarial "win-win" ways through exercises in rational analysis and social solidarity. Reformers, not revolutionaries, they created a broad context for a multidisciplinary field of organizational studies. The essay also captures the sad result for most anthropologists of having to choose between being interested in organizations as a subject and staying in academia.

The history of the social sciences is as much a history of academic institutions and organizations as it is a history of independent social scientific subject matters. In her essential work on the history of American social science, Dorothy Ross (1992) points out that the American social sciences all grew out of political economy in the latter part of the nineteenth century as part of the feverish creation of graduate degree programs based on a productive American misunderstanding of the Humboldt University model of the German research university.[5] Starting with Johns Hopkins University and spreading quickly from there, the social sciences began carving out territories and creating independent graduate programs. While economics and history broke away from political economy earlier, by 1910, economics, history, sociology, anthropology, political science, and psychology had all become separate disciplines with separate professional associations, journals, and meritocratic ladders and control systems managing their mini-cartels.

While each of these fields has a unique history and dynamic, some major features are shared. All began promising to study society for the purpose of improving it. All ended up studying society for the purpose of building their own disciplinary edifices, working hard to stay at a safe distance from the study of socially controversial topics that got academics into political trouble.

Mary Furner's history of economics (Furner, 1975), Patricia Madoo Lengermann and Jill Niebrugge-Brantley's (1998) history of sociology and David Price's history of anthropology's suppression by the FBI and CIA (Price, 2004) all reveal the extraordinary pressures brought to bear on social scientists who linked their research to controversial social issues of the day. Madoo Lengermann and Niebrugge-Brantley, for example, show that people like Jane Addams, Sophonisba Breckenridge, and Adna Weber were key players in early sociology and political science, and, as sociology consolidated its academic status, they were exiled from the professional associations and later written out of the history of the discipline. By the 1950s, it seemed that most social scientists in academia had learned that the best practice was to write in jargon for other members of their own discipline and to keep out of the study of issues of public importance that would produce controversy. The alternative was to leave academia. This greatly stimulated the exit of "applied" social scientists from academia and/or the demotion of the applied fields of organizational behavior, human development, business administration, rural sociology, etc., to the second-class academic status that they retain to this day.

In the case of anthropology, the story is slightly more perverse. American anthropology's founder, Franz Boas (1928), stated clearly in *Anthropology and Modern Life* that the purpose of anthropology was to shed light on the great issues of our day, and he trained the likes of Margaret Mead and Ruth Benedict to do just that. Yet he was exiled from the American Anthropological Association precisely because of his activism on social issues. Figures like Warner, Mead, Benedict, and Oscar Lewis who felt a direct professional obligation to teach the lessons of anthropology to the public to improve business management, race relations, gender understanding, and the treatment of indigenous groups are either ignored or passed over lightly in the history of anthropology, while those who write discipline-oriented, theory-laden books and papers are celebrated as the leaders of the academic profession.

Despite being founded almost simultaneously and arising from radically different historical inspirations,[6] the disciplines of sociology and anthropology found themselves forced into joint departments at many institutions and spent decades carving up that turf until they were eventually able to separate. This turf war resulted in sociology getting the western industrial world as its territory and anthropology accepting the role of studying the exotic other elsewhere. So complete was this division of labor that recent generations of historically ignorant academics (including anthropologists) equated anthropology with the study of primitive and peasant societies and used to assert that, when the primitives and peasants were gone, anthropology would disappear. This, of course, overlooks the presence of Warner, Arensberg, Chapple, and many others the paper alludes to who obviously "did not get the memo." It also completely misunderstands the implication of American anthropology in attempts to stop the genocide of American Indians, to deal with the legacy of slavery, and to combat immigration quotas and eugenics in the USA. Lest this be thought to be a personal rant, I will point out that the Society for the Anthropology of Europe and the Society for the Study of the Anthropology of North America, units of the American Anthropological Association, were not founded until the final decades of the twentieth century. What the exotic anthropology myth does not misunderstand is how happily many anthropologists dedicated themselves to the non-western world having found that they could thereby stay conveniently out of the fray when issues of racial equality, civil rights, poverty, and genocide in the USA were being debated hotly.

When anthropology finally recovered from its lethargic acceptance of exile from the west and began noticing the extreme positivism and ongoing development of sterile and anticultural rational choice theories of society in economics, sociology, and political science, anthropologists began flooding back into the west and now across the surface of global capitalism to examine organizations, communities, businesses, policies, and practices using the lens of ethnography with its combination of rich data and linked social and cultural perspectives. This has ushered in a new generation of anthropological research on organizations that has not only been productive but has produced significant social critique.

Works like those of Irene Bellier and Thomas Wilson (2002), Melissa Cefkin (2009), Alberto Corsı´n Jime´nez (2007), Joseph Dumit and Regula Burri (2007), Douglas Holmes (2000), Hirokazu Miyazaki (2004), Julian Orr (1996), Annelise Riles (2000), Shore and Wright (2000), Marilyn Strathern (2000), Gillian Tett (2009), Susan Wright (2003, 2004, 2005), and many others now form an essential basis for understanding the transformations and experience of living in contemporary organizational environments. And, as in the founding generation, these works give a lie to the easy disciplinary boundaries that the Fordist imaginary applied to academic work has produced. The above writers have much to learn from and to teach to Chris Argyris, Steven Barley, Joanne Martin, Peter Senge, William Torbert, Harry Trice and Janice Beyer, John Van Maanen, Karl Weick, and many other important organizational theorists/actors.

Organizations are complex, largely open systems operating in a fluid and complex global environment. Understanding them and assisting them is an inherently interdisciplinary and theoretical/applied effort. While this not only seems desirable and is actively called for, there is nothing less likely to happen under current Tayloristic academic professional conditions.

So the final observation I would make is that the next step in moving toward a more vibrant field of organizational studies is to take on the analysis and reform of academic institutions themselves. It is not enough to ratify or vitalize the anthropology of organizations. Among the few remaining redoubts of extreme Taylorism, universities are internally structured to discourage collaborative, multidisciplinary research in the social sciences. Not only are turf wars in the way, but the entire administrative structure is a segmentary hierarchy with separate silos of meritocratic struggle held together by a dean's control over the budget and the provost, president, and board of trustee's control over the dean. Added to this is a national meritocratic system that ranks disciplinary departments by how well the faculty color within disciplinary lines and which ranks universities by the number of highly ranked departments they have as determined by publications in narrow professional journals, citation indices, and getting grants from the largely discipline-bound federal funders. Cautionary tales abound. Science and technology studies was once viewed as a multidisciplinary challenge to the hegemony of academic Taylorism in the service of major social issues. Now they have their own professional associations, journals, and self-contained departments in many universities, just like other departments. The similar domestication of what was once called Women's Studies is vividly described by Ellen Messer-Davidow (2002) in her *Disciplining Feminism.*

As a result of this history, we are now faced with a set of highly consolidated, antiquated, authoritarian university organizational structures, and a set of evaluative processes that reinforce those very constraints. Simultaneously, the public and policy makers claim universities ought to be producing meaningful research for governmental, public, and private use. The policy makers and their academic administrative counterparts instrumentalize these demands by imposing ranking and funding systems, and these ill-conceived ranking systems guarantee that relevant, multi-disciplinary research will not occur except against the grain of the institutions. That is, academic work outside of current disciplinary boundaries and published in journals outside of their professional associations countless or not at all in promotion and salary decisions. They also affect the collective ranking of departments negatively.

These kinds of organizational contradictions and dilemmas are too complex for any one field to master and even less to move aside. Together, as a multi-disciplinary group of scholars and organizational change practitioners, we now have the organizational research and change capabilities to take on such challenges directly, but only if anthropologists, sociologists, political scientists, economists, psychologists, planners, scholars of organizational behavior and business, and the other related applied fields join forces to recreate something of an authentic and engaged "social science." Continuing to operate in the paint-by-the-numbers schemes invented by industrial engineers and policed by academic and political accountants is intellectually incompetent and socially irresponsible, as the founders of this subject Morey and Luthans portray would have argued.

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Notes

1. The interest in the origins of organizational ethnography is now a crowded domain. Numerous fine accounts are available. A small sample would include: Baba (2006, 2012), Bate (1997), Burawoy (1979), Cefkin (2009), Schwartzman (1993) and Wright (1994). On the Hawthorne Studies, Gillespie (1991) is magisterial. On the contribution of anthropologist W. LloydWarner to the Hawthorne studies, Baba (2009) has much to say. Not to be missed either is the spritely – if snarky – treatment of Hawthorne (and the role of Elton Mayo) by Stewart (2009). I draw on all these works and join the crowd in my compressed commentary here.
2. I use the hyphenated term "quasi-ethnographic" to indicate that Warner's design (in consultation with Mayo and the rest of the research team) for the Bank Wiring Observation Room (BWOR) phase of the project was to place a more or less silent observer in a position to record the behavior of 14 workers on the job in conditions experimentally designed to replicate the normal, if artificially isolated, work environment. This non-participant observer produced meticulous behavioral records of who said what to whom, when, where, for how long, about what, in what tone (i.e. friendly, playful or antagonistic) and so forth and recorded production rates – rather like a time-and-motion industrial engineer only focusing on social interaction rather than physical movement. The "observer" was supplemented by an "interviewer" (a different research associate) outside the work setting who then interviewed the same workers repeatedly – following Mayo's non-directive clinical style – over the five month period of study about their work and their reactions to it. Warner visited Hawthorne periodically but was never directly involved in the study, leaving the data gathering process to hired hands who turned the data over to the HBS research team to pull out the "findings" and interpret them. This is hardly the lengthy and intensive "live in and live like" mode of ethnographic practice now associated with organizational ethnography.
3. Many of these contributions come from those only indirectly linked to the Hawthorne Experiments. In the 1940s, 1950s and early 1960s these contributors formed a loose network of applied anthropologists and published a good deal of ethnographically based research. They were based first at Harvard University (and at the time in contact with Mayo) and then moved on to the University of Chicago. Conrad Arensberg, Eliot Chapple, and Burleigh Gardner were among the most prominent (and prolific) members of this group. Several members of this group led by Gardner (and including Warner) established Social Research Incorporated (SRI) in Chicago, the first successful ethnographic consulting firm whose clients at various times included Coca Cola, Ford Motor Company, Sears and other high profile corporations. This venture was not so much business anthropology as an anthropology business. 4. William Foote Whyte explained to me how he and others at the Harvard Society of Fellows worked across all the social sciences and only eventually decided on a particular social science to take their doctorate in, or at least this is how I remember our conversations.
5. Ironically, those stimulated by the German example came back to the USA and created a mix between the OxBridge collegiate model and the German research university, claiming all the while to be implementing the German model. Now, German higher education policymakers want to create universities on the "American model," by which they really mean Harvard, Yale, Princeton, Stanford, etc., out of a German public higher education that lacks anything like the financial resources of those wealthy elite institutions. These confused borrowings back and forth across the Atlantic have led and are leading to very different end results.
6. Anthropology arose as a universalizing natural historical approach to the evolution, history, and cultures of all humankind, which accounts for its four-field structure (biological anthropology, archeology, linguistics, and cultural anthropology). By contrast, sociology arose from a concern with social order, social structure, and group processes and traces its antecedents quite differently. Both disciplines claim the likes of Weber and Durkheim but for quite different reasons.

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