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An anthropologist's reflections on defining quality in education research

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In the USA there is a contemporary discourse of crisis about the state of education and a parallel discourse that lays a large portion of the blame onto the poor quality of educational research. The solution offered is 'scientific research'. This article presents critiques of the core assumptions of the scientific research as secure argument. These assumptions include: a misleading metaphorical conflation of education and medicine; an equating of 'scientific' with 'empirical' or 'rigorous'; a linear understanding of the relationship of research to practice; a parochialism that ignores research from other countries; a confusion of research quality with utility; and a naive belief in progress—'better living (and learning) through science'. Ironically, science-based practice is put forth as the solution to what ails education in the USA in the absence of scientific evidence that such an approach to educational reform is effective.

One reason I find so much about the contemporary discourse on quality in education research to be odd and even wrong-headed is that I came late to the field of education and education research. I was trained in anthropology and I did the research I am best known for before I had my first job in education and indeed before it ever occurred to me that I might end up a professor in a school of education. I got into education, in a sense, accidentally: as a post-doctoral fellow in the mid-1980s I did a study of preschools in Japan, China and the USA. This study was an ethnography, a comparative study of the cultural meanings insiders ascribe to their preschools and of how early childhood education programmes function as sites of acculturation. But because the subject of this study was early childhood education, it had the effect of transforming me professionally: I went in as an anthropologist and came out as an expert, in the eyes of others, on early childhood education. The success of the book led to receiving my first tenure-track academic position as an assistant professor of early childhood education in a Department of Curriculum & Instruction in a College of Education. I have been teaching in schools of education

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ever since. And yet, even after almost 20 years of being a faculty member in education, I still in many respects feel like an outsider. Engaging in discussions about what constitutes quality in education and in education research is one of those times when I feel most outside. In this article, I will attempt to convey some of my outsider's sense of puzzlement, confusion and sometimes irritation I feel about the way discussions about quality have been playing out in the USA over the past 10 years or so.

In this article, I write about quality in education research from several inter-related perspectives: as a faculty member located in a college of education in a large American research university; as a past member of the U.S. National Academies of Science (NRC) Committee on Research in Education (CORE); and, especially, as an anthropologist of education.

Scientific research in education as a cultural narrative of crisis and redemption

The contemporary American discourse on educational quality has at its core a narrative, a story with a past, present and future. The way this story goes: educational achievement has eroded over the years and is now at dangerously low levels, threatening U.S. economic vitality. This low achievement is caused by poor quality teaching, which in turn is caused by the low quality of teacher training done by schools of education and the poor quality of research produced by educational researchers. Faculty in schools of education are intellectually weak versions of their counterparts across campus and, guided more by ideology than by science, they support trendy progressive teaching practices rather than scientifically based practices. Research conducted by faculty located in schools of education lacks rigour, which is to be expected given the lack of discipline-based training and the low standards that characterises education researchers. Even when good research is conducted it gets ignored because practitioners do not know how to sort good (e.g. 'scientifically supported') research from bad research, a problem exacerbated by the fact that there is so much bad stuff out there. The good news is that forces external to schools of education are getting involved, by-passing schools of education by creating alternative routes to certification of teachers, funding researchers outside the schools of education to conduct rigorous research on education issues and pressuring schools of education to change the way they prepare education researchers.

This narrative is a contemporary American discourse that feels true in large part because it is so often repeated. It is a narrative that circulates across many domains of contemporary American public life: it can be found stated explicitly in documents and speeches of the Bush White House's Department of Education and in editorials in newspapers supplied by conservative think tanks. But it is not only, or even primarily, a Republican concern or story. Democrats as well as republicans backed the 'No Child Left Behind' initiative and democrats are as likely as republicans to argue that there is a crisis in American education. Democrats and republicans disagree about many education issues, including privatisation and vouchers and charter schools. But

politicians from both parties agree that making education research more 'scientific' is a key to improve educational achievement. This solution is the one that appeals to democrats as well as republicans and to American society as a whole because it is based on the combination of two familiar, enduringly popular narratives—a moral panic about the sorry condition of education and the declining wellbeing of America's youths combined with an optimistic belief in 'Better Living through Science'.

The core elements of the story of educational crisis and redemption through scientific research are so commonsensical, so oft repeated and so widely believed that it feels true. But it is not, or at least it is believed to be true despite a lack of evidence. There is no clear evidence that education in the USA is in decline, that the quality of teaching has fallen or that student achievement is not what it once was. As David Berliner and Bruce Biddle argue in their book *Manufactured crisis: Myth, fraud, and the attack on America's public schools* (Berliner & Biddle, 1996), middle class, white American children perform as well on international achievement tests as do students in the highest achieving countries. Where the USA falls short is in the education of children who live and go to school in the country's poorest neighbourhoods. Schools in middle-class and wealthy communities are achieving at very high levels, suggesting that the core problem is not that U.S. teachers do not know how to teach or that they lack a scientific basis for their pedagogy. Similarly, the argument that the quality of research in education is poor and that the preparation of educational researchers therefore needs to be overhauled is put forward with little or no evidence. I agree with critics of research in education that there is a lot of poor quality work being published, but I would argue that there is a lot of poor quality work being published in many, if not all, disciplines and that the core problem of educational research in the USA, as with the core problem of K–12 student achievement, reflects not an absence of high quality work at the top of the discipline so much as the prevalence of low quality work at the bottom. I attribute the prevalence of so many poorly conceptualised studies in education journals not to inherent flaws in the approach to doctoral student preparation of the top education doctoral programmes, but instead to the fact that many of the thousands of people earning doctorates each year in education in the USA and many of the untenured professors publishing articles in second, third and fourth tier educational journals have little interest in or aptitude for doing research and are driven instead by the growing requirement that even faculty working in teacher training programmes in non-research universities are required to publish or perish.

The solution offered for these problems, namely, the introduction of scientific principles to the field of education, from the pre-K to the post-graduate levels, is also put forward without the evidence that such a scientific approach is effective. There is no evidence that Japan, Singapore, Finland and other countries that are cited as examples of countries with far better education systems make better or more frequent use of scientific research in their curriculum planning. The irony is that 'science-based practice' is put forth as the solution to what ails education in the USA in the absence of scientific evidence that such an approach to educational reform is effective.

In the late 1990s, the National Research Council of the National Academies of Science formed the Committee on Research in Education (CORE), which in 2002

published its report, *Scientific Research in Education (SRE)* (NRC, 2002). On the whole, *SRE* is a very sensible document reflecting the wisdom of its very sensible and learned committee members. It is carefully worded and cautious not to say anything extreme (the collaborative nature of the enterprise and the levels of vetting used by the National Academies precluding extreme positions). But my contention is that even such a reasonable report by such reasonable people has within it the narrative of panic, education bashing and science as solution discussed above. Reading *SRE* closely and drawing on my experiences as a member of CORE in the second go-around of the committee (2002–2004), I will give some examples of these dynamics. My point is to attempt not so much to argue with *SRE* as to try to get outside it, to question its basic assumptions and to show that it is an ideological/cultural product and to thereby provide some critical reflections on the problem of defining quality in educational research.

SRE's core assumptions

The report issued by the Committee on Research in Education, *Scientific Research in Education*, begins with a statement of the severity of the problem (the sense of panic about the poor quality of education and educational research) and then proceeds to offer more and better scientific research as the solution, without addressing the political and social context that underlies both the sense of crisis and the support for scientific intervention. The argument of the report, as stated in the Introduction, is that the country is crying out for the overdue application of science to the problems plaguing American education:

In this ever more complex world, in which educational problems tend to be portrayed with the urgency of national survival, there is (again) an understandable attraction to the rationality and disciplined style of science. Simply put, for some problems citizens, educators, administrators, policy makers, and other concerned individuals want to hear about hard evidence, they want impartiality, and they want decisions to rest on reasonable, rigorous, and scientific deliberation. (p. 12)

This is a tricky but significant passage. The phrase ‘tend to be portrayed’ lets the authors have their cake and eat it too—they cite the crisis and battle for national survival without quite endorsing the panicky tone. The passage is also notable for its lack of authorial agency: it is not the authors of the report who are pushing the scientific agendas; rather, the authors are just responding to the public’s will.

In this passage, as throughout the report, we see the sophistication and reasonableness of the committee members and indeed can infer the process of debate and compromise that led to each carefully crafted sentence in *SRE*. Clearly, the committee members are smart and sensible. Each step of the argument seems reasonable. And yet the effect of the whole effort I find problematic and even dangerous (to strike an admittedly alarmist note of my own).

One major problem is the silence about the political and social context in which this committee was formed and the report issued. The report is presented as a solution, but is coy about the problem. At a public session for the release of the report, one of

the invited commentators, Frederic Erickson of the UCLA School of Education, asked for an explanation, which he felt was missing, of the political context that led to the committee being formed and the report being written. Michael Feuer, Director of the Center for Education of the National Research Council, attempted to provide an explanation:

When we were given this assignment, one of the things we did, as we always do, was talk to people in the field. And I remember one very poignant conversation I had with a distinguished scholar to whom I said that we were about to launch a study of the scientific quality of education research. And his answer was, well, finally a short report will come out of the Academy. That, along with other things that I heard confirmed a general sense that there is a perception, right or wrong, that the scientific quality, whatever that means, of education research is not as good as it could be, number one. Number two, that the problems in education are so fundamental to American society and to the future of the American way of life, that we better apply the best tools we have, whether they be scientific or other, to addressing them. And that there is a perception that we haven't done a good enough job of that. Now I'm not endorsing that perception. I'm reporting my understanding of what that perception has been. And third, that there is a longstanding, historical traditional in American society to turn to science, technology, and rational thinking as a way to address very fundamental complex problems. (<http://www.nationalacademies.org/cfe/Morning%20Session.html>)

I find this explanation telling in that it both makes the accusation that justifies the report and at the same time claims to not endorse 'that perception'.

In an article published in the *Educational Researcher* later that year, Frederick Erikson and his colleague Kris Gutierrez repeated this critique that the report was disingenuous in not addressing the wider political context (Erickson & Gutierrez, 2002). This was just one of the critiques put forward in the special issue of the *Education Researcher* devoted to discussion of the report. There have been many critical responses to *SRE*, as we should expect and welcome as a part of the scientific process. But the response of the authors of the report to critical commentaries has often been to accuse those who disagree and critique the report of misreading. In fact, such misreadings were anticipated by Pat Graham, of the Harvard Graduate School of Education, who in her comments at the *SRE* briefing declared herself an admirer of the report, but who also feared that it would be misunderstood:

You need to read footnote 4 on page 94 that says history and philosophy are also relevant to understanding education. You need to read on page 52, "science does not necessarily mean good." Because if you do not read this report carefully, and if you only read it with executive summaries, as some of us have sometimes done in reading reports, you will get a simplistic notion that the report does not, in its complexity state that what we have here is the paradigm for the hard sciences that need to be applied to education. And that it is just scientific, and then it would be all fine. That's not what the report says. But a cursory reading would leave you that impression. (<http://www.nationalacademies.org/cfe/Morning%20Session.html>)

This is a sensible warning. But why is it necessary to issue such a warning not to misread the report in this way? I would suggest Professor Graham's concern arose not only from her low opinion of the reading and attention skills of some of the report's intended audience but also from the fact that the report has embedded within it

precisely the argument we are told not to (mis)read into it. *SRE* in several places tells its readers what it is not saying, in such phrases as ‘We are not suggesting that there is not good research being done that is not scientific’. I suggest that such disclaimers, like Michael Freuer’s joke about the brevity of a book on scientific research in education, simultaneously reveal and attempt to mask their authors’ biases against what they see as the weakness of education scholarship and in favour of what they see as the strength of science over other forms of scholarship.

A conflation of science with rigour and empiricism

In their article in the special issue of the *Educational Researcher* on *SRE*, Michael Feuer, Lisa Towne and Richard Shavelson (the report’s editors) write: ‘Our attention to scientific educational research is not intended to exclude or minimise other forms of educational research and scholarship (Feuer, Towne & Shavelson, 2002)’. Versions of this disclaimer are sprinkled throughout the report. But again, I would suggest that the authors of *SRE* want to have it both ways. As a member of the second iteration of CORE (I came on the committee after *SRE* had been published), I found in discussions with my fellow committee members a confusion about whether our purview was scientific educational research or the general state of educational research and about whether *SRE* and our new committee were calling for the scientific research in education to be more rigorous or for more of the research in education to be scientific. I think the answer is ‘both’. In our committee discussions, as in *SRE*, there is a frequent slippage between addressing the needs of educational research in general and the needs of scientific research in education. This slippage sometimes takes the form of a confounding and conflating of the term ‘science’ for ‘rigour’ and for ‘empirical’. This goes to the core of my concern about the impact of *SRE*: it may be used by university administrators and funders of research to justify a shifting of faculty lines and grant monies away from educational researchers whose work is non-scientific and towards the self-identified scientists. Feuer and Towne’s response to this concern is that this is not the intent of the report, that to use the report in this way would be to misuse it and that a report issued by the National Academy of Science of course is going to express a belief in the value of a scientific approach. Fair enough, but the problem is that when it comes to defining the problem and offering solutions, education research that does not fall within the parameters of science is forced to compete with scientific research on a playing field that is not level.

One of the intended audiences for the report is the ‘federal education research agency’, as used in the sentences: ‘This study will review and synthesise recent literature on the science and practice of scientific education research and consider how to support high quality science in a federal education research agency’ and ‘How can a federal research agency promote and protect scientific quality in the education research it supports?’ This agency in the USA is called ‘The Institute of Education Sciences’, IES. The key figure in this agency at the moment for defining quality in education research is Grover (Russ) Whitehurst, the Assistant Secretary. Trained as an experimental child psychologist and known for his development of ‘scientifically-based’

approaches to the teaching of early reading, Whitehurst during his tenure at IES has narrowed and focused definitions of fundable research, most famously in his calls for creating a 'culture of science' at IES, for funding of only 'scientifically-based research', for the closing of the Education Research Information Center (ERIC), for funded research on literacy to be done more by psychologists and less by faculty members in departments of language and literacy and for focusing the training of educational researchers on random assignment research designs. Each of these decisions has had significant impacts on education research and education researchers in the USA. It is the *SRE* report's unwillingness to directly acknowledge or take on Whitehurst's narrowing and politicised understanding of what counts as research that was behind the concern expressed by Erickson and Guterrez and others that *SRE* lacked an attention to its social and political context. *SRE* to its credit makes it clear that it favours a much more expansive definition of science than does the current administration of IES. But the authors of *SRE* claim to be unconcerned that the impact of the document is to strengthen the position of Whitehurst. As Michael Feuer and Lisa Town write: 'And frankly, if our findings happen to coincide with the viewpoints of some federal officials, so be it.'

On several occasions when I complained in discussion on the CORE committee about the favouring of scientific over all other forms of research and the conflating of science with rigour, I was told by other committee members, 'We consider you a scientist. Anthropology is a science'. Some committee members could not understand why I would not take this comment as a compliment and as a solution to our disagreements. I would point out that although I conduct empirical work and consider my methods rigorous, I do not base my notions of empiricism and rigour on science. Anthropology, sociology, history, philosophy and other disciplines that study education have their own canons of rigour and their own ways of defining quality of research. My fellow committee members would once again assure me that they were not arguing that the non-scientific disciplines were not worthwhile or that they had nothing to contribute, and yet, when the conversation would move on, I would once again hear 'evidence-based education' used as a synonym for 'scientifically-based' and more generally for 'empirical' and 'rigorous'.

Metaphorical thinking

The *SRE* report, along with the larger contemporary discourse on research in education, has a tendency towards sloppy metaphorical reasoning. The two most common of these metaphors, building moon-rockets and wiping out disease, are presented in one sentence in the introduction of the report:

To meet these new demands, rigorous, sustained, scientific research in education is needed. In today's rapidly changing economic and technological environment, schooling cannot be improved by relying on folk wisdom about how students learn and how schools should be organized. No one would think of designing a rocket to the moon or wiping out a widespread disease by relying on untested hunches; likewise, one cannot expect to improve education without research. (*SRE*, p. 12)

In this passage, approaches to education (including, presumably, approaches such as Dewey's philosophy and Freire's politics) that are not based on scientific research are reduced to 'folk wisdom'. Equally problematic, the forms of science that are offered as models for education are among the worst comparisons that could have been selected. Educational problems are in some ways like some scientific problems, but they are not very much like sending a rocket to the moon or wiping out a disease.

The use of the metaphors of physics and engineering is particularly unfortunate because educational problems could be more usefully compared to the problems studied by other 'hard' sciences, such as evolutionary biology and cosmology, disciplines, like education, that address questions that have moral and political dimensions and that do not lend themselves to one-off, once-and-for-all solutions.

The medical metaphor is by far the most common one held up as a model for educational researchers. Here are some typical examples: the first one from Chester Finn, a leading figure in the conservative educational policy movement:

The education profession is awash in fads and bad ideas. It favors curricular and instructional strategies grounded in ideology and wishful thinking over those based on scientific proof. In the crucial area of reading instruction, for example, colleges of education continue to induct new teachers into 'whole language' methods despite decades of evidence that phonics-based methods work better with most youngsters. Diane Ravitch and E. D. Hirsch have brilliantly traced the profession's abiding affinity for 'progressive', 'child-centered' methods that accord with its beliefs even though they don't work very well for children. If physicians behaved similarly, hospitals would still feature leeches, incantations and mustard plasters. Remedy: Out with the snake oil. Only research-based practices should be tolerated in our classrooms—and only bona-fide scientific research should be tolerated by the leaders of this profession and those who run our schools (available at: <http://www.mdtaxes.org/news-stories/educ.gadfly.chester.finn.1.3.02.htm>).

Next, a quote from Lawrence Summers, when he was still president of Harvard: 'If we took the same attitude towards rigorous evaluation in medicine that we take in education, people would still be being treated with leeches' (available at: <http://www.president.harvard.edu/speeches/2003/chicago.html>).

SRE, as shown above, fell into this pattern of comparing the contemporary state of education research to pre-twentieth-century medicine. I suggest that this metaphor is misleading, unhelpful and, in the end, damaging in several ways. First of all, it idealises and misunderstands the practice of medicine, of medical research, and the relationship between the two. For example, there are studies that show that up to 70% of prescriptions written by doctors are incorrect, in the sense of being inconsistent with the medical literature and the intended and approved uses of the drugs. Other studies emphasise the social, economic and political nature of much medical decision making. Secondly, the medical metaphor for education focuses on the wrong medical subfields. Surely, addressing educational problems such as the chronic underachievement of students from disadvantaged communities is less like finding a cure for polio than it is like tackling such intractable problems as sexually communicated diseases or obesity. Medical research has led to improved understandings of obesity, and yet more and more young Americans are dangerously overweight. Is the core of

the problem here the poor state of medical research in this area? Pat Graham makes this point eloquently:

Major research in medicine has allowed physicians and surgeons much better tools with which to operate. Anesthetists have better skills in putting people to sleep so this can go on. Finding medications that control widespread ailments such as diabetes, hypertension, and coronary disease. When medicine has turned to changing the way people live their lives—lose weight, don't drink so much, don't use drugs, don't engage in illegal, unsafe, or socially disapproved sexual activities—medicine has had much more difficulty changing individual behaviors than it has done in changing the techniques that allow us to learn. Smoking is perhaps the best example. (available at: <http://www7.nationalacademies.org/cfe/Morning%20Session.html>).

The sloppiness of the medical metaphor extends into analyses of the problems and the proposed solutions to what is taken to be the poor preparation of education researchers. In appeals to professionalise the field of education, medicine is held up as an example, with the formation of the American Medical Association and the standardisation of the education and licensing of doctors presented as a model for education. The problem here is a confusion of the preparation of doctors with the preparation of medical researchers. The training of doctors in medical schools in the USA is relatively standardised. It could reasonably be argued that teacher preparation could benefit similarly from more standardisation from state to state and university to university (although I would argue teacher preparation programmes are already very alike across the country, despite the absence of a national teaching certificate, and that there is no evidence that greater homogeneity of training programmes leads to greater teacher quality). The medicine metaphor completely breaks down, however, when used as a model for the training of educational researchers. There is no standardised training or licensing of medical research in the USA (or elsewhere). Many of the leading medical researchers (including Nobel Prize winners in medicine) are not themselves physicians. Medical schools offer little training to doctors in training in how to conduct medical research. Successful medically trained researchers acquire most of their skills outside the formal curriculum, through a research apprenticeship model. For these reasons, calls for the improvement of the quality of education research via the greater standardisation of the curriculum in doctoral programmes in education should refrain from using medical research as support for their argument. Indeed, I know of no research showing that the standardisation of preparation of scholars in any academic discipline can be shown to increase the overall quality or vitality of research in the field.

Linear views of the relationship of research to practice

The idea that complex social challenges, such as the education of a population as diverse as that of the USA, can be solved through a process of scientific research leading directly to applications for practice is flawed in being too linear, too hierarchical and insufficiently contextual. It is a simplified and idealised model of how science works: value free research, testing hypotheses one at a time, leading to support of

some hypotheses and the dismissal of others and in turn to the adoption of better, more effective practices by practitioners. This model of how science works and how it influences practice policy is not supported by studies in the history of science (Kuhn, 1970; Latour, 1987). It is cited as the necessary strategy for helping the USA catch up with countries ahead of us in the international education achievement league tables, and yet it is a model not followed by the countries ahead of the USA in achievement and there is little research evidence to suggest that this model works for improving education (see Tobin, 2005).

A related problem is that this view of scientific research in education positions practitioners as downstream, as receivers but not producers of knowledge about teaching, or worse, as functionaries who should be given scripted lessons to deliver, rather than trusted to follow their professional judgment instincts (which are reduced to 'folk wisdom'). The model of scientific research in education most valued by *SRE* is not teacher research (research carried out by practitioners in their local sites) but rather the development and evaluation of interventions carried out by scholars located in universities that are intended, once proven to be effective, to be 'scaled up', which means to be disseminated to teachers across the country (Tobin, 2005).

I suggest this is a version of the problem of privileging what Bruno Latour describes as forms of science that seek to be universal over forms that are highly contextual. In his book, Bruno Latour suggests that there are two different kinds of scientific knowledges: the local and the universal (Latour, 1987). The universal types are not necessarily better science than the local types; but they spread more successfully not just because they are pushed by more powerful forces but also because they are less dependent on context and therefore more able to travel. From this point of view, educational research that emphasises the contextuality and contingency of learning in particular locales is less powerful, less useful and less deserving of funding than research that claims to be getting at universal theories of teaching and learning and aims at developing new approaches to teaching that can work across contexts. This means, for example, that ethnographic, 'situated cognition' and 'cultural historical activity theory' approaches to studies of learning in particular communities both abroad and in the USA are less likely to be seen as being as valuable or as deserving of major funding as studies featuring randomised assignment designs, which are considered to be the educational research 'ideal' (in the language of *SRE*) or 'gold standard' (in the language of *IES*), because such experimental designs are believed to be the best suited to identify 'what works'.

The problem with defining education research in terms of utility

The moon-rocket and disease eradication metaphors as models for education research share an implicit, unvoiced central assumption: educational research should be useful; it should be an applied science, focused on identifying 'what works' and thereby serving the advance and accumulation of educational knowledge and improving the quality of education in the nation's schools. I do not question the idea that education research (sometimes) has impacts on what goes on in schools. There have

been very few studies demonstrating how education research improves education practice and educational achievement, but I do not doubt that this sometimes occurs and I think the goal is laudable. What I question are the starting premises of this approach to defining quality in educational research. Much of what I consider to be the best educational research is not concerned (directly, at least) with solving particular educational problems or introducing an educational innovation and evaluating its effects. Quality of research in most disciplines is not defined by utility. The best literary scholarship is not the scholarship that leads to changes in how books are written or taught; the best scholarship in physics does not lead in a direct way to inventions that improve the quality of everyday life; nor does most research in political science lead to changes in the way we are governed. Of course, more research in a professional discipline such as education, medicine, social work or engineering will be addressed to practical problems than will most research in the traditional academic disciplines. But this does not mean that the quality of research in the professional fields should be defined by the degree to which it is useful. The impacts of education research are hard to measure and may take many years to come to fruition. Studies that do not aspire to be practical may turn out to have more dramatic impacts than studies that are self-consciously 'use-inspired' (Lagemann, 2002). My concern is that the eagerness of educational researchers to respond to calls for 'evidence-based educational policy' has worked to reduce rather than raise the quality of research in education. The medical and moonshot metaphors lead to valorising of research that claims to have the potential to directly impact practice over research in education that aims to understand, without claiming to be directly useful.

The problem of progressivism

Even more basically I want to challenge the assumption that the quality of education improves over time. Defining quality in education research by the utility of its findings only makes sense if we assume that education progresses from one generation to the next. I would argue that we have no *a priori* reason to assume that education today is better than it was a generation ago. Some things improve over time, others do not. What is education? Is it more like sending rockets further and further into space (which, arguably, we do better now than we did 20 years ago and which we certainly do better now than we did a century ago)? Or is education more like family life, community, rhetoric, poetry or philosophy, which we would be hard pressed to demonstrate have improved and perhaps have even declined over the generations in many societies.

My questioning of the notion of progress in education is informed by my ethnographer's belief in cultural relativism. Just as cultural relativism, which is one of the guiding principles of cultural anthropology, argues that we should not judge one culture from the point of view of another, historical relativism would suggest that we should not judge the past from the perspective of the present. Therefore, just as an educational ethnographer would not judge one's culture approach to socialising and educating children as better than another's, neither should we judge the education of

today as better as the education of the past. In other words, as we think about education across time and space, we need to critique and control not just our ethnocentrism, but also our progressivism, our tendency to talk about historical change in education in terms of a narrative of progress and to believe that newer forms of education are superior to older ones and that change is better than continuity. Defining quality of education research in terms of research that causes change may work to exacerbate the relentless pressure already put on teachers and school systems by politicians to be continuously changing. Education researchers should try to stay above or outside the fray of pressure to change schools, and to describe and understand these pressures rather than to be part of them.

Provincialism/parochialism

As an educational anthropologist and comparative education scholar, I am struck by the provincialism of much of the discussion in the USA of quality in education research. In the contemporary USA, during an era of increased calls for more and better education research, there is decreasing interest in (and funding for) international/comparative education. In *SRE*, there is very little mention of research done in other countries (other than mentions of the US lagging on TIMMS and other international measures of educational achievement and a description of Robert Putnam's study of civic engagement in Italy). In the reference sections, of the approximately 300 works cited, it is difficult to find any not done by US based researchers, on US populations, published in a US based book or journal. As in the Sputnik era, the argument of *SRE* is couched in national and even patriotic tones: what can the USA do to improve the quality of educational research in the USA and thereby improve the quality of education in the USA. Similarly, the science being called for and funded by the Institute of Education Sciences is a science that focuses on the question of what works in the context of education in the USA. This makes sense given that the National Research Council and the IES are American public institutions; and yet there is a contradiction between calling for scientific research and limiting the purview of the research to the USA. What kind of science confines itself to national boundaries?

The USA is looking for 'what works'. But most of the research cited in *SRE* and funded by IES excludes studies of what works in other countries. Many countries do much better than the USA on reading and mathematics, but the USA shows little and declining indication of wanting to learn from them.

A league table approach

The international/comparative studies conducted by the US researchers that get the most attention and funding are those tied to TIMMS and other large international studies of educational achievement. Such studies, inevitably, emphasise a 'league table' approach that focuses on who is at the top of the standings and frets about the questions of 'What place are we in?' and 'What can we do to move up the rankings?'

From an ethnographer's point of view, these large international studies are interesting, but problematic as standards for high quality educational research because they are insufficiently contextual.

This league table approach to comparative education research also serves to fuel the sense of crisis discussed above. And it leads to misleading correlational thinking: If the Japanese have higher mathematics scores than we do and if they teach in different ways (e.g. more emphasis on algebraic principles in early grades), then the differences in the ways they teach are producing the higher scores. The Japanese higher scores can be explained by many things, including socio-economic factors, structural factors (e.g. a centralised school system), teacher support (lesson study), higher achievement motivation, or test-taking savvy. To conclude that the higher test scores are caused by any single practice in the classroom (textbooks, pedagogy) makes no more sense than to conclude that the cause is wearing uniforms or taking shoes off before entering the classroom. It also leads to a sort of 'victor's justice' form of reasoning—the countries at the head of the achievement tables are assumed to have better educational systems and approaches than those in the middle or bottom of the tables. But, given the very high correlation between performance on standardised tests, educational achievement and socio-economic status, it could be that a very poor country with low overall achievement scores is doing some very interesting and useful things in their classrooms while a rich country with high scores may be getting the high scores despite rather than because of the strength of their approach to pedagogy and curriculum.

These large international studies also tend to fail to pay attention to within country variation. If Japan has higher mathematics achievement scores than the USA and also higher student achievement motivation, we should not make the mistake of jumping to the conclusion that the higher achievement motivation is a causal factor unless we can first show that within Japan those students who achieve the most have the highest achievement motivation. The causal inference must be supported first within the country before being used for cross-national inferences.

How to use cross-cultural educational research

How, then, should we use cross-cultural studies of education? As an alternative to league tables, correlational reasoning about differences and decontextualised borrowing, I suggest that a better use for cross-cultural/cross-national studies of education is to use ethnographic ('thick') views of education in other countries/cultures to help us imagine how education can be different and thus to denaturalise how we see education in our own culture. For example, to the degree my own comparative research on early childhood education in Japan, China and the USA has had an impact on the field, I would argue it is not via a linear process of first persuading scholars with scientific evidence who then persuade policymakers and practitioners but instead by directly addressing practitioners with evidence that questions taken-for-granted assumptions, expands the repertoire of the possible and rehabilitates disparaged practices and ideas.

Conclusion

Where does this leave us? Perhaps an anthropologist is the last person one would want participating in discussions of quality in education research. I suspect at times I made my co-members on the Committee on Research in Education think so. My understanding of the implications of cultural anthropology leads me to be intuitively suspicious of any quest to define quality, other than as a cultural belief system. It also makes me uncomfortable with notions of progress. Anthropology, as a field, has been split between those anthropologists who embrace and resist calling themselves scientists. As an anthropologist on the non-scientific side of this divide (but near the barrier), I find myself uncomfortable with any conflation of rigour and empiricism with science. For these reasons, I was then and now remain uncomfortable with the National Academy of Science forming a committee on scientific research on education. My position then and now is that it is appropriate for a committee working under the auspices of NAS to comment on and attempt to influence the *scientific* research conducted in education but not for such a committee to take a position on what constitutes quality research in education. Attempts to define quality in education research, no matter under whose auspices they are conducted, are exercises of power as well as knowledge that hold implications for who gets hired and tenured, what projects get funded and what gets taught in graduate programmes to the researchers of the future. It is appropriate for faculty in schools of education to conduct research on educational issues and problems. But my argument in this article has been that it is counterproductive to define the quality of educational research by the degree to which it is deemed to be either useful or scientific.

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