

*Keynote Address:*

*First Congress of Qualitative Inquiry*

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## The Politics of Evidence

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*The evidenced-based movement has influenced medical research to the point that the agendas and methods of qualitative inquiry are often excluded from resources of medical granting agencies. Yet, its narrow definition of what constitutes evidence and its myopic vision about health must be challenged. In this article, the author argues that qualitative research does contribute to a reduction in morbidity and mortality without the cost in dollars and lives that are necessarily incurred in evidence-based inquiry. She asserts that we must reframe our definition of evidence to meet this new ethic of inquiry—which she calls the ultimate ethic—as a way of conducting research.*

**Keywords:** *qualitative evidence; types of evidence; ethics*

**T**he title that I have used, “The Politics of Evidence,” is an oxymoron: *Evidence* is something that is concrete and indisputable, whereas *politics* refers to “activities concerned with the acquisition or exercise of authority” (Abate, 1996, p. 1152) and is necessarily ephemeral and subjective.

This evening, I will examine how the politics of evidence, the politics of ignorance, stigma, and conflicting agendas (which extend to academic and governmental levels) are impediments to health research—and perhaps also educational research—and constrict qualitative inquiry. This oppressive movement is impeding how, when, and to whom qualitative inquiry is taught, contracted, funded, conducted, published, read, and implemented. I will argue that the long debate over the qualitative/quantitative paradigm issues has now gone beyond preferences for a style of approaching research and has become a more serious concern. And this issue is one of the primary agendas of this conference.

[This] political economy of evidence . . . is not a question of evidence *or* no evidence, but who controls the definition of evidence and which kind is acceptable to whom. (Larner, 2004, p. 20, emphasis in original)

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## THIS IS THE STORY

A model to evaluate research rigor was introduced in 1972 by Archie Cochrane (1972/1989). In his publication *Effectiveness and Efficiency*, he recommended standards for medical research. This model placed randomized control trials as the gold standard for evidence and mere opinion at the lowest level. The model, intended as criteria for the evaluation of drug trials to determine treatment efficacy, has been embraced in medicine as a new standard. The *standards for quality of evidence* were classified by Sackett (1993) as . . .

*Grade A.* Randomized trials with low false positive (alpha) and/or low false negative (beta) errors supported by at least one (preferably more) Level 1 randomized trial.

*Grade B.* Supported by at least one (preferably more) small randomized trials with high false positive (alpha) and/or high false negative (beta) errors. These are usually inadequate for implementation, but meta-analytic techniques used to analyze the results of 2 or more trials may obtain statistically significant results to move these into Level 1.

*Grade C.* These consist of *Level 3*, nonrandomized two-group concurrent cohort comparison (historical control or another site as comparison); *Level 4*, with no comparison group; and *Level 5*, opinions of expert committees. Grade C is *not* recommended to inform practice.

### Qualitative inquiry is classified as Grade C.

*Evidence* and *evidence-based practice* have become the new mantra for medical care; it has spawned meta-analyses and the Cochrane Library: a depository of these reviews assessing evidence by evaluating series of trials—or replications. This new agenda has resulted in conferences, societies, journals, and databases, and in new approaches to care—evidence-based practice—in which modifications of therapy are made based on its recommendations.

Despite its main criticisms—basically, that the trial conditions are not replicable in day-to-day clinical care and the trial's mean score does little to inform the individual case (Kravitz, Duan, & Braslow, 2004)—evidence-based practice is a trend that is here to stay.

### WHERE DOES THIS LEAVE QUALITATIVE INQUIRY?

Obviously, qualitative inquiry is a poor fit, for it has never purported to be a method that can be used to evaluate the efficacy of drugs or other treatments. But rather than excluding qualitative inquiry from the Cochrane criteria because of the nature of qualitative data, (textual, interviews, conversation, observations), qualitative research was immediately classified as “mere opinion,” as Grade C, the lowest level of evidence, not recommended for implementation.<sup>1</sup> Thus, perceived as clinically

useless for their agenda of treatment efficacy, it was not taken seriously nor given any credence, until recently.

But my major concern is that disciplines that were the primary users of qualitative inquiry with a mandate for health **were virtually excluded from the resources provided for medical research**. That is, research from disciplines such as nursing, rehabilitation, occupational therapy, counseling, social work, and the humanistic specialties in medicine, such as family practice and psychiatry, became less credible. Specialties that were not primarily concerned with drug therapy, or that extended beyond this—specialties that valued the *art* of care as well as the science of care, disciplines that were primarily concerned with relationships, interactions, and the context of care—these all slipped from the priority list of medical funding agencies.

The immediate *political* response of many medical granting agencies was to adopt the Cochrane criteria as the standards for evaluating *all* research proposals and, hence, for allocating research funding. In foundations and granting agencies that applied the Cochrane criteria *carte blanche*, this meant that qualitative research was not considered fundable.

In the United States, nurses had their own funding source in the National Institutes of Health (NIH) NCNR (National Center for Nursing Research), now NINR (The National Institute for Nursing Research), and NIDA (National Institute on Drug Abuse) and were less affected by this trend. But in Canada, Australia, and New Zealand (and, probably, the United Kingdom), where the control of health care funding was in the hands of medical researchers, this trend was crippling.

Federal organizations and private foundations that fund research are excellent mirrors of the state of the science. Their health care priorities and the grants that they fund reveal what research is perceived to be necessary and of outstanding design. Requests for data to both NIH and to CIHR for the percentage of qualitative grants received and funded have bought the response that they do not keep such statistics.<sup>2</sup> Yet, in the United States, since the 1980s, there has been consistent lobbying by groups of nurses to increase the number of qualitative members on the committees or to establish special committees with the expertise to review qualitative applications. The agencies' response that other methods do not have special review groups is not exactly correct. Presently, the review groups are predominantly quantitative by default. For instance, the review groups in nutrition understand and respect the methods used by nutritionists, or in engineering, methods used by engineers. There are two problems with the committee structure in NIH for qualitative researchers to get funding. First, the committees are predominantly quantitative, with token minority qualitative members. Second, is the preference that most committee members should have NIH funding, although this is not a requirement. Such a practice perpetuates the status quo and makes it difficult for enough qualitative researchers to establish the track records necessary to "break in." In all fairness, if the committee does not have the qualitative expertise needed within the committee membership, another reviewer may be brought in, in person or by phone. But the *disadvantage* of this system is clear. How can one unknown outsider sway a committee (in which friendships and power relations are already established) and, if on the phone, without even the advantages of a personal presence? And it is worse in Canada (from my experience as a reviewer in New Zealand, Australia, Great Britain, and South Africa), where only the external reviewer's written report is considered by the committee, and the "expert" does not have the privilege of listening to

and participating in the committee's discussion and debate, nor of voting. These problems continue to this day.

The first response of the qualitative researchers to this inequity in funding in the 1980s and 1990s was an immediate appeal, but their voices were soft. Criticisms were often published in sources that were not on the reading lists of the policy makers. Furthermore, because research is squeezed into most medical curricula—and qualitative inquiry is not usually included in the syllabus—these appeals came from nursing, occupational therapy, speech therapy, and counseling, these being disciplines that were tangential to the mission of the medical foundations, disciplines that were focused on the *person* rather than the therapy and which were not funded as mainstream priorities by the medical review boards. The objections came from stakeholders who were perceived to be outside the core mission of the review boards, so that these complaints were of little concern to the councils approving the funding decisions. And the appeals came over the heads of quantitative health researchers (mainly experimental psychologists and epidemiologists), who were better established, and better funded, and who probably agreed with the medical scientists and decision makers.

The second response of qualitative researchers (“If you can’t beat ‘em, join ‘em”) was to become a part of the Cochrane movement. A large group of qualitative researchers formed a group that met regularly, with the agenda of including “contextual evidence” in the reviews. Their initial task was methodological—that is, to determine how one should incorporate qualitative findings into the quantitative reviews. Previously I wrote,

I am now suggesting that it is time to be honest with ourselves. The assumptions underlying evidence-based medicine are a poor fit with the assumptions of qualitative inquiry. Furthermore, we have contrary research agendas: Whereas the epidemiological and experimental designs for clinical drug trials seek to decontextualize, qualitative research asks them to consider the context. We have different definitions and agendas for “providing care”: their focus is on the pill and if it works; our focus is different—why patients might decide whether to swallow the pill or to accept, reject, or modify the prescribed treatment, or how it affects patients’ lives. Both perspectives are equally important for “efficacy” but produce complementary information rather than information that may be incorporated into the same reviews. (Morse, 2005, p. 3)

Despite these words, I now believe that this group *has* made headway in raising the consciousness and status of qualitative inquiry. They are making a difference in the way qualitative inquiry is perceived. Some medical journals, such as the *BMJ*, now routinely have a qualitative section, perhaps due, in part, to this lobby. But this is a “side effect” of their work.

The final response was methodological—to develop methods for conducting qualitative meta-analyses. This task itself was onerous, because, of course, textual data are not additive; qualitative studies do not intentionally replicate, and so forth. Following meta-ethnography (Noblit, 1984), the effort was spearheaded by Margarete Sandelowski and her colleagues, and is still ongoing. The primary approach followed quantitative meta-analyses: Select pertinent studies, critique these studies according to identified standards (Sandelowski & Barroso, 2002), and then use some technique to develop a model according to the major theoretical commonalities (Thorne, Jensen, Kearney, Noblit, & Sandelowski, 2004).

So that is the story to date. But from my perspective, now, in 2005, this *emphasis on evidence* is not going to go away. Indeed, for those of you who are heaving a sigh of relief that you do not do health research or are thanking your lucky stars that you are not entrenched in a health care discipline, my prediction is that, as it has spread from Britain, to Canada and Australia, and then to the United States, so it will spread laterally, invading other applied disciplines, in particular, education. Politically, qualitative researchers are in for a long and rough ride.

I believe the problem that we face is not with qualitative inquiry itself but with mainstream medicine's entrapment in clinical drug trials, experimental design, and quantitative analysis—in other words, their perception of their discipline as a biological/physiological rather than a humanistic discipline. I maintain that despite the recent rise of humanistic medicine, their unwavering adherence to randomized drug trials, epidemiological designs, and insistence on the criteria outlined by Cochrane (1972/1989) as *The Standard*, and fueled by the pharmaceutical industry, is actually occurring in health care at the expense of more significance advances—advances that may make even greater contributions in reducing morbidity and mortality. I maintain that medical granting agencies' limited support for qualitative inquiry, and their limitations in acknowledging, respecting, and funding qualitative modes of inquiry and accepting alternate evidences, will impede advances in health care (Morse, 2006).

Two factors are driving the health care/medical agenda, and these factors cannot be ignored. The first is that the public lobby for cure is stronger and louder than the public lobby for care. Medical research is expected to reduce morbidity and mortality. The second factor is the political and public lobby of reduced costs, that is, efficiency in care. Medical care is becoming too expensive. These questions are interrelated. Let us ask instead . . .

## DOES QUALITATIVE RESEARCH SAVE LIVES?

When I look at the outcomes of most publications in *Qualitative Health Research*, authors claim that their research creates models for practice that “provide insight and understanding” into the experience of patients, families, and caregivers. It is important to be understanding, but in the hardball of everyday life, our “soft” research, with such nebulous outcomes, is not useful to policy planners and those responsible for the health of the nation. From this perspective, our research is not directly relevant to our health care agenda, which is intent on reducing mortality, lowering morbidity, and reducing costs.

Ironically, medical knowledge is dependent on qualitative inquiry. The compendium of signs and symptoms, albeit developed somewhat haphazardly in the 18th and 19th centuries, was dependent on observations and description. This continues, particularly in the identification of new diseases, for example in the identification of AIDS during the early 1980s. New medical procedures are documented using case study design (consider the evolution of heart transplants). Qualitative research is also being used to explicate symptoms (e.g., the early signs of heart attacks in females; Brink, Karlson, & Hallberg, 2002). But pointing out such obvious inconsistencies is not enough. This basic research (and I use that term deliberately) is not adequate for our critics—they need to see the numbers!

## CAN WE DEVELOP “PROPER” EVIDENCE?

Theoretically, we could use a mixed-methods design and calculate any impact on mortality resulting from our research, but in reality our research—when it *has* an intervention—uses designs that are not relevant to Cochrane (1972/1989) standards. Excellent clinicians (and excellent teachers) do not use behavioral interventions that consist of rigid protocols but, rather, use a blend of science and clinical wisdom. Interventions based on modifying behaviors and working through relationships lack the scientific rigor required by evidence based protocols. Our problems include (a) the impracticality of using double-blind treatment and control groups with replication in at least two independent sites with behavioral interventions (e.g., consider family therapies); (b) the reduction of such interventions to rote and precise rules counters the flexibility in approaches when underlying philosophies and treatment realities require uniqueness when caring for individuals; and (c) there is extraordinarily large contextual variation within the presentation of illnesses and needs, and in the contexts, cultures, and expectations of patients. Clearly, another form of evidence must be developed to justify qualitative inquiry as legitimate, appropriate, and desirable.

## ALTERNATE FORMS OF EVIDENCE

What are these “alternative evidences” for demonstrating efficacy? I am thinking of problems that are too chaotic to be explored using experimental design—for example, qualitative problems. I am thinking of demonstrating and extending our findings using *logic* and *common sense* and, if necessary, what our repertoire of *qualitative designs* that have previously not been part of mainstream qualitative health research.

## EXPANDING QUALITATIVE METHODS TO EXPAND TYPES OF EVIDENCE

*What are these designs?* I am thinking of designs used in the fields of engineering, such as nonhuman models in trials using simulation; also of methods used in biomechanics; also, of putting interventions in place following a single “near miss,” rather than waiting for statistically significant disasters, and the deliberate trialing using *n of 1* research. I am also thinking of legitimizing ethological qualitative microanalysis, used in anthropology, as evidence in its own right.

These research designs are well funded outside of health care but, strangely, are not considered rigorous within health care. How serious is the problem? My own trauma room research has identified “talking through” to help terrified patients with overwhelming pain, when analgesics are ineffective or withheld, to maintain control and not to fight caregivers (Proctor, Morse, & Khonsari, 1996). The efficacy of such human interventions cannot be readily demonstrated in a two-group design, but it could be demonstrated using logic and modeling (*How does resisting/fighting care increase the severity of a head injury?*). While the intervention is expensive (another nurse is needed), the cost savings in reducing trauma and the severity of

head injuries would be remarkable. I could also argue for other advantages—“talking through” synchronizes the trauma team, so that care is actually administered faster. If care is administered within the first “golden hour” after the accident, mortality is reduced.

## BEYOND SOCIAL SCIENCE: ADDITIONAL MODES OF QUALITATIVE INQUIRY/ESTABLISHED TYPES OF EVIDENCE

*What are these methods?* The first group I have classified as *Qualitative microanalysis*. These are characterized often by a single case, by abduction, and by attention to detail.

1. *Forensic designs*. This research is conducted “detective-style” on a single case or incidence, usually following a serious incident or major disaster, including loss of life. The goal of these methods is to identify causation and, hence, prevent recurrence, rather than—as in police work—to convict or—as in journalism—to expose. At the QHR conference in 2004, Linda Connell (2004), NASA scientist, described the Aviation Safety Reporting System (ASRS), which collects reports on aviation safety events and incidents, and the role of qualitative inquiry in identifying cause: “Black box cockpit recordings are qualitative data.” From such research, there are invariably changes of policy and procedure, changes of design of the aircraft, or further investigation into human limitations that may have contributed to the incident. A qualitative design that indisputably saves lives.

Forensic designs are used by many disciplines—obviously by police at crime scenes, in cases of sexual assault, domestic violence, missing persons, fraud investigations, and audit procedures. There are also fields of forensic engineering in anthropology and archeology. Of course, when a building has collapsed or a mummy has been discovered, qualitative methods are the only sensible way to proceed, and these techniques are accepted as standard practice.

Nevertheless, according to Connell (2004), the methods used by NASA to “diagnose” aviation errors are now being used in hospitals in the United States to investigate medical errors (Barach & Small, 2000). This is perhaps the most basic of applied research, for it is from examination of these single cases that pattern recognition and principles emerge. This new focus on patient safety will make this one of the most expanding areas of qualitative inquiry, and we should officially embrace these techniques and add them to our repertoire.

Closely associated with the previous aviation model is a new ASRA, NASA Ames Research Center, project, in collaboration with the U.S. Department of Veterans Affairs, that evaluates reports describing “near misses.” In this case, the “incident” has not actually occurred, but from a single report of “almost,” a close shave, the circumstances are investigated, warning bulletins released, and policy changed. In other words, an intervention is in place before the “problem” becomes an actual event. How to demonstrate efficacy? Just as avoiding a pedestrian at the last minute does not create a statistic, or dodging another vehicle and avoiding a fender bender does not result in actual cost savings, except at the population level and over time, such proven efficacy remains hypothetical.

Because the researchers are not waiting until there is loss of life—they are responding to a hypothetical case—that is the converse of statistical significance: It is hard to claim credit, to demonstrate a drop in mortality. These researchers are working from a theory of causality that states if something *almost* happened once, it could *actually* occur; they are using logic and experience, not experimental design. Their data rely on anonymity in reporting and a guarantee of “no reprisal” to those who report the incidents; hence, they leave no audit trails and cannot demonstrate the validity of their data. But these conditions ensure both that the researchers receive necessary data and that data are as comprehensive as possible; such conditions enhance validity. These are conditions of evidence, of hypothetical outcomes, and are devoid of the quantitative criteria of replication—*the ultimate ethic*.

What do I mean by the ultimate ethic? Simply put, it is to learn from near misses and to modify practices and develop policy from them. Connell’s (2004) example of the disregard for this model was a B757 wake turbulence accident at John Wayne Airport in Orange County, California, in December 1993.

The pilot did not know that the air traffic controller (ATC) had descended a B757 (which has extreme wake [vortex] frequencies [from its wings]) through his intended flight path. The captain was aware of the danger of wake vortices (although he was not told that the preceding aircraft was a B757)

and “was acting appropriately, according to what we’ve all been taught. The Westwind crashed in uncontrolled flight three miles short of the runway” (Pendleton, 2003, para. 10).

At the congressional hearing, the congressman for Orange County, responding to the comment that one near miss was not statistically significant, asked with sarcastic sadness, “And how many bodies on the runway is statistically significant?” (Connell, 2004).

2. *Deliberate trial or testing of interventions with N = 1 research*. An example of this type of research is the first heart transplant, and may or may not be happening today with human cloning. The design is usually experimental and outcomes unknown or uncertain, and the nature of the trial demands qualitative microanalysis, perhaps combined with some repeated quantitative measures design. Case study design is an inadequate description of this type of research.

This method has tremendous potential in health care for the examination of rare events, such as heart transplantation or the separation of conjoined twins. Are the findings of such research important? I believe Piaget (1954/1999) used this design when he observed and took careful notes observing his infants, and we know the impact his research has had on our understanding of infant behavior.

3. *Observation and precise, microanalytic observational description*. This is classic qualitative inquiry, which is not used enough, and it is the method most in need of development in qualitative inquiry. The use of video recordings enables microanalysis of movement, touch, and talk, and examination of the pacing of the care, so that behaviors, interactions, and responses can be examined.

Is this research of any *assistance* to our agenda of saving lives? This method is used in biomechanics and can be used, for example, in observing patient mobility when studying patient falls. This will enable bioengineering solutions, in the form of a safer bed or walking aids, to be developed and trialed.

Video is often used for conducting research with nonverbal patients, such as with patients with advanced Alzheimer's, or in exploring infants' response to pain, or the study of the breastfeeding dyad, and so forth. Thus, qualitative analysis using videotapes is powerful. It enables us to document, to illustrate our practice, and to communicate our findings and certainly should be used more often.

4. *Simulation*. This is the "crash test dummy" type of research. To reduce risk, simulation replaces people in certain high-risk situations. Despite the use of models used in teaching, I do not know of a single case of this type of research being used in qualitative research. This does not mean, however, that we should forget this option exists.

## DEVELOPING QUALITATIVE EVIDENCE

As researchers, we are tired of conducting underfunded research that seemingly goes nowhere. Yet, forcing ourselves into a quantitative system does not appear to be the answer. While we know that our research is significant and addresses problems that may otherwise be declared not researchable, our seemingly insurmountable problem is to convince those that control research funding, curricula, and the publication of texts and mainstream journals that our work is significant. We need to convince them that logic and common sense can produce powerful forms of evidence, and that sometimes we cannot afford, both in terms of morbidity and mortality and of dollars, the cost of quantitative inquiry.

Qualitative researchers sit on the fringes of research, but remember that it is on the fringes where the greatest advances are often made. We are addressing the confusing and chaotic problems that are too difficult to tackle quantitatively. But they are important problems,

Let us look internally, to ourselves, and bring together all our resources, all we know methodologically, and all that we know as professionals. Then, with a united voice, a rising chorus, demand the resources and attention that our research deserves.

## NOTES

1. Elsewhere, I suggest that Cochrane (1972/1989) was intending to target clinical opinion or clinical judgment regarding treatments in this level, and not qualitative inquiry and the type of problems it addresses, in this categorization. In fact, Cochrane probably did not know anything about qualitative inquiry as it is practiced today (Morse, 2006). Thus, it is an error—and invalid—to categorize qualitative inquiry into the Cochrane review criteria.

2. NIH-funded applications (with rare exceptions) are in the public domain, but this information could not feasibly be obtained one application at a time.

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