

Medical Sociology as a Vocation

Journal of Health and Social Behavior
2014, Vol. 55(4) 375–385
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DOI: 10.1177/0022146514557131
jhsb.sagepub.com



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Abstract

This article extends Weber's discussion of science as a vocation by applying it to medical sociology. Having used qualitative methods for nearly 40 years to interpret problems of meaning as they arise in the context of health care, I describe how ethnography, in particular, and qualitative inquiry, more generally, may be used as a tool for understanding fundamental questions close to the heart but far from the mind of medical sociology. Such questions overlap with major policy questions such as how do we achieve a higher standard for quality of care and assure the safety of patients. Using my own research, I show how this engagement takes the form of showing how simple narratives of policy change fail to address the complexities of the problems that they are designed to remedy. I also attempt to explain how I balance objectivity with a commitment to creating a more equitable framework for health care.

Keywords

health care, medical sociology, qualitative methods, Reeder Award, vocation

Reeder Award talks appear in print as articles in the *Journal of Health and Social Behavior* a year or so after their initial performance. The article removes from the talk the context that allowed me the freedom to use the occasion to speak personally of the connection between my work as a medical sociologist and my life as a person. Producing a text from the talk shifts registers; the sacred is transformed into the mundane. The talk was prepared to meet the demands of a time and place; the article removes from the talk the context that made those words appropriate for the occasion. In what follows, there is a biographical frame of reference not typically associated with articles in professional journals.

My intent in this article, as in the talk that preceded it, is an explication of the title, "Medical Sociology as a Vocation." Given the rare opportunity to say whatever I wanted, I desired to talk about my work and career in a way that embodied whatever contribution I have made to medical sociology "distinguished" enough to be deserving of the Reeder Award. Moreover, at a time when market prospects for younger scholars are grim at best, I also wanted to remind us all that our collective

work is diverse, has value, is worth the effort, and most importantly, has a role to play in repairing a broken health care system.

These are lofty goals, easier to state than to achieve. Deeply held convictions, when expressed in journal articles that prize impersonality and the passive voice as markers of *professional style*, risk appearing overwrought and portentously self-important. Since I have spent the past 37 years both doing and teaching medical sociology as a qualitative researcher, I thought of the talk, then, and this article, now, as an opportunity for spelling out what has made my work meaningful enough to and for me to keep at it for so long, preoccupied with, but never quite answering, the same questions; for making clear how my approach to these questions has remained constant; and for pointing out how, paradoxically, that constant approach is also dynamic, always in the process of changing.

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The title of the Reeder talk was and subsequent article is intended to be evocative. I want to invoke the spirit of Weber's "Science as a Vocation," which was originally delivered as both an inspirational and aspirational speech to graduate students and younger scholars as well as a gloomy assessment of the current state of affairs in the academic world of the time (Gerth and Mills, 1946). Weber's is an essay that I teach often, admire in part, and am discomfited by in part. (I was always aware of the admiration; the discomfort became apparent writing this essay.) I admire the essay because better than any other text that I know, it describes the profound inability of science to make sense of, much less answer questions of substantive values. As Weber himself put it, "science can tell us what it is necessary to do if we wish to master life technically, it cannot tell us whether we wish to do so."

My discomfort with the essay is rooted in the sense that Weber is writing as if he were at war with himself. I find it difficult to read the impassioned rhetoric of Weber's essay and remember that he is presenting an argument for "the value neutrality" required to discover and reveal "inconvenient facts." There is something unsettling to me in Weber's impassioned call for dispassion. But perhaps what makes me most uncomfortable is how aware I am of fighting that same war with myself. Tensions among value-neutrality, objectivity, my subjectivity as an observer and the biases, conscious or unconscious, that creep into my work because of those biases remain constant concerns for me as I pursue "Medical Sociology as a Vocation."

I must admit that what having a vocation meant has remained for me somewhat mystical. A vocation, the term cannot be precisely translated from the German or easily separated from Weber's conception of "The Protestant Ethic," was some harmonic convergence of *zwecks* and *berufs* with *gemeinschafts* and *gesellschafts*. Medical Sociology has become my vocation even though I remain uncertain about both why and how that occurred as well as what, precisely, that means. Moreover, my understanding of what it means to possess a vocation continues to change and emerge over time.

Weber's essay suggests that there is only one way to acquire a scientific vocation: one hears a powerful inner voice with such a force that "here one stands and can do no other." The individual with a genuine "calling" to science as a vocation is unfit for any other occupation. Weber's assertion, that only those with a blinding passion for science can succeed, is another reason that the essay provokes discomfort in me. As a theoretical statement,

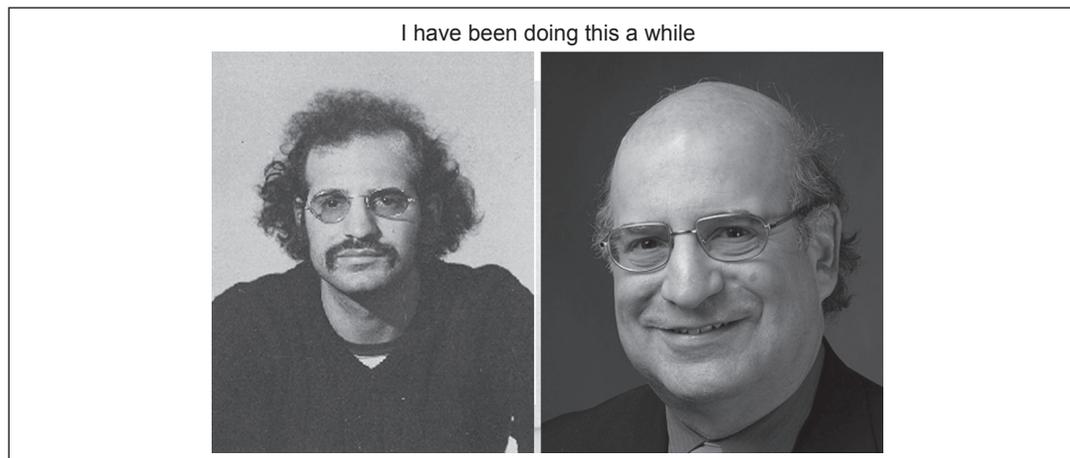
it fails to recognize the demonic power of science in a world that is no longer magical, but is rather, as Weber reminds us, disenchanted. As an empirical statement, it fails to acknowledge the shortsightedness that inheres in pursuing a blinding passion for science, unmindful of the all too foreseeable, undesirable consequences of some lines of inquiry (Kempner, Merz, and Bosk 2011). Walker Percy (1938:117) states the issue eloquently:

More distressing consequences occur when the zeal and excitement of the scientific community runs counter to the interests of the world community, e.g., when scientists at Los Alamos did not oppose the bomb drop over Hiroshima and Nagasaki. The joys of science and the joys of life, as humans, are not necessarily convergent. As Freeman Dyson put it, the "sin of the scientists at Los Alamos was not that they made the bomb, but that they enjoyed it so much."

The perversity inherent in this enjoyment—a pure expression of "science as a vocation"—is a point that Weber insufficiently appreciates.

Weber's essay also embeds a benign and malignant view of the scientist as a functionary in a university bureaucracy. In the benign view, the scientist is a paragon of "value-neutrality." True scientists are individuals whose disinterested approach to inquiry allows them to discover inconvenient facts that cut against their own self-interest. However, their commitment to science is so great that it impedes their ability to prosper in the bureaucratic environment of the university. Weber counsels that unless one is prepared to see mediocrity prosper, one ought not take up science as a vocation. This sentiment leads to a malignant view of academic hierarchies. Those who possess no genius or passion for science rise to the top of academic hierarchies and then use their professional authority to impose their *Weltanschauung*, stifling creative and alternative points of view.

Weber does not seem to recognize that in addition to following a powerful inner voice—a romantic view of everyday labor in an otherwise disenchanted world—there is another way to acquire a vocation. I have acquired mine in that other way. I drifted into medical sociology as a vocation (Matza 1969). Others labeled me a medical sociologist long before I came to think of myself as one. Over a course of years, without my initially setting out for it to be so, medical sociology, as a subdiscipline within sociology, became my *habitus*. It has become what I do, what I try to teach my graduate students to do, and



what I try to get skeptical outsiders to take seriously. I think that whether one is seized by a calling or drifts into it makes a great deal of difference to how one views one's labor as a vocation. Precisely spelling out that difference is a complex task.

Drifting Into a Vocation. Medical sociology is my vocation if only because it is the work that I have been doing every day for almost 40 years. I do that work in a very particular way. As a qualitative researcher, I watch people at work in hospitals and then I sit down with them and a digital recorder to ask them about what I observed. I write about what workers tell me, typically reframing their understanding of their work so that it adds to our stock of knowledge about professional behavior in highly regulated yet somehow still chaotic bureaucratic organizations. I write as well about both how to do this research and how new regimes of research regulation have impacted qualitative inquiry.

Qualitative inquiry is, of course, one of multiple ways to ask and answer questions about the nature of social life. However, as a mode of inquiry it is ideal for the type of questions that interest me most. These questions are the enduring conundrums of social life, what I learned to call as an undergraduate "the primary mysteries of the human condition." The hospital is a site in which such questions abound. There is birth and death; there is undeserved pain and suffering. In the hospital, all preexisting forms of social inequality are found in their most concentrated and toxic forms. If one is interested in exploring how social groups manage uncertainty, define acceptable risk, account for unexpected adversity, and rationalize why virtue is so often unrewarded while evil so often goes unpunished, a better site for inquiry than the hospital is difficult to imagine.

So, as I mentioned a few paragraphs earlier, I drifted into medical sociology as a vocation—and that having arrived at my vocation in this way rather than being driven there by some powerful "inner voice"—there are some irreducible differences between a Weberian sense of vocation and my own. First, in claiming medical sociology rather than science as my vocation, I am placing the constitutive accent on the topics that fall within the domain of a subdiscipline rather than a method of inquiry. Medical sociology includes, paraphrasing Geertz (1973), both deductive inquiries in search of, if not a law, a hidden structural regularity as well as interpretive ones in search of a meaning. I think of the search for those hidden structural regularities as the mind while the interpretive search for meaning as the heart of medical sociology. A robust medical sociology requires not only a heart and a mind, but also that both work together.

Further, in Weber's essay, there is a blind faith in value-neutrality that I can never bring myself to display in practice. Are not research questions a blend of facts and values impossible to disentangle? Is it even possible to form a value-neutral research question? For others, perhaps it is so; but for me, not so much. Finally, I believe that we need to be **involved** in how our research is interpreted and used in the delivery of care. We need to influence and talk to an audience larger than each other. We need to be at the table with policymakers, system administrators, and frontline workers. Being cited by other sociologists, however gratifying that might be, ought not be confused with having an impact in public arenas. For Weber, science as a vocation is an "ideal type," one that is conceived as separate from but has as its antithesis "politics as a vocation." In practice, of course, science and politics are interdependent. As a consequence, both physicians and sociologists who

study them are required to balance an ethic of responsibility with an ethic of ultimate ends.¹

Weber was certainly correct in stating that good work requires that researchers care about their work. But more, much more, is needed. When asked by skeptics—and there is no shortage of skeptics about the worth of sociological inquiry—we should be able to show both to ourselves and to others how our research makes a difference to the ways we feel, think, and act when ministering to the sick. We need to balance open-minded objectivity with a normative commitment to creating a more egalitarian society. We need to recognize it is not enough to analyze the world; we need a genuine commitment to figuring out how to fix what is broken in order to change it.

And there is so much that is broken. As social inequalities widen so do health disparities. We pay too much for costly interventions that provide too little benefit for too few. We manage pain and death better than we ever have, but we still manage both poorly. We have too many “preventable adverse events” despite heavy investments in interventions to reduce them. We need to develop better ways to deliver accessible, high-quality care, even as we continue to contest what quality is and how to measure it. We need to develop ways to provide patient-centered care at the moment that treatment becomes standardized and driven by evidence-based protocols.

I have wrestled long and hard with how to conduct research that satisfies the criteria for sociological work that demands to be taken seriously among my peers in the discipline and, at the same time, have that research engage an audience of policy-makers outside of sociology. When I began to pursue qualitative research in medical sociology, I believed that a Weberian commitment to value-neutrality was all that I needed to accomplish both ends for three reasons. First, I thought this stance was necessary to negotiate a domain as filled with conflicting interests and actors as medicine. Second, I thought refraining from overly prescriptive statements was a way to remain open-minded. I believe with research that it is not only better, but also more convincing, to show rather than to tell. Third, I felt skilled at social diagnosis and utterly unprepared for social therapeutics.

I felt that fixing the broken parts of the social organization of health care was well outside of my zone of expertise. Over time, I have come to see that many of those who possessed certainty about how to fix the broken parts of the health care delivery system would be better served if they possessed more humility, less swaggering confidence in the rightness of their prescriptions, and a greater ability

for some self-reflection. I have come to see my uncertainty about how to fix what is broken in the world of health care as a strength and not a weakness. I have come to believe that the certainty of those who propose solutions is in itself dangerous and that certainty needs consistently to be subjected to the skeptical questioning provided by empirical studies of how abstract policy is implemented on the ground.

Finally, there is one more critical reason for me to urge an approach aimed at influencing policy-makers: namely, this is a critical time for health care. In the United States, we are in the midst of the largest overhaul in the financing of health care since the advent of Medicare. The financing reforms will inevitably lead to changes in how care is organized. These changes include, but are not limited to: new mechanisms for accountability, including public reporting of outcomes; greater standardization of treatment based on evidence-based guidelines and organizational protocols; new institutions for the delivery of primary care; new strains and stresses on the capacity of the delivery system as those without access to health care providers enter the system; and new pressures to constrain costs while improving quality.

In a certain sense, this last reason for involvement—crisis and change—is not as compelling as it might first appear. During the nearly 40 years that I have been studying the medical profession and the hospital as a social organization, massive change has been a constant and the times have always been critical. The words *crisis* and *health care* have been paired with great frequency and applied to a wide range of substantive topics.

During the time that I have been a medical sociologist, I cannot remember a time when the increasing cost of care or limited access to care in poor urban or rural areas has not been an issue. Malpractice, the need for tort reform, and the excessive costs of defensive medicine have also been constants. Another constant has been providers' complaints about excessive regulation while patient advocates have voiced similar concerns about lax or ineffective regulation. Concerns about the high medical costs incurred at the end of life as well as the increased costs of an aging population are yet other concerns that have been constants. Moreover with climate change, antibiotic-resistant bacteria, environmental toxins, the legacy costs of past and current wars, as well as potential viral pandemics, we add new crises to what was already a long enough list. At the same time, the discourse on health care has radically altered. We now talk about

health care as a commodity purchased in a competitive marketplace rather than as a public good. As sociologists, we have an obligation to highlight those patient populations whom market solutions are likely to serve poorly. We also need to remind policymakers that there are alternatives to viewing health care as a marketplace commodity like any other.

Weber's solution to how we deploy expert knowledge while preserving democratic and bureaucratically equalitarian values—namely, that one cleaves the self in such a way that one is an expert in dialogue with other experts but claims no special insight when speaking in public arenas—does not seem sensible to me. In fact, one reason I feel obligated to speak out in favor of some policies or against others is because of my experience as a researcher. To pretend otherwise is a willful denial of the obvious, an exercise in self-delusion that is not likely to be taken seriously.

Tikkun Olam, or "Repairing the World". How do I reconcile an engagement with policymakers with an equally strong desire to continue to do work that represents as neutrally and fairly as possible the voices of the multiple stakeholders in the socially fraught situations that I find myself drawn to studying?

My original intention in becoming a sociologist was to put myself in a position to be a change agent. My first published paper (Bosk 1974) discusses how Hasidism, a seventeenth-century messianic movement in Judaism, managed to survive to the present day despite the failure of the Messiah to arrive. The major finding of that paper was that that survival rested on a novel re-interpretation of the creation myth of a twelfth-century Kabbalist, Isaac Ben Luria.

In this tale, after the world was created, God fashioned seven vessels to hold each of the divinity's essential qualities. When these divine qualities were emptied into the vessels, they proved to be so powerful that the vessels shattered and the sparks of the divinity's essence fell to the earth. Hasidim divided the divine sparks into two types: those that were general and those that were personal. Each person has a duty to return both the general and the personal bits of fallen divinity to its rightful sphere. This act of returning divine sparks was known as *tikkun olam*, or "repairing the world." I found then and still find today the notion of an ethical obligation to repair the broken parts of our world and of myself appealing. Picking up one's personal sparks as well as the ones that were both self-evident and collective

was seen as acts that one should engage in both while waiting for and to hasten Messianic redemption. For a very long time, I thought responsible inquiry that presented inconvenient facts about how we created self-serving rationales for our actions was sufficient to fulfill the obligations embedded in *tikkun olam*.

There were many reasons that I had for this belief. Chief among these was the way my graduate training divided intellectual labor. The empirical world—the world of *is*—was the province of sociologists and other social scientists. The normative world—the world of *ought*—was the province of others, mostly philosophers and theologians. Every time I used the verb form *ought* or *should*, whatever faculty member was critiquing my work would underline the word and scribble some sarcastic, demeaning, or ego-lacerating remark in the margin. I learned to avoid using these prescriptive verb forms. Looking back, I see that I overlearned a classic false dichotomy.

But then that was not, nor is it now, the whole story. I was, and am still, aware of how much of sociology wears its normative convictions on its sleeve. Too often, critiques of medicine that I read—especially during graduate school when sociologists were enamored with the idea of *professional dominance* and prone to essentializing and totalizing medical care as biomedicine—bore too little resemblance to the medicine that I saw practiced. As a result, those stakeholders, who these critiques were written to influence, summarily dismissed them. Playing my normative cards close to my vest seemed to me a reasonable strategy for being taken seriously in domains outside of sociology. For the purposes of getting on with this paper, let me add one final reason that I shied away from a too direct engagement with policy: I feared—and to some extent still fear—that advocates for this or that position become enamored of the policies for which they advocate. As a result, they become intellectually less flexible. Grinding it constantly against the same stone, their sociological imagination gets dulled. I feared that fate for myself. But these are all beliefs that I now find neutralized by the countervailing argument: health care is too broken for us to grumble among ourselves about its parlous state—we need to add our voices to economists, ethicists, clinical epidemiologists, and others trying to improve how care is delivered.

So how do I square the circle? How do I hold on to the idea that I can provide reliable accounts of social process, advocate for policies that I believe in, and stay open to the possibility that I may be

wrong? Methodological consistency is one technique that I deploy for trying to remain open to the myriad ways that behavior in the empirical world might not align with the theories of behavior that I favor. To borrow once again from Geertz (2000), I use four questions to guide inquiry: (1) Who are these people? (2) What do they think they are doing? (3) To what end are they doing whatever it is that they are doing? and (4) Within what frame of meaning does this self-description make sense? Keeping these questions in mind accomplishes two goals: first, it allows me to show rather than tell others the data for the positions that I argue for or against; and second, it allows me to see where my arguments are thinly supported at the same time that it allows others to make up their own minds based on data that I present to support those arguments.

Working among health service researchers, I often encounter resistance to having observational data being considered valid data. Is it representative? Is it generalizable? Is it actionable? Very often these are the wrong questions to ask. No field site is representative or generalizable. Nor is this a problem that can be cured by multiplying sites or ramping up the number of respondents to interviews. The knowledge sought is of a different order. Observational data are invaluable for explaining the gap between the formal plan and its execution in practice. I have done more than enough telling and far too little showing in this article so far. The time has come to provide from my own work some empirical support for the linked arguments: in qualitative research objectivity is an aspirational ideal, the goal of this attempt at objectivity is to provide a reliable account of meaningful behavior, that reliable accounts need to engage policy questions, and that qualitative research engages policy by showing how much more complex and messier the empirical world is than the rational ordered schemes much policy presupposes.

In the Beginning, I Watched Surgeons. Describing the part of my approach that has remained steadfast is simple. I believe as fervently today as I did when I wrote these words for what later became *Forgive and Remember* (Bosk [1979] 2003:5–6):

Any programmatic change which intends to make professionals more accountable must of necessity start with a complex phenomenological understanding of what currently passes for accountability and how it is achieved. Field research such as this informs policy by grounding it in a firm understanding of how participants

construct their social worlds. It is only from this concrete understanding of the present, practical order that any changes in the existing interactional politics of social control.

Even though my belief that in order to understand social life one must observe it as it occurs remains unchanged, over time my appreciation of the difficulty of “providing a complex phenomenological understanding” that meets the contingencies of any moment has grown.

I can think of any number of reasons for this. One is as the stock of my experiences grew, my conception of who was a “participant” expanded and that expansion made attempts at a “concrete” understanding of “the existing interactional politics of social control” harder to capture. My tunnel vision, when I began work in what became my calling, is understandable. I was young and inexperienced. I was observing social control in a rather small group—a surgical training program at an elite academic institution.

There was not much reason for me to think about what I was doing other than “watching surgeons provide accounts for deaths and complications that were expected or were an unpleasant surprise.” When I did the fieldwork for *Forgive and Remember*, the management of surgical practice was a private professional matter that occurred largely at the unit level—units could be individuals, practice groups of surgeons, or hospital Departments of Surgery. There was no mandate for the public reporting of outcome data. Audits of performance were seen as an undertaking, the inherent complexity of which made it not worth even attempting. Ethical questions were seen as issues that “an attending alone must decide” either after consulting with the patient and their family or not, depending on the attending.

As I described them, social controls in surgery were largely an internal and informal matter. This is, of course, no longer the case. We now have publicly reported measures on accessible websites. The patient safety and quality improvement initiatives within medicine have reduced the tolerance for what used to be considered “normal complications” of surgery.² Since the original publication of *Forgive and Remember*, professional regulatory bodies, audits of risk-adjusted outcomes as measures of performance, and financial punishment attached to “preventable adverse events during an episode of care” have all grown exponentially. And while I am far from certain that all the changes are beneficial or that a book first published in 1979 had anything to

do with increased public attention to patient safety, I do like to think that *Forgive and Remember* played a small role in calling both professional and public attention to how slipshod and unsystematic the accounting for deaths and complication was in surgery. The central paradox of the system of social control that I described was that the individual expectations for performance set during training were exacting while the concern of the collectivity of surgeons that those exacting standards were met once training was complete was virtually absent. Scandals around performance monitoring—for example, the perplexing inability to identify and bring to justice physicians such as Harold Shipman or Michael Swango who were also prolific serial killers—served to disable arguments that regulatory standards of the profession alone protected the public welfare (Dixon-Woods, Yeo, and Bosk 2011; Stewart 2000)

THE NEXT STEP: FROM WATCHING DOCTORS TO WITNESSING HEALTH CARE

My next foray into the field took me into social settings more complex by far than anything that I observed watching surgeons. I wandered into the world of genetic counseling and decision making in the Pediatric Intensive Care Unit. By wandered, I mean to say that I was “invited” to do research in settings where the patient or family were more likely than not going to be a more active “participant” in any medical encounter than a surgical patient “aetherized on a table.” Unlike my fieldwork among surgeons, the invitation and the initial observation came before I had a well-formulated research question. Second, while studying genetic counselors and decision making in the Pediatric Intensive Care Unit, new actors were added to the decision-making units that I studied: “bioethicists”—drawn from medicine, nursing, theology, law and philosophy; hospital administrators concerned with costs, risks, and benefits as well as adverse publicity; and multiple advocacy groups who spoke for patients’ interests in ways that were frequently contradictory. This is a shorthanded, if longwinded, way to say that the world that I was observing changed around me. Given how dynamic the delivery of health care is, as well as how narrow my previous exposure to it had been, and how different the problems that were confronting front-line personnel were from the questions that make up the “normal troubles of surgeons,” I do not find it hard to explain why the tone of my

writing changed. What is more problematic is explaining exactly what changed and how.

I have tried elsewhere to describe how my own definition of my role as a qualitative researcher—my warrant for being there—changed when the objects and subjects of my inquiry were no longer surgeons but became genetic counselors and their clients; specialists in pediatric intensive care, their intubated patients, and their worried parents; and all the new experts that David Rothman (1991) identified as “Strangers at the Bedside.” In a word, I began to think of my research activity less as merely “watching surgeons” and more as “witnessing the delivery of health care” in settings in which decisions were made in time-pressured situations under conditions of unresolvable uncertainty. How to describe the difference?

As both a watcher and a witness, I remained committed to something that resembled objectivity as I described what I watched. At the very moment that I was clinging to a belief in objective description, a self-reflexive turn among many anthropologists and more than a few sociologists questioned whether such an attempt was one worth pursuing. Was not the claim to objectivity a claim to privilege the voice of the expert in describing what is “really real” (Kleinman 2006)? And is not “really real” in the life-world of the patient and their family system very different from the life world of the physician-scientist?

Two questions remain before we get to end. First, what is the difference between watching and witnessing, and second how does the sort of witnessing that I engage in help repair the world? I think the difference lies in the activities I was watching, who was involved, what kind of issues were being discussed, what parties had a stake in the outcome, and what responsibilities I felt as a researcher watching as the private troubles of other became public issues.³

As I stated previously, when I was observing surgeons, I barged into a closed system. How I understood that closed system depended almost entirely on my willingness to be present. I placed a boundary on the times and places that I could learn about the socialization of surgical residents: work in the hospital was permissible; play outside was not. Going for beers after a shift was a terrific way to gather juicy data, but it also gave the impression that I was a friend, not a researcher. I was interested in how attending surgeons held each other and residents accountable for deaths and complications. I had a well-defined sociological question in hand and a “bounded whole” to study. What more could a qualitative researcher ask for?

The nature of the problems that those who received genetic counseling and those who provided it or those who were on vents in the pediatric intensive care unit, their parents or legal guardians and the team of providers who cared for them including physicians identifying themselves as specialists in intensive care were much more public than the private issues that surrounded the social controls operative in surgical training. In part because the problems were more public, I went from a hermetically sealed operating room to very large conference rooms with very heated debate among multiple stakeholders. Among those stakeholders were patients and their families, treating physicians from multiple specialties, patient advocates, hospital attorneys, law enforcement officials, private attorneys representing families, theologians, and self-identified experts in bioethics.

When I studied surgeons, my opinion was rarely solicited, and even when solicited, my opinion was never given much weight. As a rule, surgeons knew what they were doing; and even if they did not, they knew better than to ask a sociologist for advice. This was not so for any of the research I did from the late '70s through the mid '90s. Much to my irritation, I was asked frequently and by a variety of stakeholders: "What would you do?" (Bosk 2008). The question, whether it was asked in a public setting or a more private one, never failed to upset me.

The issue was not that I did not have strong opinions about what should be done; sometimes I did while at other times I was as bewildered as anybody else. What I did not feel I owned was a warrant to speak. I was in a position to be asked "What would you do?" because I was observing whatever was happening as a qualitative sociologist.

My recommendations had little to do with sociological expertise. Sociology is a generalizing craft—we are masters of the scatter plot, diviners of the frequency distribution. But, "What would you do?" is a question that calls for an answer about how to act in a situation. There I felt at sea. I concluded *All God's Mistakes* (Bosk 1992) with a promise that whatever projects I undertook in the future, I would never again be a mute observer.

WITNESSING, EXPERIENCING, AND HUMBLY SUGGESTING

When I first began to do research, I thought my vocation as a medical sociologist entailed the close observation of work in clinical settings. Whenever I thought self-consciously about what I was doing and its place in a sociological tradition, I always

thought of the industrial sociology that looked at interpersonal relations and production processes on the shop floor. At the same time, I recognized that the shop floor that I was looking at very much resembled a total institution for the resident physicians, with whom I was hanging around.⁴ This twin conceit helped me fashion the narrative structure of *Forgive and Remember*. I could describe resident socialization without having to reference any group other than members of the Department of Surgery at Pacific Hospital.

As a qualitative researcher studying the delivery of services by genetic counselors or the care to patients in the Pediatric Care Unit, the strategy of focusing on the workers that I was observing as a discrete bounded whole no longer served me well. First of all, the work of the physicians who I was observing—genetic counselors and attending physicians, fellows and residents in a Pediatric Intensive Care Unit—was impossible to describe without referencing the wider world. The boundaries of each unit were permeable. Technological breakthroughs changed diagnostic and therapeutic capacities; changed diagnostic and therapeutic capacities raised questions about those occasions when life could be mastered technologically, whether it was worth doing so that were well beyond the dilemmas of Weber's "mortally ill man" in "Science as a Vocation" (Gerth and Mills, 1946:144).

Advancing technology and enhanced clinical capacities turned what had once been hypothetical into practical dilemmas. As these new dilemmas presented themselves, organizations needed to develop procedures so that unprecedented clinical dilemmas not derail the ability of the organization to function. All the techniques that had worked for me before gathering data were now useless. It was impossible to imagine that the work and the workers whom I was observing were capable of being described as if they were some isolated group of natives cut off from the wider world. This was so because that wider world had entered the clinic through media accounts; through highly publicized controversies, both local and national; and because of the rifts, tensions, anxieties, and emotional distress disputes within the organization created. I had always been taught—and fervently believed—that qualitative researchers need not worry about observer effect; that after an initial period of getting adjusted to the presence of the researcher, workers reverted to established routines. In the situations I found myself observing this was no longer so. While my workers had established routines for typical work, most of their energies were consumed by atypical situations

without fixed routines. During my research, I was asked with a frequency I always found annoying: “What would you do?”

Under the circumstances, conducting research while attempting to minimize observer effect was frustrating. I felt that what was demanded of myself as a professional contradicted what was demanded of myself as a fellow human being alongside other human beings who were facing imponderable existential questions of meaning. So the split I called attention to in the beginning of this essay—my role as a professional with a vocation and my life as a human being—needed resolution if I were to continue doing research.

Embracing the idea that observer effects were inevitable and that perhaps I could use them to improve the delivery of care was both a way out of the intellectual corner into which I had painted myself and, at the same, very different from the “fly on the wall” researcher that I had once been. All that I lacked in my attempt to resolve the split between the disinterested observer doing the research from the human being doing the same was an opportunity to practice “qualitative research in action.”⁵ One soon appeared.

The Institute of Medicine report on “preventable adverse events” has created in its wake a vigorous quality improvement and patient safety movement in health care. There is consensus that the quality of care needs to be improved and steps need to be taken so that patients are safer—less prone to suffer preventable harm—during hospitalizations. Unfortunately, the consensus about goals for the delivery system is not matched by a similar agreement on how to achieve those goals.

Let us take “checklists” for example. There is some controversy over their use as a tool for quality improvement. On the one hand, there are those who think of checklists as self-executing tools for improvement (Gawande 2009). On the other, there are those (and here I include myself) who believe the issue is more complicated—a lot of adaptive work needs to be done to change workplace culture so that frontline workers believe that checklists lead to improvements in outcomes (Bosk et al. 2009; Dixon-Woods et al. 2011).

Behind the question of how checklists work is, of course, a much larger question: How does one overcome the barriers to change and improve the quality of care in hospitals. And behind these questions lurk others: How does one measure quality? How much trust should we place in existing metrics for quality? What dimensions of experience do they give too much weight? Which dimensions of

experience do they ignore? How ought the delivery system reward improved quality? Ought the delivery system even reward improved quality? Why not simply expect or demand it?

These are the type of questions that I have been researching since 2011 as part of an AHRQ funded contract CUSP for Safer Surgery.⁶ The specific goals of the contract are: (1) “to reduce Surgical Site Infections and other complications of surgery” and (2) to “improve the culture of safety within surgery.” The first of those goals is measurable; the second not so much despite a variety of tools for assessing safety culture within an organization and units of that organization. I am one of three co-principal investigators on this project. The other two are Peter Pronovost, MD, director of the Armstrong Institute for Quality and Safety, and Clifford Ko, MD, director of the American College of Surgeons’ National Surgical Quality Improvement Project. My postdocs and I are referred to as the “Ethnography Team” despite the fact that our work does not fit any traditional definition of ethnography.

We have spent a considerable amount of time at the Armstrong Institute observing how a national quality improvement program is assembled and modified as it goes into the field. The CUSP for Safer Surgery has enrolled approximately 250 sites from 37 states, Canada, and the United Kingdom. The sites are organized into five cohorts. We have visited over 20 sites twice a year apart to observe how the program as designed is implemented on the ground. We are not merely observers; we are active participants. During the design phase, we listened to disagreements among members of the project team over how to present the elements of the project, what data to collect, and how. We raised questions and provided suggestions during planning sessions. Once in the field, we played multiple roles as we gathered data at sites—we gave advice when asked about how to engage workers, how to organize teams, and how to organize the projects. When we returned from the field, we offered suggestions based on our observations for improving the delivery of the project’s content.

I suppose all this is a way of saying this ethnographic fly has flown from the wall to the conference table. I think of this as a natural progression. As I have pursued my vocation, I found a way to have a voice. At the same time, I remain engaged in some very basic sociological questions. These may broadly be conceived as “Quality Improvement as a Collective Enterprise.” I have both practical and theoretical goals for this research. This is one of the most comprehensive studies of a quality improvement initiative

to date. I hope it makes a contribution to the growing inter- and multidisciplinary niche of “implementation science.” Three specific contributions come to mind. First, we hope to identify barriers to change and the strategies successful sites use to overcome them. Second, we hope to identify the areas of cooperation and conflict among all the different stakeholders involved in quality improvement initiatives. Third, we hope to show measures of quality changes expectations, redefine roles, and redistributes responsibility for improving quality.

These are, of course, questions of fundamental interest not just for medical sociologists, but also for the discipline itself. In studying a quality improvement initiative, we learn about how organizational change takes place; we learn about the limits of standardization in care work on unique individuals; we learn about how managerial regimes change the nature and meaning of work; we learn about how audit trails create the appearance of transparency; we learn about how organizational practices vary and how they are isomorphic; and we learn about how context matters in accomplishing organizational goals.

CONCLUSION

At the beginning of this essay I promised to discuss how my vocation as a medical sociologist has remained constant and how it has changed. What has remained constant is my commitment to qualitative research as a way to get at issues that form not just the heart of medical sociology but all inquiry into social life: How do we make sense of experience? How do we manage risk and uncertainty? How do we explain unexpected failures? How do we explain the inequitable distribution of good health, wealth, and emotional well-being?

I have remained committed to creating reliable accounts although I am more cautious about describing those accounts as “objective.” Qualitative research methods are dynamic and capture emergent meanings. My own understanding of my vocation as a medical sociologist has likewise been dynamic and emergent. I have moved from simply watching doctors to witnessing health care to participating in projects designed to reduce preventable adverse events. As I look back, I think my methods have changed much less than the domain that I studied. Social controls have in medicine moved from a private professional matter to one that is much more public—one that is subject to audit, public “report cards,” and media account. As this has occurred, the social world that I was studying changed around me, I tried

to adapt by developing a greater sensitivity to the complexity of that world. And, like Weber, when observing a group struggle with ultimate existential issues, I tried to always approach both their struggles and those struggling with empathy and humility.

ACKNOWLEDGMENTS

I would like to thank Elizabeth Armstrong, Abigail Bosk, Emily Bosk, Marjorie Bosk, Joanna Veazey Brooks, Ray De Vries, Ksenia Gorbenko, Daniel Menchik, Mark Neuman, and Ethan Schoolman for support help and support. I would like to thank the Robert Wood Johnson for a Health Investigator Award that reconnected me with issues surrounding patient safety. I would also like to acknowledge support from the Agency for Health Research and Quality.

NOTES

1. I am thankful to Mark Jacobs for pointing this out.
2. The concept “normal accident” needs to be expanded to include normal errors—in medicine “normal errors.” Physicians recognize this when they refer to a patient or a patient’s treatment as a “train wreck.”
3. The echo of C. Wright Mills’s (1959) *The Sociological Imagination* is intentional.
4. To the best of my knowledge, Don Light was the first sociologist to compare sociologists to inmates of a total institution.
5. The echo of LaTour’s (1987) *Science in Action* is intentional.
6. I am not working alone, of course. I have been ably assisted by Joanna Veazey Brooks, Ksenia Gorbenko, and Catherine Van de Ruit.

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