

The Light Bulb Test

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My vision of the sociology of education comes out of identification with the aspirations of the nineteenth-century founders of sociology, many of whom, in Comte's ([1851–1854] 1896) words, saw the new discipline as the “queen of the sciences.” For this founding generation, sociology could aspire to be the most important of the social sciences because it took the widest view, focusing not on a single set of social institutions but on all social institutions examined in comparative and historical perspective. Many of my teachers at Berkeley and Harvard shared something of this aspiration, and it was consequently easy for me to maintain it, however at odds it may have been with the main lines of development of the specialization area in which I was most interested. For someone who comes from this tradition, the questions are not, “What is the government funding now?” or “What is the most efficient way to obtain tenure?” Instead, the questions are, “How have human beings lived together over the centuries under different demographic and different technological conditions?” “What institutions and cultural understandings have they erected to organize action?” “How have they sustained social order?” and, “How and why did their institutions and social relations change over time?”

When this perspective is applied to the institutions of schooling, a comparative and historical framework is clearly important. We would want to investigate, as Max Weber did, what the educational experience of sons of aristocrats trained for leadership and heroism looked like, what the education of mandarin Chinese preparing for a place in the imperial bureaucracy looked like, what the education of a middle-class German brought up under the philosophy of *bildung* looked like, and what the education of technical specialists in the age of bureaucracy looks like. And of course we would also want to look beyond the education of elites, as Weber failed to do. We would want to draw on those who studied the Lancasterian system of drilling the urban poor in industrializing

England and the anthropologists who have studied the far-from-regimented schooling typical of rural areas in many developing countries.

Not only do the worldwide sweep and comparative focus tell us about the possibilities of schooling in human societies; they can also bring our own situation into clearer relief. It is hard, for example, to know that the social status and pay of school teachers can vary dramatically with significant effects on professional style and expectations if one looks only at the contemporary United States, in which teachers are paid relatively poorly compared to other educated workers. Such a narrow perspective misses out altogether on the value the Germans and Japanese have placed on the teaching profession as evidenced by their teachers' salaries and status in society. We might also be in a better position to judge how well our two-year community college system works in producing technically qualified labor if we compare it to the German dual system of apprenticeship training or the Japanese system of institutionalized network ties between secondary school teachers and employers. The broader perspective I commended in the essay can be completely compatible with present-day concerns and policy-related research. However, if this broader perspective prevailed, contemporary-concerns and policy-related research would be appreciated as valuable applied arms of the field rather than its core.

Given this background and these identifications, it will come as no surprise that I found much to appreciate in Rob Warren's response to my essay, or that I emerged a little dejected from reading it. Warren's analysis brings out

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major contextual influences shaping the contours of the sociology of education. Warren emphasizes the way our field has been influenced by the topics that government agencies are interested in funding, the interests of university administrators and others in the capacity of sociology to solve social problems, the availability of large data sets and fast computers, and the perceptions of junior professors about the most effective ways to obtain tenure in an era of rising publication expectations. Since the dominant orientation of the sociology of education that I described in "The Collective Mind at Work" (2009) goes back to the 1950s, I have to question Warren's emphasis on "big data" and his failure to consider social networks as influences on the tenure process. Nonetheless, his contextual analysis seems to me to have hit pretty close to the bull's eye.

I was discouraged by Warren's analysis because resource dependency so completely trumps intellectual autonomy in his reflection on our circumstances. In the classical model of the research university (originating in Germany and followed in the late nineteenth century by the leading private universities in the United States), the university was intended to be a place where scholars could pursue basic knowledge relatively free from economic and political pressures, and it was intended to be a place in which intellectual achievements, as judged by the scholarly and scientific community, won the highest praise and had the widest influence. Of course, today we can easily appreciate that the forces Warren describes do circumscribe the autonomy of intellectual workers, but that does not change the principle that the community of scholars and scientists has, or should have, the dominant voice over the adjudication of what constitutes important work. In this model, if the community says something is important, university administrators and government bureaucrats may not like it, but they would at least have to acknowledge that they lack the specialized knowledge to contest the judgment. It follows that handing over authority that should belong to the scholarly community to those who lack the training to judge the significance of the work it produces impresses me as a form of defeatism, however persuasively justified it is as realism. To the extent sociologists of education accept Warren's realism, they may be toasted by political and university authorities, but they will lack the capacity to do what I take to be their main job: illuminating the worlds of schooling in relation to other social

institutions and in relation to the experiences of the different social strata.

There is another way to look at this, however—one that suggests reasons for an ongoing tension between two visions of the role of sociology and, by extension, the role of the sociology of education. Sociology also has social reform origins in the work of Charles Booth (1889) and others who examined the slums of big cities in the age of industrialization. Moreover, the German research university model, with its emphasis on the pursuit of pure knowledge, has never been the only important university model in the United States. The social reform tradition fits well with a particular conception of the research university, the land-grant university model, found in the United States (but not elsewhere). The land-grant university model placed service to state and society much higher on the rung of values, competing with, and in some disciplines superseding, the pursuit of pure knowledge. The sociology of education has been greatly influenced by sociologists who worked in land-grant institutions, and it is therefore not very surprising that the study of a predominantly public institution like schooling should be influenced by the land-grant university's commitment of service to state and society.

Where Warren seems resigned to our field's dependency, David Bills, Stephanie DeLuca, and Stephen Morgan (hereafter BD&M) are confident about our continued good prospects. They argue that the field's emphasis on inequality and school achievement has provided a cohesive focus, "a rich and strong body of work" and "genuinely cumulative" knowledge. I agree; these are the virtues of "normal science," and we see them in the sociology of education. But the same could be said of a painter who paints only in blue. If I painted only in blue, I would have a cohesive focus, I might produce a rich and strong body of work about blue, and I could gain cumulative knowledge of the tonal complementarities and oppositions in the spectrum of shades of blue. The question is whether I would want to know everything possible about the color blue and only about the color blue, or whether I would want to explore a broader range of colors. In *Schools and Societies* (Brint 2006), the spiritual predecessor to "The Collective Mind at Work," I laid out reasons for employing a broader palette. I am happy to grant that studies of inequality and school achievement in U.S. schools are among the most important topics an American sociologist of education might

wish to pursue. However, as our field continues to grow and becomes more international in orientation, a process of structural accretion could occur; we could add important new topics while maintaining the study of inequality and achievement in American schools at its current robust state of health.

In the end I see no clear path to resolving the tension between these two visions. The broader vision I commend requires much more historical and comparative knowledge than many are interested in obtaining, and—I agree with Warren—it will have little impact on the capacity of researchers to obtain either grants or tenure. Its main advantage is that its range reflects the highest aspirations of our discipline. It will need to continue to produce important results in order to gain broader acceptance among the majority of those who work in our field. And of course, even that may not be enough to change the orientation of the field, given the power of invested interests. At the same time, I am cheered by the openness expressed by each of the respondents in this symposium to the possibilities of a cosmopolitan and topically inclusive sociology of education.

I would like to address a *lietmotif* in these responses: the relationship between our methods and our value. A common theme seems to be that if our methods are good, our results will have value. Park extols comparative and historical methods as a necessary approach to the theory development I seem to call for. Binder champions ethnographies as producing the new insights that I seem to be longing for. And BD&M provide a critique of quantitative methods intended both to prune and to improve on the repetitive themes I seem to lament.

There are many ways to contribute to the development of a field; methodological improvements and methodological variety are two ways. In this vein, BD&M make important points about quantitative methods. They write, for example, “The clear majority of regression models published in sociology (not just in the sociology of education) are overstuffed with adjustment variables and cannot sustain causal conclusions based on any of their coefficients.” This entire paragraph should be read carefully, perhaps several times, by everyone who uses quantitative methods. So should the rest of their discussion about the maladies they have found in the work of authors of quantitative submissions. One indicator of the health of our field is its willingness to engage in just this sort of self-critique.

Even so, I think the faith these responses show in the curative power of better methods is at least partly misplaced. Good methods are essential to the production of high-quality work, but they are no substitute for good ideas. We can see this by holding the work of our colleagues and ourselves to “the light bulb test.” We can ask of our work, “Does this work make the light bulb in the brain of an intelligent reader turn on?”

Let me illustrate what I mean.

Do people remember Christopher Jencks et al.’s (1979) *Inequality* because of the path coefficients in the models of status attainment his team presented? Or do we remember it because those models, so carefully developed, showed us that adding resources to schooling alone could not solve the problem of inequality and that if we wanted more equal societies we would need to embrace a more equal distribution of income? The coefficients become meaningful because they carry the force of a larger story about the sources of inequality in U.S. educational and income attainment. Similarly, I doubt that many will recall the regression coefficients in Paul Attewell and David Lavin’s (2007) chapter on the parenting benefits of a college education in *Passing the Torch*, but many will remember that the disadvantaged women who finished college read to their children, took them to cultural events, and learned about their children’s friends far more often than the otherwise similar women who did not finish their degrees. These are memorable findings, and the care with which the quantitative analysis is conducted helps us feel that the illuminating story they tell has a solid foundation. Richard Arum and Josipa Roksa’s (2011) book *Academically Adrift* created a stir not because the authors used quantitative methods—there’s debate about how well they used them—but because they told us something that we might have feared but did not know: Requirements at many colleges are dismally low, and a large proportion of students are not showing any improvement in analytical and critical thinking skills between their freshman and sophomore years.

Similarly, ethnographies may be hard to publish in journals like *Sociology of Education* not because they fail to fit into article-length constraints, but because many do not provide illumination that shines out beyond the particular social spaces in which they are set. Do we remember Erving Goffman because of his field notes on con men, inmates, beachcombers, and Scottish

islanders? Or do we remember him because of the pithy, illuminating concepts that he coined—impression management, frontstage and backstage behavior, audience segregation, role distance, presentation and avoidance rituals, deference and demeanor, total institutions, strategic communications, frame breaks—and the rules of behavior he identified for people engaged in different forms of interaction? Goffman's chapters on these topics are not much beyond article length. Similarly, in our own time, will we remember the details of the lives of the parents and children with whom Annette Lareau (2003) talked in *Unequal Childhoods* or her idea of concerted cultivation with its memorable allusion to successful gardening? Ethnographers may require books to fully explore their terrain, but the essence of their contributions can be distilled, and properly illustrated, at article length, as Lareau (2002) herself has proven. Of course, the sources of illumination go deeper than mere phrases. Paul Willis's (1977) ethnography *Learning to Labour* would not have been remembered only for his concept of the counter-school culture, although it is an important concept. The boisterous and craven figures who comprised "the lads" and "the ear'oles" in Hammertown School were also important to the force of the book, and so too was the irony that a counterschool culture of working-class defiance should culminate in adult lives of quiet submission. If I can express the force of his book, however imperfectly, in two sentences, Willis could have expressed it at article length had he chosen to do so. We should ask about the books lauded by Amy Binder how many of them have produced pithy, illuminating concepts or compelling, unpredictable narratives with memorable themes that pass the light bulb test. Only those that do are likely to shape our sociological imaginations in the future.

This line of thinking explains also why I am not as confident as Hyunjoon Park that a turn toward comparative and historical work is the pivot that will necessarily produce good theory. The elusive capacity to create important concepts, to develop revealing indices, or to distill essential principles does not form as a function of adopting a comparative approach. The fertile mind is the conjurer of the new reality we grasp through comparison just as it is the conjurer of the new reality we grasp by thinking about the materials we have collected in a single society. I know that Robert K.

Merton read the work of anthropologists who worked in a wide range of societies, but his theorizing emerged mainly from the confrontation of a creative mind with the materials of his own society. Though he may have stuck close to his study on Riverside Drive, Merton (often in collaboration with others) produced an array of concepts that continue to produce light in the brain: manifest and latent functions, unintended consequences, anticipatory socialization, opinion leaders, relative deprivation, role sets, and the rest. Closer to our time, Pierre Bourdieu engaged in a deep and prolonged engagement with just two societies: the kabayle and *les francais*, but he produced another repository of pithy, illuminating concepts: social and cultural reproduction, symbolic violence, forms of capital, habitus, methodological reflexivity, and many others. The creation of a vocabulary does not require a comparative approach. But Park is right that some forms of theorizing do require testing across societies. These are the forms in which a causal principle or a generalized measure, rather than a conceptual vocabulary, is proposed. For example, if we are interested in what sort of destiny demography may be in higher education, we can turn to the work of Martin Trow (1974). But we cannot know whether the outcomes Trow associates with the elite, mass, and universal stages of higher education are true unless we examine the social relations of education in many societies at each of his hypothesized developmental stages.

I am not calling for sound bite sociology. The scholarly and methodological circuits of any work, whether article or book, must be well wired to keep the light we produce from flickering out. Strong methods are guarantors that ideas do not just sound good but can be proven. Unfortunately, no methodological elixir can produce good ideas. It boils down in most cases to sweat plus insight. However, the sociology of education community can help to advance the cause of good ideas. By asking ourselves if our own work and that of our colleagues passes the light bulb test, we can set our sights deliberately in a direction that will more often produce the illumination we seek.

It required rare editorial discernment to see the possibilities for fruitful dialogue in "The Collective Mind at Work." The essay seemed, after all, destined for obscurity. It was given originally as a lecture in Lisbon, attended by no more than

150 Portuguese social scientists. It was subsequently picked up but buried in a limited-circulation newsletter. It has been cited rarely, and it is now four years old. I want to thank David Bills and his deputy editors for having the imagination to see the possibilities the essay presented. I also want to thank the six colleagues, including David, who took time to think about the essay and to write these thoughtful responses to it.

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