

What Future For Social Science?

The three authors whose books are reviewed here have very different backgrounds and orientations, but they would agree on this much: Educational research modeled on quantitative research in the social sciences, which is itself modeled on research in the physical sciences, is at best a futile pursuit and at worst an exercise in domination. It is ironic that all three authors, David Scott, Angela Brew, and Bent Flyvbjerg, perceive quantitative, applied science to be in a state of crisis at the very moment that the recently passed Reading Excellence Act is urging that government funding be granted only to "scientifically based research."¹ Each of our authors would deplore this policy. Their criticisms of social research based on mainstream science are not dissimilar, nor are they without merit, but they differ in what they would offer in its place, and what kind of warrantability their alternative view of research offers. Can researchers avoid the dominant quantitative paradigms and still have something valuable to say to policymakers?

David Scott's *Realism and Educational Research* is written in the face of the United Kingdom's efforts, not unlike those in the United States, to make educational policy decisions responsive to the results of "scientific" research.² In the United Kingdom, according to Scott, the "dominant ideology of positivism/empiricism has become even more dominant." Although it is often hard to track the specific arguments in many of Scott's chapters (which range over topics, some of which, like the recent history of educational policy in the United Kingdom, show little apparent connection to either realism or educational research), it is fairly easy to identify the genre of research he deplores - the atheoretical, quantitative, randomized trials of curricular or pedagogical programs now also in vogue in the United States. While never presenting any illustrations of exemplary research, Scott is explicit about the dimensions of social reality a scrupulous educational researcher in search of understanding ought to address. These include a focus on both structural properties and interpretations of those properties by individual actors; the relations between different structures at different points in time; the intentions of individual actors; the intended and unintended consequences of actions by individuals and their effect on structural properties; and, finally, what he considers "the crux of the matter": "The degree of structural influence and the degree of agential freedom for each human interaction" (RER, 35, emphasis added). Now, talk of degrees of structural influence or degrees of agential freedom is out of place unless there is some way of identifying, indeed of measuring, "more" and "less." Hence, it looks to me as if even Scott's Utopian research program cannot escape the need for quantitative inquiry involving ordinal, if not cardinal, measures of some kind.

Scott devotes a chapter to critiquing "school effectiveness" research, influential on both sides of the Atlantic. The strategy behind this stream of research is first to identify schools that produce unusually positive results and then to try to identify the factors that produce those results. Scott's critique of the tacit normative commitments of this literature, while it makes no claims to originality, is sound nonetheless. To summarize two points, Scott argues that calling a practice "effective" in this context can mean no more than that it has

been linked statistically to a designated outcome - typically academic achievement as measured by tests - but the term "effective" is often interpreted in a global sense by readers of this research and even by the researchers themselves, an inference that is completely unwarranted. The second point is that the rhetoric of "effective schooling" often assumes a particular school organization with a manager directing the work of teachers in a hierarchical fashion - a form of organization that is strongly contested by promoters of teacher professionalism, including many teachers' associations.

Scott deploys the realist framework of Roy Bhaskar, itself derived from Rom Harré's pathbreaking 1972 book *Philosophies of Science*, to lodge another criticism of the quantitative paradigm in educational research.³ This paradigm, argues Scott, conflates the identification of an "independent" variable statistically associated with some "dependent variable" with the cause(s) generating that particular outcome. Scott's elucidation of the distinction is not as clear as it might be, but here is what I think he means: When, to take his own example, poverty is statistically linked to low reading scores, this cannot mean, strictly speaking, that lowering family income below some threshold produces depressed reading achievement in the way that, say, cooling water below a certain temperature produces ice. If it did, all poor children would do poorly in school, which we know is not so. According to Scott, the correlation does not really help us understand the mechanism that causes many children from families below the poverty line to perform less well on reading tests.

I think Scott is right about this, but what follows? Two quite different kinds of response are equally plausible: A conservative's response might be that a policy designed to raise poor children's achievement ought to wait until a better understanding of the precise mechanism linking level of income to reading achievement is provided. Another possible response, just as warranted, is that one way of increasing the likelihood that poor children will read proficiently is by providing their families with incomes sufficient to take them out of poverty. My point is that successful intervention does not have to wait for an understanding of the mechanism that produces reading failure among the poor. To take an analogy, policymakers could be confident that a reduction in smoking would lower the incidence of lung cancer long before the precise mechanism by which smoking produces cancer was discovered.

As I have noted, Scott identifies no research exemplars, but he makes it clear that qualitative investigation focused on the lives of individual actors is at least an important piece of the puzzle. He devotes one whole chapter to it. According to him, biography and autobiography allow "appreciation of the driving force of society - the complex interactional activity of a number of individuals, all seeking to create and recreate themselves" (RER, 95). Clearly no researcher calling himself a realist can overlook the way individuals understand their situation (for this determines what actions they take), but neither can a realist take actors' interpretations as the last word, because actors often misconstrue their situation. Following H.G. Gadamer, Scott seeks to resolve this tension by viewing the research process as a dialogue between author and subject, one in which "a consensus can be achieved despite differences - indeed because of differences" (RER, 110).

He illustrates this approach in his presentation of the life narrative of Mary O'Brien, an Irish teacher working in a London comprehensive school. O'Brien's professional self-image changes over time as a result of her experience during a career spent in different schools that operate in different political climates. In the current competitive climate, she notes that teachers at other schools who, in a previous era, would have served as sources of information and allies are now

viewed as competitors: "we all have to be concerned about the schools' image, and there are very, very crude marketing ploys that we have to implement" (RER, 109).

Does such a narrative speak to the concerns of policymakers? Indeed it does, for it identifies consequences of accountability regimes that might have gone unnoticed - consequences that may, indeed, subvert reformers' intentions. Here we see how the competition among schools may cause teachers to abandon useful sources of practical guidance - their peers in other schools. But the narrative speaks to policymakers only on the assumption that O'Brien's experience is typical, if not of all teachers, then at least of some identifiable subgroup, something Scott tacitly assumes without evidence - otherwise why present the case? But how could evidence for O'Brien's typicality be found? Only by surveying a number of teachers, selected in such a way that inferences to some larger population is shown to be warranted. If the teacher or student who is the focus of a case study is anomalous, any policy based on its conclusions is bound to lead us awry. The point here, one I think Scott would not reject, is that policy for an entire system cannot rest on one or two case studies in the absence of a warrant that their findings are replicable in other settings. The search for such a warrant takes us back to quantitative inquiry.

The need to introduce quantitative considerations comes through once again in the subsequent chapter focused on researching race and ethnicity. Scott here makes the point that anecdotes showing individual teachers displaying overt racial prejudice are not sufficient to convict an entire system of discrimination. The definition of discrimination must incorporate a "dimension of scope," differentiating between "individual and isolated cases," "a large number of cases," "all teachers and thus all cases," and "institutional arrangements" that permit or do not permit teachers to resist them (RER, 116). Here again, it seems that quantitative inquiry is unavoidable if we are to determine what degree of discrimination exists in order to identify the appropriate response.

The point I have been stressing is simply that even a scholar who is highly critical, often rightfully so, of quantitative inquiry in education cannot avoid answering the questions of how many? how often? and how much? if he or she is to address policymakers.

Angela Brew's concerns in *The Nature of Research* are far broader than Scott's, encompassing the entire university within a broad political and economic context.⁴ Where Scott's critical energies are focused on scientism in educational research, which I define as the importation of scientific procedures to domains where they are inappropriate, Brew would challenge the scientific enterprise itself. The data that provide the springboard for her reflections are transcripts of interviews with researchers in a variety of fields concerning their work and their own understanding of the nature of research. Like many scholars, Brew is concerned about the way that funding sources are driving the research agenda and is troubled by the fact that universities are coming to speak the same language as business corporations and government agencies. Her diagnosis is centered not on the influence of funders and administrators, however, but on scholars' own conceptualizations of their work. She contends that the Western tradition of research is itself defective. Concepts such as objectivity, falsification, critical distance, methodological rigor, consistency, separation of knower and known, contributions to knowledge reflected in numbers of publications, and competition of individuals and laboratories for primacy may once have been liberating, but they now are suffocating. What does Brew propose to install in their place? I quote her observations at some length so you can appreciate the flavor of her writing:

In the emerging traditions of what I have called experiential research,

truth and validity are not necessarily achieved at all stages nor are they an outcome. They are instead processes to be aimed for....Traditional knowledge is inherently conservative. What is replacing it is dynamically radical and progressively transformative....In the new form of research we see that the emphasis has shifted towards examining the world-as-experienced; towards the idea that reality is a construct; and towards a blurring of the distinction between subjective and objective worlds. For while traditional research has resulted in a great deal of knowledge about the latter, it has, by denying subjectivity, meant that we are still very ignorant regarding the processes of human subjective experience. We have not learnt how to live. Research has told us very little about how to be happy and almost nothing about how to be wise [NR, 101, 104, 140).

Such sentiments are suitably reinforced by citations to Jean- Francois Lyotard, Paul Feyerabend, and other theoretical masters, whose conclusions are assumed to have been demonstrated. I would hope that merely citing such passages would be enough to discredit them; still, we need to ask, what precisely is Brew proposing?

If Brew wanted to see university professors committed to exploring human subjective experience, perhaps she was just looking in all the wrong places. She need have looked no further than the music and art departments; the literature, creative writing, and poetry programs; and the drama, dance, and other performance departments. Are not Marcel Proust, Paul Klee, Virginia Woolf, Bela Bartok, and Martha Graham among the greatest twentieth-century explorers of subjective experience? Perhaps she would like universities to offer more opportunities for students who lack artistic talent to explore their own subjective worlds and experiences. But these opportunities are also available in the counseling center, where there is no demand for objectivity or critical distance, where the process is all, and where truth and validity fade into the background.

But this is not what Brew has in mind. Unlike Scott she does offer a model of her ideal, one found in a doctoral thesis "on the role of imagination in how individuals construct their own personal autobiography" (NR, 58). The author, Alex Nelson, studied the way he and five other Roman Catholic priests understood the "remarkable" change that occurred in their lives when they decided to marry. (Whether they were forced to abandon the priesthood is not clear to me from Brew's account.) One device Nelson used to elicit their stories was to encourage "each to tell their life story in the form of a parable," one of which Brew reproduces (NR, 58). What are we to make of such "progressively transformative" scholarship?

It might or might not be therapeutic for us to recast our life stories as parables, but why should we view this collection of parables as a contribution to scholarship! Surely the answer would need to have something to do with the patterns Nelson discovered in the parables - patterns that reveal something more general about the way minds work (not necessarily all minds, but at least those of people whose lives took a certain turn or shared a certain experience). Suppose, however, that there are no discernible common patterns at all. That, too, could tell us something about human individuality and creativity, but note that such a conclusion would also require an inference from these six to other human beings. The value of the study would then lie in showing that even six individuals who might be expected to see the world the same way viewed it idiosyncratically. This might be a provocative general hypothesis worthy of further investigation.

Suppose Brew were to say, "Stop harping on the notion of generalizability and just take the texts as revealing how six fellow humans make sense of their lives. Don't glimpses into the minds of six unusual individuals have any value to you?" In effect, she would be

asking whether there is anything wrong with a text that announced at the outset, "Here is how I, Alex Nelson, and five other former priests tell the story of our lives." To be sure, Alex Nelson's collection of life parables might make fascinating reading, but if that were all there was to it, why think of the collection as a contribution to scholarship? And why would one award a doctorate to someone for facilitating and transcribing six autobiographies? Brew hopes that work like Nelson's will receive greater legitimacy in the university. I, myself, suspect that, were taxpayers or other supporters of public universities to believe that dissertations like Nelson's were becoming more common, they would withdraw their increasingly tentative support.

I will give more space to the third of the works under review, because it is by far the best. Indeed, the other two books are often beset by clumsy writing and shoddy or nonexistent editing. Bent Flyvbjerg's *Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again*, has, as its subtitle indicates, a critical section and a constructive one.⁵ A Danish scholar, Flyvbjerg is enormously erudite, as conversant with continental as with Anglo-American theorizing about society and social science. Like our other authors, he sees mainstream social science as failing to enhance the values of freedom and democratic participation that most of its practitioners profess to support. But he goes further to argue that mainstream social science has never made good on its promise to generate robust epistemic theories with genuine predictive power, nor is it likely to do so. Flyvbjerg bases his diagnosis of social science's limitations primarily on the work of Hubert Dreyfus and Pierre Bourdieu. The argument is intricate and sustained over several chapters, but its gist is that social science theories are necessarily context-bound in a way that natural science theories are not. It is not simply that generalizations about human behavior hold only under certain conditions, for this is true, as Flyvbjerg acknowledges, of generalizations in ecology as well. The difference is that generalizations such as the latter are describable in terms of objective states of affairs while the former depend on human interpretative skills that cannot be explicitly formulated in propositional form. He uses an example from Bourdieu to make this point vivid. Suppose A gives an object to B, and at some later time, B gives an object to A - have they exchanged gifts? It would seem so, but whether the transfer constitutes an exchange of gifts depends on a variety of presuppositions concerning the transactions. For example, one must assume that the two objects are different: if they were one and the same, we would have no exchange of gifts at all; B's return of the object might be interpreted as an insult to A. According to Flyvbjerg (following Dreyfus), all socialized natives have the practical, intuitive understanding of when a transfer of objects from one to another constitutes a gift, but they cannot articulate that understanding as a set of rules, because there are no such rules. Hence, Flyvbjerg argues, the social scientist cannot articulate them either.

If mainstream social science is at a dead end, what is to replace it? Flyvbjerg articulates an alternative model, which, drawing on Aristotle's distinction between *episteme*, *techne*, and *phronesis* (loosely, scientific knowledge, craft knowledge, and practical wisdom), he labels *phronetic social science*. *Phronetic social science*, inspired more by Michel Foucault than by Aristotle, eschews the discovery and formulation of general epistemic claims about the workings of societies and rejects the stance of a disinterested spectator. Its inquiries are local and contextual, focused primarily on specific practices that reveal the way power operates to obscure or impede a more ample participation by more diverse groups of citizens in determining their collective fate.

The *phronetic social scientist's* final product takes the form of a narrative case study based on archival research, interviews, and participant observation. It employs quantitative and qualitative

methods as called for by the problem, and ongoing reports to be shared with all those who have a stake in the process. So far, the model has much in common with critical ethnography as practiced by some educational anthropologists. There is a crucial difference, however. The phronetic social scientist may play an active role in the process he is studying. Flyvbjerg intervened, for example, to facilitate dialogue among stakeholders who were at loggerheads: "I recommended a set of policy and planning measures for how city governments may significantly reduce their risk of ending up with...[the sort of] undemocratic policies seen in Aalborg" (MSS, 160).

What is taken from Foucault, besides a wish to focus on the micro processes of power operating in specific contexts, is a special interest in the way that power and rationality are related, the way in which power determines which rationality will reign in a particular context. When talking as a theorist, Flyvbjerg adheres to Foucault's Nietzschean notion that there are no bare facts observable from some neutral ground, only interpretations from particular perspectives, as well as the notion that the interpretations that prevail and, in a sense, create social reality will be those that the powerful impose. According to Flyvbjerg, his own view contrasts with that of C. Wright Mills and others who think that "speaking truth to power" is a realistic possibility.

Flyvbjerg situates and defends his model in six chapters that cover a lot of ground in a masterful fashion. Here I will merely list the central topics: the nature of values in social inquiry, a defense of the case study, an analysis of the debate between Jurgen Habermas and Foucault on impediments to a more democratic society, guidelines for phronetic social science, and a detailed account of a phronetic investigation conducted by Flyvbjerg himself.

I found the illustration immensely helpful in gaining an understanding of what the phronetic alternative looks like in practice, so let me very briefly describe it. Flyvbjerg studied an urban renewal project in his home city of Aalborg for a period of "almost fifteen years." The case presents a classic contest between the Chamber of Industry and Commerce's vision, which favored greater automobile access to the downtown shops, and the conviction of the planning committee and other constituencies that an increased automobile presence would undermine the vitality of the historic cultural and commercial district.

Flyvbjerg's investigations led him to discover a number of ways by which the chamber was able to dominate the planning process, excluding some legitimate stakeholders from important decisions and framing the issue in a way suited to its interests. He documents the way statistics were collected, marshaled, and disseminated to support these interests. One of the publicly stated aims of the project was to enhance the safety of bicyclists, but Flyvbjerg was able to document a steep rise in bicycle accidents following implementation of the plan.

Flyvbjerg also describes how his study of the bicycle accidents was challenged by an alderman during a radio interview. The alderman "said to the nation that he held proof that my numbers were wrong." Following the broadcast, Flyvbjerg sent the alderman his raw data and analyses, asking the alderman to identify his errors. When the alderman's staff failed to do so, the alderman felt obliged to retract his accusation, and the entire story received nationwide publicity.

Why do I describe this incident in such detail? Because it seems to me to be an example, precisely, of "speaking truth to power." Flyvbjerg cannot have it both ways: he cannot claim that power produces rationality - that "the decisive aspect in relation to the fate of the Aalborg Project is not whether the one or the other interpretation is 'correct' or 'rational' but which party can put the greatest power behind its interpretation" (MSS, 153) - and at the same time claim that "We

[scholars] are paid to be that group in society which is best equipped to produce data, knowledge, and interpretations of the highest validity and reliability" (MSS, 157).

Flyvbjerg takes pride in the way his release of the study of bicycle safety challenged the case made by the powerful chamber. I would argue that it did so, however, not through the imposition of an alternative "rationality," but simply through the release of facts whose relevance and interpretation were difficult to challenge regardless of which side one supported. Indeed, were one to accept at face value the doctrine that power produces rationality, one would be denying the very promise of phronetic social science, whose point is to challenge power by rational means.

I think Flyvbjerg's Foucauldian talk of "power producing rationality," far from being the expression of a deep philosophical insight, is simply a way of giving a philosophical spin to what most of us would recognize as a political commonplace: that interests arrayed on different sides of an issue will attempt to frame the issue in a way that furthers their interests (think of the abortion controversy, for example, or disputes over taxes or school vouchers) and that they will supply evidence, or construe available evidence, in a way that supports their position and discredits that of their opponents. It is possible to imagine that a dominant or insurgent group might try to legitimize or discredit an entire genre of investigation or analysis (which might arguably be labeled a "rationality"), but Flyvbjerg produces no evidence that this happened in Aalborg.

How, then, are we to judge phronetic social science? Let us remind ourselves of its purpose, according to Flyvbjerg:

[T]he purpose of social science is not to develop theory, but to contribute to society's practical rationality in elucidating where we are, where we want to go, and what is desirable according to diverse sets of values and interests. The goal of the phronetic approach becomes one of contributing to society's capacity for value-rational deliberation and action (MSS, 167).

This aim appears admirable to me; there is something both sound and appealing in its focus on enhancing the conditions needed for wise and democratic decision making in local contexts. In education especially, where generalized solutions seem to fail on a regular basis, and where there is a rich tradition of critical ethnography and of teacher research (including what is sometimes labeled action research), Flyvbjerg might be taken to provide the theoretical underpinning and legitimation that these genres seem to need.

What makes me uncomfortable is that so many diverse kinds of texts could plausibly fit his general description as to empty the word "science" of any meaning at all. Even journalism in the muckraking tradition seems to warrant inclusion. Take, for example, Barbara Ehrenreich's *Nickel and Dimed*, her personal account of working in several low-wage jobs for months at a time in different parts of the country and trying to live solely on these low-wage salaries.⁶ Ehrenreich documents many specific practices by powerful employers that violate the dignity if not the legal rights of employees. Indeed, Ehrenreich's book seems to reinforce Flyvbjerg's conviction (taken from Foucault) that "Understanding how power works is the first prerequisite for action, because action is the exercise of power. And such an understanding can best be achieved by focussing on the concrete" (MSS, 107).

Suppose Flyvbjerg were to argue that, while this text does contribute to our collective thinking about what is desirable and what we should do, it fails to qualify as phronetic social science because it does not

manifest the social scientist's *techne* (craft) of investigation. This response, in turn, would raise the question of what is distinctively valuable about this *techne*. Surely the answer must be that it is "best equipped to produce data, knowledge, and interpretations of the highest validity and reliability."

Here we see a potential tension between the demand for valid and reliable knowledge and the phronetic social scientist's commitment to democratic values. Suppose, for example, that Flyvbjerg's analysis of the accident data had supported the allegations of the powerful chamber. Would he have been obliged to report it (or to suppress it for the sake of advancing his political aims)? Such a potential tension exists for any mode of inquiry that appears to know what its investigations will uncover. I only wish Flyvbjerg had addressed this issue directly.

In his conclusion, Flyvbjerg says that the only alternative to phronetic social science

will consist of a "headless" form of ad-hoc social engineering no longer given credence by a superstructure of social-science theory. It will instead be dictated by a functional means-rationality defined by the ruling relations of power. This kind of practical social-science activity does not require advanced graduate and postgraduate specialized institutions of higher learning (AiSS, 167).

Here, I think Flyvbjerg underestimates the potentialities of mainstream social science theory, even of the most decontextualized kind, to generate "engineering" solutions to specific practical problems - solutions that do more than serve the interests of the powerful. Let me offer one illustration: the design of markets. Alvin Roth, professor of economics at Harvard University, is a leader in the application of game theory to solve practical problems of market design. In 1998 Roth redesigned the national clearinghouse that matches about twenty thousand physicians per year to their first job. Roth has employed a combination of abstract game theory, analysis of empirical data, and computational experiments and exploration - all activities that require a very high level of expertise - to design algorithms that permit matches of individual physicians to hospitals in a way that minimizes the likelihood that either employer or employee will fail to find an acceptable match or that some employer and employee would prefer to be paired with each other rather than with the match assigned them by the computer.⁷

A proponent of phronetic social science might say that this kind of application of economic theory fails to challenge instrumental rationality, and fails to open up the question of values. To some extent, this is true; the market designer simply assumes that preference satisfaction is a good thing, just as Flyvbjerg simply assumes that greater participation by stakeholders in public planning is a good thing. But the market design is not indifferent to issues of value, since the creation and redesign of the clearinghouse was based precisely on a realization by all parties (not just the employers) that the existing system was not only inefficient but provided incentives for participants (on both sides) to engage in strategic behavior that worked to the detriment of those who participated in good faith.

Finally, I raise a question about Flyvbjerg's work similar to the one I raised about Scott's narrative of Mary O'Brien and Brew's exemplar of the priests' autobiographies. Flyvbjerg mentions that his original report on the Aalborg project filled two volumes. My question is why anyone (besides local residents who want to know more about the "backstage" behavior of those they read about in the newspapers) would want to read a two-volume work on events transpiring in a medium-sized Danish city unless it carried at least a pretense to episteme - that is, to

an understanding of the operation of power that transcended that particular situation? I think that Flyvbjerg himself would contend that his study is revealing about the operation of power, not just in Aalborg, but in many other "locations." After all, in supporting the case study method, he argues,

One can often generalize on the basis of a single case, and the case study may be central to scientific development via generalization as a supplement or alternative to other methods. But formal generalization is overvalued as a source of scientific development, whereas, "the power of the good example" is underestimated (MSS, 77).

Flyvbjerg would quickly add that his aim in Aalborg was not to formulate a general theory of the way power operates but to provide an exemplary thick description at the level of concrete practices - a description that would provide investigators in locales, far and near, with clues that could assist them in pursuing their own investigations. My point here is not to criticize Flyvbjerg but to point out that the gap between epistemic and phrenetic social science may be somewhat narrower than he supposes.

I think most readers of this review would welcome and applaud the efforts of these three authors to demonstrate that the emperors of "scientific research" parade naked. Some may view Flyvbjerg's excellent book as legitimating the critical work they themselves undertake or value. But here it is worth reminding ourselves of a crucial difference between Flyvbjerg and most other critical researchers: Flyvbjerg not only studied a planning process in a single context over a decade and a half; he participated in rendering that very process more transparent, more accountable to the citizens of Aalborg. Perhaps this model of intensive study and intervention is one in which American scholars of a critical bent could find inspiration.

Is the phrenetic approach the way for the social sciences to redeem themselves? I think that Flyvbjerg has certainly presented a persuasive case for the adoption of an alternative model, and he has provided strong legitimation for an oft-discredited kind of inquiry. But I doubt that many epistemically oriented social scientists (economists, to take the most obvious example) would find Flyvbjerg's arguments so persuasive as to weaken their commitment to formulating abstract, relatively context-free models of human behavior - models designed, presumably, to explain if not to predict actual behavior in specific contexts. Such models, scholars like Roth would contend, provide the framework without which real-world applications could never be designed. Whether drawn to phrenetic or epistemic social science, any social scientist with a disposition to inquire into the foundational assumptions of his or her practice would benefit from studying Flyvbjerg's book. Any philosopher of social science would find the lucidity and sustained vigor of the argument, the balanced and nonpolemical tone of the text, and the original reconceptualization achieved by combining the ideas of philosophers from disparate traditions, an unusual treat.

I mentioned at the outset that each of the authors discussed here is, at least in part, reacting against the current attempt, both here and in the United Kingdom, to bring a particular model of social science to bear on the improvement of schooling. I sympathize with those trying to follow the lead of the "harder" sciences, those who want to eschew ideology and opinion and get "hard evidence" about the consequences of the policy choices that face us. Is it not, after all, the task of science to serve as impartial jury, to help us overcome our local and subjective biases and predilections? Since almost all our experience is confined to local contexts of doubtful universality, is not science needed precisely to guide our decision making on the local, state, or national scale? If the variability of contexts makes more generalized theory impossible,

does it not, by the same token, render "intuitive" judgment of broad policies suspect? Yet I also identify with these authors in arguing that the rhetoric of "scientifically based research" is itself a weapon in an ideological struggle waged by powerful interests on behalf of a very specific and limited conception of certain kinds of learning designed to serve certain kinds of ends.

Here is a context where talk about competing "rationalities" is not out of place. Backers of the "scientific" approach legitimately ask, In the absence of impartially weighed evidence concerning their outcomes, what besides fashion, intuitive attractiveness, or, indeed, power, would warrant preferring one educational approach or way of organizing schooling to another? Those who challenge the "scientific" approach legitimately ask, In order for policy choices to be "scientifically based," does not the requirement that evidence from many schools be rendered comparable and commensurable inevitably reduce the range of outcomes considered worthy of recognition to those that can be represented numerically (most commonly, test scores)?

I believe these competing "rationalities" are unlike those Flyvbjerg, following Poucault, identifies in one important way. In their accounts it makes sense to identify specific "rationalities," specific ways of framing problems, with particular interest groups or social classes. In the case I have just described, however, it may be true that teachers or corporate CEOs are disposed to favor one way of thinking to the exclusion of the other, but that is not the deeper truth, which, I believe, lies in the fact that both rationalities struggle for hegemony within the minds of thoughtful people.

Is there a way to reconcile these warring "rationalities"? Stanford University political scientist David Laitin suggests one in his own review of Flyvbjerg's book:

I...propose that within the scientific frame, a tripartite methodology that includes narrative (the essential component of phronesis), formal, and statistical analyses is the best defense we have against error and the surest hope for valid inference....The problem with good judgment resting only on one leg of the tripartite method (exemplified in Flyvbjerg's rendition of phronesis) is that it is hard to know if one's judgment is wrong.⁸

Laitin provides some reasons and a couple of examples which are designed to show that the combination of three very different methodological approaches to the study of any single social phenomenon may succeed where any one approach alone is doomed to fail. Perhaps he is right. Unfortunately, I can think of no scholar of education equipped to carry out such tripartite investigations. But even if there were, this would hardly lead us to the promised land, for, as Laitin remarks (here unwittingly allying himself with Flyvbjerg even though he criticizes him), "Never in social science is all variance explained, and even in powerful models, the amount that we are able to explain is often paltry."⁹ This admission, coming from a social scientist committed to the formulation of rigorous causal explanations of social phenomena, to epistemic as well as useful social science, should stimulate a certain level of modesty among mainstream social scientists. Were such displays of modesty more common, our three authors might not have felt such a strong need to write the three treatises reviewed here.

1. Aee Debra Viadcro, "Ed. Dept. Quietly Funds More Experimental Studies," *Education Week*, 11 December 2002: 1, 12.

2. David Scott, *Realism and Educational Research: New Perspectives and Possibilities* (New York and London: Routledge palmer, 2000). This book will be cited as RER in the text for all subsequent references.

3. Roy Bhaskar, *The Possibility of Naturalism: A Philosophical Critique of the Contemporary Human Sciences* (Atlantic Highlands, New Jersey: Humanities Press, 1979). See also Rom Harré, *The Philosophies of Science: An Introductory Survey* (New York and London: Oxford University Press, 1972).

4. Angela Brew, *The Nature of Research: Inquiry in Academic Contexts* (New York and London: Routledge Palmer, 2001). This book will be cited as NR in the text for all subsequent references.

5. Bent Flyvbjerg, *Making Social Science Matter: Why Social Inquiry Fails and How It Can Succeed Again* (Cambridge: Cambridge University Press, 2001). This book will be cited as MSS in the text for all subsequent references.

6. Barbara Ehrenreich, *Nickel and Dimed: On (Not) Getting By in America* (New York: Metropolitan Books, 2001).

7. See Alvin E. Roth, "New Physicians: A Natural Experiment in Market Organization," *Science* 250 (1990): 1524-1528; and "Game Theory as a Tool for Market Design," unpublished manuscript, 1999, available at . Last accessed 6 October 2003.

8. David D. Laitin, "The Perestroika Challenge to Social Science," *Politics and Society* 31, no. 1 (2003): 169.

9. *Ibid.*, 178.

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