

David F. Labaree

A Sermon on Educational Research

Why would anyone want to deliver a sermon on educational research? After all, sermons are usually reserved for subjects of the highest importance, such as morality and faith. But for most people, educational research as a form of professional practice doesn't rise to that level. It's a marginal activity – ignored by the public, resented by educators, and used by policy-makers only when it aligns with their political agendas. But I am not giving this sermon to most people; I am directing it exclusively at that small group of stalwarts who are setting out on careers as educational researchers. For those of you in this group, the field is not some marginal enterprise; it's going to be your profession.

Like most sermonizers, my aim is to give you advice about how to live your life (in this case, as educational researchers) and to offer reflections on the larger meaning of this life. Neither the advice nor the reflections are likely to be things that you will be hearing from your advisor, dean, or journal editor. Both parts of the message go against the dominant norms in the field. So why should you listen? Certainly not because I have any special authority to make these claims. I don't. I speak not as a role model for how to do educational research but as a survivor of 30 years in the field, who has learned the hard way, through trial and especially through error, that I was sadly misinformed about the nature of the enterprise. So this is not a success story but a cautionary tale. These are things I wish someone had told me early in my career, but having this knowledge probably wouldn't have made things any easier. For one thing, I probably wouldn't have believed it, and (I assume) neither will you. For another, knowing what is going on in your domain of professional practice doesn't reduce its difficulties; such knowledge only helps you rationalize why you strayed from the path that the traditional principles of the profession were pressing you to follow.

Every sermon needs a text, and here is mine. Emerging scholars in the field of education should keep these counter-principles of professional practice in mind: Be wrong; be lazy; be irrelevant; and think of your work as an effort to balance the values of truth, justice, and beauty. Let me explain.

Be Wrong

Trying to be right can get scholars in trouble. Too often it leads them to work so hard to avoid being wrong that they end up being boring. The truth is that if you are not speaking at the very edge of your data then you are probably not saying anything interesting. This is the problem with most dissertations, which concentrate analytical attention on the elements of the story that have rock-solid grounding in the evidence and therefore push all the interesting issues to the margins.

The thing to keep in mind here is that for scholars the function of writing is to work out complex intellectual problems that you can't resolve in your head. When you start writing a paper, you have some ideas about where you think the analysis will go and an outline that props up these ideas. But you won't know if this plan is viable until you work through the

argument step-by-step in writing. Only this process allows you to reflect on each element of the argument, find out how these elements fit together, and discover where they all lead. Often as not, you find yourself straying from the outline, as your original plans don't work out and as the details of the analysis on paper lead you gradually in a different direction. This feels like you failed somehow, because your original plan was wrong; but that is just the way real writing works. You are not writing out an idea that is already fully formed; that's not writing, it's transcribing. Instead, real scholars develop their ideas as they write. It is easy to have a great idea in your head about the nature of schools or the history of education, but having such an idea and even telling it to someone over lunch is not doing scholarship. It's like singing in the shower. It may sound good, but the real test comes when you go into the studio, record your voice, and then listen to it on replay. That's what writing does for scholars. It's our reality check. Talking a good game doesn't matter; working out an idea on paper is the only thing that counts for us.

What this means is that scholars learn from their own writing. It's only when they get to the end that they finally figure out what their point is, which then means they need to redraft the entire paper in order to make that point clear. Keep in mind that if you don't learn from writing the paper then no one is going to learn from reading it. The implications of this, however, are rather scary. You never know when you start a paper (or a dissertation, or a book) whether it is really going to work. Writing is a high-wire act with no safety net, where the possibilities for disaster loom at every step in the process, arising from every effort to define your point, support it, and connect it to the next point. It is risky business; and if you try to avoid the risk and move toward hard ground, you give up on the possibility of doing something interesting. You regress to the mean of the field and find yourself repeating what we already know. Why would you want to be safely boring, whether for a whole career or for a single paper? Instead you should reconcile yourself to being wrong over and over, learning from your mistakes, and moving ahead in an effort to develop ideas that are worth considering.

One last benefit that derives from embracing the risk of wrongness is that it frees you from one of the worst pathologies of the earnest scholar: the unwillingness to declare a project finished. Howard Becker (2007) calls this the problem of never getting the paper out the door. If you are obsessed with being right, you are never finished: there is always another study to do, another book to read, another theory to explore. But if your aim is not to be right but to be interesting, then it's ok to let go when the paper is sufficiently engaging to be a contribution to the literature. Remember, your job is not to nail down the subject for all time. There is a name for people who pursue this goal: unpublished scholar. Instead, your aim is to inject into the scholarly conversation an idea, an example, a body of evidence, a perspective that is not already out there and that provides an interesting new way to see an old problem. You want your contribution to be sufficiently logical, grounded in literature, and/or validated by data to be credible, so readers can't easily dismiss it. But it doesn't need to be beyond reproach. Let others develop alternative arguments and introduce alternative data. Let them point out where you are wrong. That's ok. Being wrong in this way advances the conversation in a field and provides fresh ways of understanding the field's issues. As scholars, that, after all, is what we're trying to do.

Be Lazy

If trying to be right is a problem for scholars, so is trying to be diligent. In education and other research domains, we put too much value on hard work. If you plan to study a subject, we are told, you need to lay out a detailed plan that requires you to read every article and book that is remotely related to the subject and then gather and analyze a mountain of data on it. If you're doing quantitative work, you need to code all available information, use this to develop a massive database, and then explore every nook and cranny of this database using the full range of available statistical tools. If you're doing qualitative work, you need to sift through every dusty box of records in the archives, interview every possible subject several times over, transcribe every word of these interviews, and develop a complex system for coding and commenting on every component of the data.

Sound familiar? My advice is: Don't do it. The point of carrying out research is not to master every piece of trivial data on a subject and treasure every word of the related scholarly literature. Instead, the point is to find something in the data and in the literature that might be enlightening – to an academic or practitioner or policymaker or citizen. And in pursuing this goal, it is helpful to have a broad streak of laziness. The best strategy is not to plow down the middle of all the data but to look for a shortcut to the good stuff. Look for the workaround rather than the plodding path; keep focused on the interesting material and don't get lost in the minutiae; focus on telling an interesting story, drawing on the evidence for support rather than relying on the data to be the story.

The sad fact is that data don't tell us what they mean, so we have to dig it out of them. Put another way, data are noise, and our job as scholars is to find the music. You can't do this by wallowing in your data for an extended period of time. That is a waste of effort. Instead, you need to use your knowledge and skill – reinforced by that streak of laziness – to figure out how to move through the cacophony of data listening for a melody. Use strategy to do this rather than bull effort. It makes no more sense for a researcher to plow into the middle of his data, hoping for the best, than it does for a general to order a frontal assault on the enemy's strongest position. Better to probe for a weak spot, find a back door, feint left and go right. There is no honor in losing half your army when you can win a battle by being smart. You don't want to be a plodding scholar but a smart one, using your resources sparingly and with telling effect, thinking your way around a research problem rather than plunging into the fray with guns blazing.

If educational researchers show too much dedication to diligence, they also show too much dedication to complexity. Overvaluing complexity has a lot in common with overvaluing validity. If you're obsessively worried about being right, you feel compelled to keep piling up data and references to support your argument, and as a result the story you are telling becomes increasingly complex to the stage when it reaches a state of unintelligibility. At this point you are left unable to tell a clear and coherent story about your research. Instead, you find yourself saying that "the story is really complicated." There is too much going on in your data, which means it is hard for you to say anything about your research without so qualifying it with exceptions and so undercutting it with alternative interpretations that it is impossible for the reader to come away from it with any new insights at all. It becomes a series of statements on the order of "On the one hand there's this, but on the other hand there's that." I'm arguing that you should vow to be a one-handed scholar. Let someone else give the other side of the story.

At heart, research is a stimulus for thought. Your job as a scholar is to tell your own story in your own way in order to insert a stimulating idea into the scholarly conversation. This means you need to look for ways to simplify the story you are telling. This story – which is another word for a theory or an interpretation – is an analytical slice through a complex array of issues and data. To make such a slice you don't want a comprehensive analytical approach that is shaped like a beach ball. Instead you want analytical tools that are as thin and sharp as a razor. This will allow you to show something new and interesting in the domain you are studying. It will allow you tell a story that is focused and lean, which leaves out almost everything in your data except the few things that are really worth considering.

Be Irrelevant

A third problem with the canonical way of doing educational research is that we're supposed to be striving for relevance. This is an issue in most domains of scholarship, but it is particularly strong in a professional field like education. We have been assigned responsibility for an institutional arena that is enormous in size and scope, ruinously expensive to fund, and highly consequential for both the individuals who inhabit it and the society that depends on it. Under these conditions, we educational scholars find ourselves under a great deal of pressure to make ourselves useful. We don't want to be part of the problem but part of the solution. We want to make schools better, improve the lives of students and teachers, and promote a more equitable and efficient society through education.

My advice is to resist this pressure and instead pursue a course of scholarly irrelevance. Why? Because the pursuit of relevance leads, ironically, to irrelevance. Let me borrow from the analysis of the subject by Mie Augier and James March (2007) to explain why.

As they show, one problem with the pursuit of relevance is that it promote myopia. It encourages you to examine a problem that arises from the arena of education with the aim of providing an analysis that might be helpful in fixing this problem. This puts the emphasis on understanding the problem in a particular time and place rather than considering it in a broader context that bridges across time and space. In brief, it promotes short-sightedness. It asks us to pull the educational problem in close in order to understand it, but in the process we are losing sight of the broader social context within which the problem is embedded and the broader historical context within which it developed. It encourages you to look at the case as a unique problem requiring a tailored fix, which keeps you from stepping back to see what it is a case of. Even if you do come up with a good analysis and a workable fix, it is likely to be irrelevant by the time the study comes out. In the interim the situation in the particular site of study has changed, so the analysis no longer applies; and it is unlikely to be applicable to other settings, since you developed it in a very context-specific manner. Conversely, the most apparently irrelevant theoretical exercise may suddenly become enormously useful because it turns out to apply across time and space. So in the long run it is more useful to focus on developing general understandings of education than to focus on developing solutions to current problems.

A second difficulty with the pursuit of relevance is that relevance is inherently ambiguous. We want our research to be useful, but useful for whom? It depends on which educational actor you are talking about – teacher, student, administrator, parent, policymaker, employer. What is useful for one person may be irrelevant or even harmful to another. For example, re-

search developing value-added tests to measure teacher effectiveness may be quite useful for policy makers and administrators, but may be seen as useless or anti-educational by teachers, students, and parents. In addition, the usefulness of research depends on which goal for schooling you are using as the criterion. Radically different goals for school lead to radically different definitions of relevance and usefulness. Depending on our position and perspective, we may want education to create productive workers, or raise test scores, or promote individual opportunity, or preserve individual advantage, or maintain good citizenship, or spur intellectual growth, or any number of other socially and individually salient ends. What is useful for one of these purposes may be useless or dysfunctional for other purposes. And since the range of purposes for education is so wide, you are likely to produce more of the latter than of the former.

Scholarship as an Effort to Balance the Values of Truth, Justice, and Beauty¹

So far this sermon has focused on giving advice about what not to do in pursuing your chosen careers as educational researchers. But now I want to turn in a different direction in order to offer some reflections about what gives the work of doing educational research its meaning. Before you commit yourself to this career, you need to ask yourself, “Why do I want to do educational research?” And you want to keep asking yourself that question as you launch every new scholarly project. Why do I want to do this? What makes the work worthwhile? What good can come from having me write this paper and from having others read it? Is this a reasonable way for you to live your life? If so, what valued ends does this form of professional practice serve?

Of course, one explanation for doing research is that it is your job. Publishing research papers is how you win a job as a university professor, and continuing to publish is how you gain promotion, tenure, and performance pay. The number of papers we publish in high-ranked journals is the key measure of our productivity as scholars and thus of how good we are at our jobs. If you don’t publish, you perish; if you do publish, and do so in the right places, your career will win for you the extrinsic rewards of pay, position, and prestige. This is certainly a major motivation for all of us in the business of educational research. As a practical matter, it would be crazy for any emerging scholar to ignore the fact that scholarly life is much more pleasant if you can write your way into the upper levels of the academic hierarchy.

These extrinsic motives for doing scholarship are a necessary part of educational research as a form of work, but they are not sufficient to justify it as a way of life. Without a larger meaning and purpose, educational research constitutes just another form of alienated labor. In the classic Marxist understanding of alienation, workers sell their labor to an employer who then controls their time and owns what they produce during that time. But the form of alienation experienced by academic careerists is far worse. Assembly line workers are renting their hands, but their minds are free – free to write poetry, dream of a better life, or plot a union action against the boss. Alienated scholars are renting their minds. They are harness-

¹ I am borrowing the trio of truth, beauty, and justice – and much more in this discussion – from my colleague Jim March, who uses this trio as the default topic of conversation in his Monday Munch seminars at Stanford. His work shows that it is possible to balance these three values in scholarship and to perform each at the highest level.

ing their consciousness to the meaningless tasks of doing research and publishing papers – as defined not by the researchers themselves but by the norms of their field, the productivity standards of their institution, and the demands of their editors – all simply in order to earn a pay check. If you are thinking this way about a career in educational research, I urge you to do yourself a favor and walk away now while your mind is still your own. Better to drive a cab with a free mind than to sit at a computer cranking out papers like so many widgets.

If you are going to pursue a scholarly career, therefore, you need to figure out what makes such a pursuit intrinsically worthy. You need to decide what satisfactions you can derive from it and what meaningful ends it serves for you and for others. When I ask doctoral students in education why they want to become educational researchers, they usually come up with answers that fall into two broad categories. One goal they see for their work is truth; a second is justice; to these I add a third, beauty. I want to argue that a scholarly life is most rewarding and most meaningful when you seek to balance all three of these values in the course of your work. Concentrating on only one or two can bring negative consequences. Instead, I call on you to think broadly about your scholarly work, allowing yourself the full range of possible satisfactions and sources of meaning that are inherent in this work and holding yourself to high standards in all three of these domains.

Truth

First, there is truth. For the most part educational researchers tend to define themselves as social scientists. A key part of their mission, therefore, is to develop a rigorously scientific understanding of education and its role in society. This means they learn research methodologies that will allow them to develop valid claims about schooling. They design and carry out studies that will sort truth from fiction, showing how things really work rather than how we might hope they work. They expose half-truths, blow up misconceptions, and counter false claims. Their dedication is to pursue the truth whatever the consequences for preexisting beliefs and vested interests.

The pursuit of truth in educational research is not easy, however. A number of perils threaten to derail this mission. One comes from politics, another from ideology. The political threat is that it is always more convenient to come up with research results that are in line with major policy objectives. Policy necessarily drives a lot of educational research, since the governments and foundations that fund most research do so in order to inform policy decisions and solve educational problems. Policymakers want clarity about what to do. But the most rigorous research tends to complicate the policy picture, by showing how the wide array of variables and contingencies that shape the process of schooling make it difficult to come to clear-cut answers about the relationship between policy initiatives and educational outcomes. In order to get funding, it is tempting for researchers to make promises about the relevance and clarity of their research that they cannot realistically keep. And in order to maintain a stream of funding, it is tempting to frame research results in a way that fits policy demands better than it fits the data. A common way we do this is by reifying the measures used in our studies (test scores, graduation rates, lifetime earnings), claiming that these limited and unreliable measures effectively represent the complex outcomes of education. In short, educational researchers have a strong incentive to lie, or at least to shade the truth.

Another threat to truth telling comes from ideology. Researchers have their own sense of what is right and wrong in the educational system and their own ideals about what role school should play in society. These personal commitments can make it easy for us to come up with research findings that match our ideals. Too often we find what we are looking for instead of finding what is really going on. We care about social justice and find that schooling reinforces injustices of race, class, and gender. We care about teachers and find that policies undercut teacher professionalism. We care about progressive pedagogy and find that these ways of teaching are the most effective in the classroom. In the name of high ideals we undermine the validity of our own research, since we are reluctant to follow the line of argument in our own studies to conclusions that will make us uncomfortable. This is a natural tendency but it is one you need to resist vigorously, since the end result is abandon the role of truth teller for that of spin doctor.

Justice

To approach educational research as the pursuit of truth helps give meaning to this work, but unless linked to the pursuit of justice this work lacks heart. After all, education is the most normative of human endeavors, by which we seek to instill the young with the capacities and dispositions we value. We use schools as a way to solve social problems, realize social goals, and build a just society. For educational researchers, seeking truth is not sufficient since it focuses on the technical issues of schooling – how it works, what its consequences are, which approaches are more effective – without ever dealing with the normative question of what education should try to accomplish. Yes, we need to understand the educational machinery and free ourselves from misconceptions and false hopes; but we also need to address the broader questions of purpose and meaning that we invest in the educational enterprise. As Max Weber (1918/1958) explained in *Science as a Vocation*, scientific research can improve our understanding of the world around us but it can't tell us how to live a good life. So researchers need to supplement their analytical skills with their philosophical commitments. They need to keep asking themselves: What kind of society do we need and how can schooling help us realize such a society? What role can my research play in making a better system of school and society, or at least not making one that is worse?

To fail to ask such questions means to segregate your role as a social scientist from your role as a human being, and no good can come from that. The difficulty, however, is in trying to establish the right balance between the two. If you give primacy to truth over justice, you run the danger of unwittingly reinforcing a structure of schooling that is unfair. For example, you may find yourself working with great skill and diligence to produce more effective pedagogies, curricula, teacher training programs, testing systems, and organizational structures that help produce outcomes you find abhorrent. Efficient means are a good thing only if they lead to ends that are morally desirable. So researchers need to be on guard that they are not implicated in making schooling more harmful for teachers, students, and society at large. And they should pick subjects of study that seem likely to help make schools better in the normative as well as the technical sense.

On the other hand, if you give primacy to justice over truth, you can find yourself so committed to a social mission that you forget about your responsibility to follow the analysis wherever it leads. I talked about this in the previous section, showing how our political and

ideological commitments can blind us to disconfirming evidence and predispose us to find what we are hoping for. Educational research, like any other human endeavor, is prone to confirmation bias. So we need to fortify the validity of our findings with rigorous methods and solid evidence as a safeguard against our natural tendency to adopt the conclusion that best matches our preferences.

This is a particularly important concern for educational researchers because of our deep-seated desire to make schools better. If our commitment to truth leaves us prone to the sin of diligence, then our commitment to justice leaves us vulnerable to the sin of earnestness. The discourse that infuses meetings of educational researchers often has less in common with science than religion. We seem to spend more time attesting to our faith than testing that faith against the evidence. We chant mantras without examining their validity: All children can learn; school is the answer; constructivism is the answer; teachers are the answer.

In light of these concerns, I urge you, as emerging scholars in the field, to harbor a healthy skepticism about the articles of faith in the educator creed. Push back against the Pollyanna tendency keep finding light at the end of the tunnel. Sometimes there is no end in sight and someone needs to say so. It is important work for scholars to uncover problematic processes in education and explain where they came from and how they work. In pursuing this work, you would do well to nurse a fondness for irony, since education is a field that is filled with it. Efforts to expand access to education for some students frequently lead to increased advantage for others. Reforms that deploy curriculum standards and testing targets to improve teaching and learning frequently end up undermining the quality of both. A century-long effort to make schooling more progressive has had a huge impact on teacher talk and very little on teacher practice. In short, as James March (1975) pointed out in *Education and the Pursuit of Optimism*, educators (and educational researchers) would do well to combine their optimism about the role of education in promoting a better world with a recognition that there is little hope this end is going to be realized any time soon.

Beauty

And then there is beauty. What, you may well ask, does that have to do with educational research? Truth and justice have a certain face validity as goals for scholarship in our field, but in comparison beauty seems would seem to be a side issue – nice if you can work it in, but ultimately unnecessary and even possibly a distraction from the main missions for the profession. In addition, when you look around at the published work in the field, you can find evidence of scholars pursuing the first two missions but little for the third. Much of this work is tone-deaf, lifeless, and esthetically repellent. Reading your way through academic literature in education is only rarely a pleasure to the esthetic senses. Often this task is only tolerable if you first numb those senses with a little scotch.

So let's go back to where I began, asking yourself why you might want to get up in the morning and spend the day doing educational research. Why good can come from this kind of pursuit? What makes it worthy and meaningful? Digging out the truth about schools can help make the world a little less dishonest; supporting a more just social role for schools can help make the world a little less unfair; and promoting beauty in schooling can help make the world a little less ugly. Research can and should be simultaneously an effort to understand, improve, and beautify. Each of these pursuits brings its own meanings and plea-

tures to the practitioners. For the third, the satisfactions come from your ability to use your research to make schooling more beautiful for others and also your willingness to make the process of doing research an exercise in esthetic expression for yourself.

In many ways, the effort to make schools more beautiful is an extension of the justice goal. For example, why should schools for the disadvantaged be places that focus on drilling students in basic skills for the purpose of improving test scores while schools for the advantaged allow a much richer experience of broad cultural learning and personal expression? But the school effects extend beyond the issue of fairness. For researchers it means making sure that your work, in the name of promoting the effectiveness or social mission of education, is not inadvertently contributing to the process of squeezing most of the esthetic pleasures from the school experience. Teachers may get higher evaluations and students may get more right answers following our research-based guidelines for educational practice, but both may find that the process of schooling has thus become an academic exercise in sensory deprivation and personal repression. Too often researchers help turn student learning into a meaningless job and help turn the performance art of teaching into a utilitarian trade.

In addition to using our research to make the lives of teachers and students more lovely, we also need to do the same for our own lives as researchers. To return to the earlier theme of researcher alienation, we not only want to avoid subordinating our minds to someone else's project but also to avoid subordinating our esthetic gratifications to the mission of serving truth and justice. These other missions are important, but so is the mission to use our scholarship as a way to sculpt our own works of art. Academic writing can be and should be a medium for personal expression and artistic creation. In our work we should be exploring the elegance of schooling as a cultural ideal and as a social construct, and we should be telling rich stories about school and society as a window on the human condition.

In the last analysis, the esthetic component of being an educational researcher comes down to writing. To engage this part of the role we need to approach writing not just as a way to tell truth and promote justice but also as a way to construct art. This need not be high art, though having such aspirations is a good thing. There is no need to freeze yourself into wordlessness for fear of not being able to write with poetic flair. Instead, I am talking about remaining conscious of the esthetic form of your writing as well as its academic content. Be observant of style as you read both inside the academic literature and more broadly in other genres. Notice how some authors write sentences that sound good, draw the reader into the account with clarity and grace, nail a point and move on. Look for the rhythms that are pleasing and those that are not; find the music in the way words come together and in the way they surge to a crescendo and then subside gracefully into silence. Try applying these insights to your own writing, training yourself to edit your work for sound and rhythm as well as for clarity and validity. Try reading a passage out loud; if it doesn't sound right, then you need to change something. Keep in mind that being an educational researcher is being a writer, and in this role you are not just writing journal articles but you are contributing to literature in the fullest sense of the word. You want to ensure that this contribution leaves the world a little bit lovelier.

As emerging scholars in the field of educational research, I urge you to follow these lessons. Be wrong, be lazy, be irrelevant; and work to balance your work in the pursuit of truth, beauty, and justice. Amen.

References

- Augier, Mie/March, James G.: The pursuit of relevance in management education. In: *California Management Review* 49(2007), No. 3, 129-146
- Becker, Howard S.: *Writing for social scientists: How to start and finish your thesis, book, or article* (1986). Chicago: University of Chicago Press 2007
- March, James G.: Education and the pursuit of optimism. In: *Texas Tech Journal of Education* 2(1975), No. 1, 5-17
- Weber, Max: *Science as a vocation* (1919). In: Hans H. Gerth/C. Wright Mills (Eds.): *From Max Weber*. New York: Oxford University Press 1958, 129-156

Prof. Dr. David F. Labaree, Stanford University, School of Education, 485 Lasuen Mall,
Stanford, CA 94305-3096, dlabaree@stanford.edu
