

What Rural Education Research is of Most Worth? A Reply to Arnold, Newman, Gaddy, and Dean

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Citation: Howley, C. B., Theobald, P., & Howley, A. A. (2005, December 31). What rural education research is of most worth? A reply to Arnold, Newman, Gaddy, and Dean. *Journal of Research in Rural Education, 20*(18). Retrieved [date] from <http://www.jrre.psu.edu/articles/20-18.pdf>

Offering a response to the question, “What rural education research is of most worth?”, we recommend an approach very different from the one taken by Arnold, Newman, Gaddy, and Dean (2005) in their consideration of the rural education research literature. We remind readers that about 150 years ago, Herbert Spencer put a similar question—“What is the knowledge of most worth?”—to his readers, concluding that the answer was “science.” We intend only light irony, however, acknowledging that the scientific spirit exerts strong influence on the project of scholarly research (for broad principles see, for instance, Shavelson & Towne, 2000). This influence is felt not only in hypothesis testing with mathematical tools, but also in historical, ethnographic, and other forms of research that rely more on analysis of language.

Arnold and colleagues’ review takes as a given that the rural education research of most worth identifies “the causes of different student outcomes” (p. 9). Such a mission might reasonably guide research in a field that doesn’t take context as the principal defining condition. We might as well *not have* rural education research, nor rural education for that matter, that fails to center itself on rural cultures and ways of engaging life.

Meaning and Motive

Why? Don’t rural schools *just happen* to be situated in rural areas, as, for instance, a sort of accident? What’s so *special* about rural places? Many people don’t know. And our university colleagues who don’t work in rural education research want a definition of *rural* to convince them that rural is somehow important. Many of us think we know, after all, what makes people *black* or *poor* or *male*. “What’s an equally simplistic answer for what constitutes ‘rural’?” our colleagues seem to want to know. Failing a simplistic answer, however, they lose interest. “What is rural? Tell us;

keep it simple!” Beale codes? Johnson codes? Multidimensional schemes? Quick!

The expectation of a best simplistic definition is largely a misstep, though, because any thoughtfully created, relevant scheme can be useful in rural education research (including black = 0, poor = 0, and male = 1). Two of us favor some combination of the Johnson codes to mark schools and districts as “rural” (e.g., rural = 6 or rural = 7 or rural = 8). Researchers with a motive to study rural can easily take their pick, matching it to their purposes and methods. The sticking point here is motive, not definitions.

The *rural* in rural is not most significantly the boundary around it, but the meanings inherent in rural lives, wherever lived. This insight points to the need to ask questions centered on, or informed by, such meanings. Explaining this position is not so simple as answering “Beale codes!” or “Johnson codes!” We have, however, articulated these meanings at length elsewhere, and there’s no reason to recap that discussion here (see, for instance, Howley, 2002; Howley & Howley, 1999; Howley, Howley, & Pendarvis, 2003; Theobald, 1997; Theobald & Curtiss, 2000).

Again: Rural meanings are the *raison d’etre*, the motive, for the best rural education research. This answer to our question might suggest—wrongly—to some readers a qualitative turn, but we’re *not* dealing with overall methods or specific issues of research design. The implications are salient to *all* modes of research, from narrowly descriptive to decidedly theoretical, and from discourse analysis to hierarchical linear modeling.

The presumption that certain causes of outcomes pertain to rural students in contrast to other students—as Arnold et al. appear to suggest—reveals an illusion that plagues many scholars, especially those with little interest in or knowledge of rural life. Why an illusion? It reflects such researchers’ vain quest for a systematic rural influence (expressible as a unitary proxy) equivalent to, but distinct from, poverty and race. Such a variable seems not to exist, or to exist as a weak influence that can hardly be mistaken as the equivalent of poverty or race.

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We argue that the holy grail of rural education research lies altogether elsewhere, in what the philosopher Alfred Schutz called “the lifeworld” (e.g., Schutz & Luckmann, 1973), in the flow of seemingly unremarkable everyday moments, where rural people make rural sense of, and with, their rural lives. These matters are not often studied, but excellent examples of engagement with the outlook of the rural lifeworld exist in this journal (e.g., see Burnell, 2003; Lyson, 2002).

Would-be rural education researchers must therefore develop the capacity to understand this lifeworld as motive for their work, and this requirement applies even if their research questions do not directly address the issues of the lifeworld. This capacity may come more naturally to qualitative researchers than to quantitative researchers, but both modalities must be informed and engendered by the meaningfulness of rural life; their interpretations must exhibit a grasp of the significance of rural meanings. Otherwise, by definition, their studies are not rural.

Rural education reports are clearly under no obligation to approve or validate the meanings (researchers are rightly dubious from a variety of standpoints), but researchers must comprehend these varied meanings. Otherwise, work that aspires to be rural education research isn’t. Instead it renders the rural connection inconsequential, if not invisible. Such research cannot contribute to understanding, much less to scientific progress.

This situation is, alas, all too common. So many quantitative manuscripts submitted to this journal for review fail to engage rural meanings, and a critical role for the journal’s reviewers is encouraging authors to engage such meanings. It’s not easy work, and the engagement itself—as the preceding discussion suggests—is also difficult. Living in rural places certainly helps, but even so, bringing these meanings into a formal academic context, even indirectly as motive, is challenging. Social, political, and corporate administration; mass media; and global marketing trivialize and obscure rural meaningfulness (e.g., see Habermas, 1987, on the “systemworld”). Our doctoral students in rural education, for instance, are routinely surprised to learn that coherent accounts of this meaningfulness exist. This situation is not new: Raymond Williams (1973, p. 6) once observed of his own rural history, “It is ironic to remember that it was only after I came that I heard, from townsmen, academics, an influential version of what country life, country literature, really meant: a prepared and persuasive cultural history.” A wide gulf separates the lifeworld and the usual forms of academic engagement, and this is a challenge not only for rural education research, but for all forms of science, and especially for social science (Schutz & Luckmann, 1973). Williams’s experience is common, and it bedevils aspiring rural education researchers.

Should Rural Meaningfulness Be Preserved?

Our McREL colleagues are partly right in the claim that many of us who self-identify as rural education researchers—together (surprise!) with most rural people—believe “that there is a quality inherent in rural communities and schools that should be preserved” (p. 1). As the Williams excerpt indicates, generations of rural writers have shared this outlook.

Arnold et al. are only partly right, however, because the attribution of an imperative—“should be preserved”—is hardly the sort of thing one embeds in a study design. Instead, embracing such an imperative is external to research design, where the motive to do this work properly lies. One may embrace it or not: but understanding the position is no more optional than understanding the rural meanings relevant to it.

Yes, many of us *do* believe that rural life is richly generative of meaning, and we want to foster that generative quality. It’s not a fault. It’s also not research. Such an aspiration, one should note, is not different in kind from wanting to improve student achievement as measured by tests.¹ Both aspirations comprehend varied motives and understandings that are prior to designing and conducting decent research.

In the discourse of the McREL article, attributing a vague “belief” to many researchers far too easily dismisses not only the motive but, more importantly, the substance of the best rural education research. It’s a truly shabby move. (In the final section of our reply, we question the motives of the McREL team. But we do so openly, on the basis of the evidence and, more critically, on the basis of an articulated argument.)

As noted above, the generative quality of rural life has been repeatedly articulated. It’s been addressed at considerable length, in public forums in education, sociology, literature, history, and political science, and not only by us (e.g., Haas & Nachtigal, 1998). The choice to ignore this conversation is not even defended in the review. Apparently, no defense is thought necessary.

Should meaningfulness in the rural lifeworld be preserved? We’re inclined, with psychologists like Jerome Bruner (1996), to answer “yes,” believing that more meaning is good. Affirming this position seems especially important now because the generative meaningfulness of the lifeworld is nearly everywhere under persistent attack (e.g., Young, 1990). We defend this position as a responsible commitment external to the technical conduct of research, but we also note that whatever one’s *position* on the issue, consideration of rural meaningfulness is essential to rural education research, even in an analysis that honors the agenda of the Institute

¹For the record, we three do indeed think that good rural schooling fosters authentic intellectual accomplishment.

of Education Sciences (IES), and perhaps most particularly in such a piece.

The Meaningfulness of Place

Organizing education scientifically has proven to be dicey business, of course, and after decades of expensive and largely abortive attempts, that project remains an unrealized hope for some. The failure is what now motivates IES to narrow its view of science, but to others, this failure is the symptom of an unrealizable fantasy.

The fate of the Tennessee study of class size (a large-scale randomized trial) is a case in point: the findings aren't generalizable to other settings and the home state wouldn't act on them (Shavelson & Towne, 2000). The impediments, in other words, are substantial even to the "best" science, very narrowly defined.

Aspirations for a stronger influence for education research will probably continue to be disappointed even with more of this "best" science, but the aspiration to organize education to conform to "best" scientific findings strikes us, and will strike others, as hubris. The hubris is extreme when the contingent meaningfulness of science is diminished by a narrow, and perhaps naïve, conflation of "findings" with "truth." Furthermore, such hubris encourages damage to the world it tries to improve, especially when so-called truth is recast as an imperative—what everyone, everywhere ought to do.

For an excellent scholarly account of the damage done in the name of science in attempts to improve the human condition, we encourage readers to see James Scott's *Seeing Like a State* (Scott, 1998). Scott finds that the resistance to the imperative nearly always produces an effect greater than, and quite different from that anticipated by, the science. Moreover, the motives of a regime in sponsoring such turns (NCLB is an example of such a turn; the IES agenda is another) put the self-preservation of the regime well before the popular good. Scott's analyses include educational examples, and they could be applied to contemporary American educational reform efforts, including those for education science.

By our mutual lights, however, and from the vantage of understanding the arguable importance of rural meanings, education is unlikely to become a generic enterprise conducted by a scientifically guided schooling bureaucracy. Instead, and as a result of the human condition, education is likely to remain an upbringing in place, through place, and partly, therefore, about place. All of this indicates that education, and hence schooling, can prospectively serve the particular places that sponsor education.

Why not? As one of us points out, the emphasis on individualism that is embedded in schooling deforms rural places (Theobald, 1997). An education that serves community and place exists; it is arguable, defensible, and many

believe it necessary. Can this project be joined by scientifically grounded research? Certainly. Can rural schools be refashioned to match the terms of the science? Doubtful, but in this instance the insufficiency of the science harbors a silver lining: good research along these lines will foster more thoughtful engagement with the important issues, and with rural meanings—those very meanings that our rural doctoral students have so seldom encountered in their schooling.

Rural education research simply *must* ask what sort of schooling rural kids are getting, why they are getting it, who benefits and who gets injured in the process, and by what mechanisms. What to *do* about such matters may emerge in the related conversations, and not just among an elite composed of scientists, bureaucrats, and politicians—but among rural citizens most prominently.

Critique as the Goal of Good Rural Education Research

Because of its concern for rural meanings, for context deeply engaged, rural education research cannot properly be confined, as we have asserted, to evaluating "what works" for rural students. Indeed, it is extremely doubtful that a simple and powerful rural marker equivalent to poverty or race exists for individuals. Chasing that marker and its hoped-for influence is, well, bad science. There's little evidence for such a quality, and so, even less for the existence of effects related to the quality.

But if the project of education research expands to "what works for rural communities," and simultaneously allows the "what" under scrutiny to include a wide array of local ways of being—and not just products brought to the market by academics, politicians, or corporations—then it moves into the realm of research proper, which for us three has always been *critique*.

Now, "critique" occupies a troubled place in science because the word in ordinary usage connotes carping, complaining, and whining—criticism of the worst sort. This, of course, is not the meaning we intend; *critique* in the academic sense refers to the exercise of careful judgment, especially in extended discussion. Of course, the construct of "what works" is also subject to doubt, and not surprisingly, the nexus of critique and science is *doubt*. Doubt sponsors critique; doubt is intuitive—a stance toward claims and appearances; critique is part of the method for dealing with doubt.

So is good science, but so much discussion of science these days omits discussion of doubt. The omission is puzzling. Shavelson and Towne (2000), for instance, use the word only three times, not one of them substantive.² We

²(a) "this book will no doubt incite debate" (p. viii); (b) "the results cast doubt" (p. 56); and finally, "analysis . . . raised doubts" (p. 60). (Results obtained from the uncorrected machine-read file available at <http://www.nap.edu/books/0309082919/html>.)

imagine that the want of doubt in discussions of science may be the complement of the very evident great faith in the progress issuing from science (Shavelson and Towne's report uses the word "progress" 55 times). Much more could be said on this topic, but a problem of doubt may well exist. There certainly is evidence of such a problem in this one book so important to education research.

In good science, doubt needs free reign. That "free reign" is the realm of critique, and such scope, both for motivating study and for carefully considering the objects or findings of research, is peculiarly necessary in education research because the means and ends of education can, with propriety, be freely chosen, even idiosyncratically chosen, quite apart from the findings of any rigorous science of effectiveness. Narrowly defined effectiveness is not immaterial, but it is not nearly *enough*.

The work that most needs doing is not only critical, it earnestly conforms to the mission of providing description, establishing systematic influences, and providing explanations. To be clear: We're not arguing here against objectivity, competence with evidence, or grasp of and contribution to theory. We're arguing *for* those things (as our doctoral students will readily attest.)

The Critique of Good Method

An insufficiently critical notion of progress is in play not only with Arnold et al. but with IES, and also with the generally admirable statement of the National Research Council's Committee on Principles of Scientific Research for Education (Shavelson & Towne, 2000). Most surprising is Shavelson and Towne's (2000) weak engagement of the issue: they argue conventionally that progressive knowledge is a hallmark of science, even social science, and even "education science," but they ignore the work of such moderate critics as Christopher Lasch and James Scott. Although we don't dispute the NRC's principles—they are among the things we tell doctoral students—we also help our students consider the nature of reality and the varied ways in which it might be known or elude being known. Just like good scientists, we're suspicious of definitive knowledge.

For research in any field to do some good, to arrive at unsuspected insights, and to achieve breakthroughs, it needs to be prescient. That prescience comes from ideas, whether the research method be experimental, causal-descriptive, ethnographic, historical, or philosophical. In education, we need them all because no other field contains and reaches all of humanity so extensively and with such substantial promise and such persistent disappointment.

Even from a purely technical perspective on quantitative research, however, Arnold et al. miss the boat. Their most serious lapse concerns the miracle of random assignment.

They, like others in the IES camp, assume that random assignment is sufficiently robust to account for students' differences. Interestingly, however, reliance on random assignment accounts for differences not at all: it simply embeds the assumption that student differences will "even out" in large enough groupings chosen at random.

With stable and replicable treatments (a condition that a large body of research on veracity of treatment suggests still falls within the realm of wishful thinking), true experiments are therefore supposed to show which treatments have the best effects with children in general. Researchers from Cronbach forward, however, have learned that treatments work differently with different types of children—that, in Cronbach's words, there are significant "aptitude-treatment interactions."

Moreover, as quantitative researchers who deploy hierarchical linear modeling methods have found, there are also consequential interactions among student characteristics, and between and among student characteristics and the characteristics of classrooms, schools, districts, and communities. Quantitative research that might try to account for all of these interactions (as well as significant main effects) might be said to "model" real schools in ways that are far more compelling than the "high quality" comparative studies of educational processes and outcomes in rural versus nonrural schools that Arnold et al. extol. Whereas experiments strive for simplicity and clarity, the character of the real world requires examination that is both complex and nuanced. (Note that this form of quantitative work is arguably similar to the notion of "critique" given in the previous section in that it provides freer reign to careful consideration of complex matters.)

Moreover, well-specified regression models that incorporate a complement of robust explanatory variables suggest causality more convincingly than experiments in which necessarily small groups of randomly assigned students pose as archetypes for all students in all places at all times. In fact, conventional usage of regression research often accepts directionality in a tacit way as causal. No one, for example, ever mixes up the direction of the relationship between students' prior and present achievement.

Perhaps the current infatuation with true experiments in education, rather than representing a strategy for increasing academic rigor, simply represents a politicization of the long-standing academic rivalry between psychologists and sociologists. Studies of context inevitably draw on sociology, and as a result, they are at the moment out of favor with a regime that believes that intellectual accomplishment is represented in test scores, finds that teaching is an educational intervention, and generally takes context as an impediment to learning rather than as a motive for learning and source of meaning.

Finis: Getting Personal

The three of us do varied work for a center whose research mission espouses an aim compatible with the one articulated in this reply. The center is funded by the National Science Foundation, and it's being put out of business by the folks bringing us just the sort of neat and narrow version of scientific research in education adopted for the purposes of the Arnold et al. review. But this isn't personal; not yet (keep reading).

We've spent careers, according to Arnold et al., motivated by a belief "that may [not] be valid" (p. 1). It's interesting, but not exactly comforting, to know that a *rural education research team* has set pen to paper and rubber to road to travel to an IES-approved destination: fawning over the agency's definition of scientific rigor. Quite likely, though, the necessary validity for this trip consists more of economic incentives than the findings of neutral science. Some of us remain economic determinists, and such people will understand the strength of such motive in such a case.

In 1913, Joseph Mayer Rice published a book entitled *The Scientific Management of Schools*, in which he called for three reforms: (a) the identification of agreed upon learning standards, (b) scientific pedagogy, and (c) accurate, scientifically-derived measures of learning. The educational agenda of the current administration, in typically American fashion—that is, as if history did not exist—is the same as one advanced nearly a century ago. For almost 100 years, we have been looking in vain for undiscovered interventions or treatments that will now, at last, reliably cause results. The reappearance of this vision will, we predict, do considerably more mischief than those who sponsor it, approve it, or silently tolerate it realize (e.g., see Scott, 1998).

An ignorant mainstream, in fact, routinely presumes that rural schools (and cultures) are deficient, almost necessarily so. The rural outlook on living well is so different from the mainstream (suburban) norm that it is vilified and romanticized, and rarely understood or authentically appreciated by outsiders. We take this fact personally, because not only have we spent *careers* based on beliefs unsubstantiated by science (the charge leveled against us), but because we've spent *lives* in rural places, among rural people, doing rural things. We like these places and these people and this work. We're staying put.

Arnold et al. don't, of course, go terribly wrong within their frame of reference, which is that of the funding agency (IES). The standard disclaimer in their case is solidly ingenuous—no, this article's extremely earnest valuation of experiments has no ontologically *necessary* connection with Russ Whitehurst's grand design for education research. But it has an evident one; either that, or the coincidence of values is, well, surprisingly accidental or rigorously principled. Readers can decide which of these three alternatives³ obtains, and doubtless will.

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³That is: instrumental, purely accidental, or principled.

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