Debatte – Discussion

A Sermon on Educational Research

Eine Predigt über erziehungswissenschaftliche Forschung

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(Red.) Die große Sorge erziehungswissenschaftlicher Forschung gilt heute den Methoden und ihren Resultaten, den Daten. In den Hintergrund geraten sind dabei die Qualität der Fragestellungen (vgl. IJHE 1/2011) und – wie David F. Labaree hier thesenartig formuliert – gewisse Grundeinstellungen der Forschenden zur Forschung selbst. Wie „Unrichtigkeit", „Faulheit" oder „Irrelevanz" zu Forschungstugenden geadelt werden können und was ausgewählte Kolleginnen und Kollegen dazu sagen, ist auf den folgenden Seiten nachzulesen.

(Ed.) Today, the predominant concern in education research is about methods and their results, the data. In contrast, the discussion about the quality of research questions has been pushed into the background (see IJHE 1/2011) as well as – as David F. Labaree suggests in the form of theses – certain basic attitudes of the researchers towards their research. How being "wrong", "lazy" or "irrelevant" may become positive attributes of our research and what a selection of scholars think about this can be read in the following pages.

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 of 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 Beauty[1]

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-

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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 is it 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 he 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-

sures 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.

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Robert Cowen
Twinkle, twinkle, little stars – or just get them organised?

A sermon is a wonderfully old-fashioned concept. My father would offer me one every month or so, at home, during my adolescence and it is still the case that, in England at least, sermons are offered in churches on Sundays. Occasionally the Archbishop of Canterbury will offer a sermon that attracts attention, by speaking about Islam or the role of women in the Church or one of our wars. However, the main role of sermonising has passed from parents and priests to politicians. They sermonise a great deal – often incoherently but with total conviction – about globalised economies, the need for skills and a flexible workforce, the urgent necessity for cuts in public finance and the simultaneous need for greater economic growth, and about the fact that bankers are capitalists.

It is thus a considerable shock to be offered a sermon by someone who is not a parent addressing a child, a priest addressing a flock, or a politician addressing a populace. It is even more of a shock that a sermon by an academic makes sense at all, even if the academic has a strong interest in history, an alertness to sociology, and a sense of humour. David Labaree has said something well worth saying and in parallel it is well worth wondering why his text feels so apposite outside of the context in which it was prepared (which was basically a graduation ceremony in the USA): "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". That is a fine starting point even if the goal is high-minded and difficult of accomplishment; but why do we need the reminder?

In slightly more formal language, what are the features of our contemporary politics of knowledge (in educational studies), which make Labaree's theme so wise, and his implicit questions so pointed?

An important change in many places – Belgium, Canada, Denmark, England (and other places, were footnotes permitted) – is the routinisation of research training. The Master's degree and the doctorate are increasingly done to a fairly strict time frame, and the contents of such degrees in educational studies are increasingly centred on the theme of research competence. This in practice often turns out to be an immersion in routine research method rather than a serious introduction to the philosophy of science or a sense of the dialogue, divergence, and dialectic between the social and the natural sciences; a ritual of 'finding a research question' rather than a release of the imagination by grasping the life-force and trajectory of an Aron or a Braudel, an Einstein or a Feynman, a Foucault or a Freire. Truncated in time, focussed on competencies, infused by quality equivalences, and increasingly marshalled, measured, and judged by national or continent-wide 'bench-marks', we get the research imagination our research agencies ask for: efficient, interchangeable, and unexciting – but ever so competent.

Entwined in this routinisation of the research imagination, and an integral part of it, is the de-personalisation of the relation between student and scholar. The tutorial hour becomes strictly and almost contractually that: an hour. The minimum, as defined in the Faculty rules or the accreditation agency's expectations, becomes the maximum: one hour at the end of which documents are exchanged or placed in the Department Office, which record the teaching-and-learning which occurred in that hour, and the preparations for the next hour of work to be undertaken at a later meeting. It is difficult to hold an academic conversation in an hour. Brilliant academic conversations can last three minutes or from lunch till dinner. Scholarship demands talking. Some of that talk may be loose and apparently pointless within a maze of speculation and guessing and mis-steps – all of which demands trust, a sense of joint commitment, mutual respect, and occasionally considerable patience. This of course is a perfectly proper way for scholars to develop – but it is rather a time-wasting way to train researchers in education.

It is that discursive shift in the vocabulary – 'scholars' and 'researchers' – which hints at a third layer in the problem. The concept of career and the concept of ownership of a career have, in our time of an expanded research industry in educational studies, shifted quite markedly. With increasing frequency from the nineteenth century onwards, the scholar of education worked in a university, and the scholar pursued a career with its own personal epistemic trajectory. The researcher contemporaneously works for a university or for a research project, and must deliver part of a project to time and to budget. Ideally researchers should be more or less interchangeable and (in Max Weber's term used in a different context) should be able to pursue a topic *sine ire et studio* – because the research project, unlike a life of scholarship, will have an end. The researcher often pursues an epistemic trajectory which is – contemporaneously – owned by others (literally, contractually) and it becomes increasingly important to ask which agencies, whether in Europe or in the USA, or inside Denmark or inside England, are setting 'the' research agenda and to ask which themes are not on it. The motif of pursuing 'robust and relevant research' – so much part of the sermonising of 'Ministers' of Education in England – almost always fails to indicate what might count as 'robust' (other than some statistics which can be quoted) and almost always fails to specify what counts as relevant (other than the latest hot policy topic in education).

In other words, in the routinisation of research training, in the shift of pedagogical relations within that training, and in the increasing bifurcation of the roles of university scholar and contracted researcher, we are seeing the penetration of the politics of efficiency into the

forms of preparation for and the conduct of educational research and the 'industrialisation' of research within educational studies.

Educational research is now a product, measureable and marketable, on the books of universities and in the market place of reputation. Educational research now has its own highly skilled labourers – 'professional' Research Officers with PhDs and high levels of certified research competence. Educational research is now a knowledge industry, which commands (and demands) major investment, as in PISA and TIMMS, and educational research is a knowledge industry in two senses: it is based on symbolic analysis and it produces knowledge outcomes. Logically – that is, ideologically in terms of the politics of efficiency – preferred outcomes are educational 'policy-product': forms of applicable knowledge intentionally aimed at 'solving' socially hot topics – for example, increasing the mathematics scores of school age children as measured by standardised international tests.

Labaree does not, *mutatis mutandis*, labour his way through these points about the politics of knowledge for the USA. He would not have needed to, anyway: his audience would have been alert to the current context of research, its politics and sociology, and its contemporary history. Nor should he have laboured through such points in a graduation address. Instead he writes brilliantly about the act of writing and offers a flurry of insights into the nature of our work. However, in addition to a scintillating surface display of professional *aperçus*, he does something else which is extremely elegant and important.

Strategically, he keeps alive the word 'scholar' in his text. It is this I think – certainly, I hope – which helps to sustain the motif which makes his script sing: the significance of the moral in education, the permanent search for the values of truth and justice and beauty – which is why we read the classics of educational thought, which is why we read the classics of political thought and the classics of economic and sociological thought and why we often, as scholars of education, became excited by the radical or the exotic, whether this was Comenius or the education of the Chinese *literati*: what were the humanising, creativity-releasing, possibilities of these educational forms and their relation to truth and justice and beauty? As we live through our moments of hubris – knowing ways to guarantee improvements in grade-point averages in local school districts, our over-confidence that educational policy is now almost a science, our current Adoration of The Manager – it is good to have Labaree's voice reminding us that we have historic responsibilities for the meanings we give to the word 'educator'; as well as the word 'educated'.

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Fabio Pruneri
"The Mass is ended, got in peace"

One of the few advantages of having to work on an article's translation is to pay close attention to every single word in order to reconstruct the complete meaning of the sentences. What me, an Italian scholar, strikes about Labaree's sermon is the 19 times use of the verb "pursue" and the noun "pursuit"; terms that have a common origin in the Latin verb *prosequi* (*pro-* + *sequor*). In its Latin sense *prosequi* has different meanings, i.e. to follow a track, a goal, a purpose, but also to continue to insist on a certain action. I appreciate, in the words of my colleague, his invitation to ascend the river of research in history of education to its source; a task that requires to go against the tide. So he invites, like a true preacher, young aspirants to research, and professionals to interrogate themselves about the ultimate questions.

As it is well known, in traditionally Catholic countries like Italy, in the sermon, the priest often warns the faithful, but really wants to hit the unfaithful. So the first category undergoes a series of criticism and observation for the latter. Similar reasoning could be applied to the sermon of "reverend" Labaree. Whoever reads this article in the journal *Bildungsgeschichte* is not an apprentice or a general lover of historical research, but an experienced scientist who takes advantage of the wisdom of a few colleagues and the authoritativeness of a specialized journal to reflect on his vocation: Labaree warns those already involved on the right track, but does not weaken the pride of those who consider themselves already arrived at.

Since I trust in the humility of journal's readers, and their willingness to accept others' opinions, I will just add some refinements and additions to the sermon.

The first is that research is the result of a great passion. Without that passion, which is a true vocation, there is not extrinsic motivation that makes interesting the long and frustrating apprenticeship before achieving important results and acknowledgments.

The second is that research, as it should play in a scientific community, requires a predisposition to introspection and personal reflection. I often speak to my students of the loneliness of the seeker. I have spent many hours of silence in my life, often alone, in front of archival documents, papers and inks faded by the time. Several times I have handled alone registers, lists of names, autograph materials absolutely free of any charm, illegible notes that seem dry sheets of paper for everyone except for me or for those who were able to give them a perspective. Yet, I have rarely experienced such intense emotions as those that you can get by discovering something long awaited and now finally available. Just like a childbirth, the search takes a long incubation period, some pain and much, much patience. Therefore, the meaning of the Latin *prosequi* as to continue, to insist is most fitting.

Paradoxically, while the possibilities of Internet, of social networks constantly push the generation of digital natives to speed, to distractions, to interruptions, and positively, with a frequent exchange of points of view, to a strong sharing of one's identity, historical research, from my point of view, requires a strict privacy and space of inner silence (Carr 2010).

Studying and writing about history of education requires a space and time that can't be easily shared with colleagues, friends or family. For this it becomes difficult to understand, even to those close to us, what we do, how we spend our time, the usefulness of our work. Yet

within a magic circle, consisting of a mix of factors, some objective (a culturally stimulating environment, an economic willingness, functional technical tools), others partial and highly subjective (a place where to focus and write, a serene professional environment, an empty mind), a good researcher develops a real craft, consisting of the collection of raw material and its subsequent processing, roughing and finish.

Then, just as the craftsmen, he brings the product (research, publication, monograph) to the market to be judged at fairs (congress) and squares (forums, websites, etc.). So the market (scientific community) will set the value of his job, in terms of credibility, longevity and resistance to the time.

I can then accept only part of the first of the paradoxes: "Be Wrong". I would translate it this way: "agree to take some risks". The result can in fact take the form of beginner's luck.

The Italian scholar Carlo Maria Cipolla, known in history of education for his major contribution to the investigation of the relationship between literacy and development, in an autobiographical passage he recalled how his early work on the exchange rates between the currencies in the ancient Italian states were born by chance when he met in an archive, a conversion table. In fact, what had seemed to him a sort of "Rosetta Stone" was pretty an ordinary document, but since he was a novice researcher, he considered it more carefully than anyone else and he devoted to this source all his energy of a young and enterprising researcher on economic history (Cipolla 1969). His results are truly noticeable.

Carlo Ginzburg opened in the book *The Cheese and the Worms. The Cosmos of a Sixteenth-century Miller* a new trend, that of micro-history, accepting the risk of making mistakes and not to reconstruct a summary of all cosmological theories of the 16th and the 17th centuries, but that of a single miller in a small village in Italy (Ginzburg 1980). In both cases the risks of failure were high, but the gamble paid off, as these authors have considerable "impact factor" for results, not shared by all, but essential to our research.

The fact remains that, along with virtuous examples, there is an undergrowth of alleged false scholars and historians who are so eager to see their name appear in some volume to be narcissistically convinced that they had discovered some secrets when in reality it would take a more careful literature search to discover that what appears to be unique and unrepeatable reality is already known.

My fellow professors will be also doubtful of the suggestion: "Be Lazy," if only because we have spent a lifetime asking our students to do the hard work and analyse mountains of data. I correct myself, the work of others has helped us to be lazy ourselves. If we did not have serial and quantitative research tools, made by some disciplined scholar, how could we develop interesting ideas?

It is true that thanks to the possibilities offered by Internet the problem is gradually shifting from the collection of materials to the selection, or even to the progressive deviation. In short, the most faithful representation of reality is the 1:1 scale, but it is equally true that any map can't be used if it is not reduced or if it does not neglect this or that detail, so as to show what the traveller wants to really find. So it's important not only an accumulation of data but also a rigorous selection of really useful information[2]. However, I get very angry when I find conclusions not supported by data. In many cases, I do not ask to see all the numbers, but at least I like to know, (in a footnote) where or when an author has found this or that

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2 The site *writinghistory.trincoll.edu* [20/9/2011] contains a born-digital, open-review volume edited by Jack Dougherty and Kristen Nawrotzki, with an eloquent title: *Writing History in the Digital Age.*

aspect of the problem being treated. I support the statement: "leaves out almost everything in your data except the few things that are really worth considering", but the process that leads to abandon the data involves that the reader must know which data the researcher has thrown away. We are faced with an oxymoron, I realise it, but it is inevitable. The reader is not required to know everything about the path taken by the scholar, but researchers must have seen everything to suggest what deserves to be chosen, at least from his point of view. Instead, I much appreciated the following sentence: "we want to make schools better, improve the lives of students and teachers, and promote to more equitable and efficient society through education. My advice is to resist this pressure and instead pursue a course of scholarly irrelevance." We have never gone very far, scholastically speaking, when we pursued profit. I say this from the standpoint of the history of Italian culture that has done nothing in his glorious past than chasing proposals of beauty, spirituality, harmony and so finding, apparently by chance, ideas of science, rationality and usefulness ... Two names for everyone: Leonardo Da Vinci and Galileo Galilei.

I am pleasantly surprised to learn that the goals of doctoral students in education who want to become researchers in education are those of truth, justice and beauty. In the cradle of philosophy and humanities (the Mediterranean sea), these categories had to do with the idea of measurement. There is no coincidence that these values accompany the religions as a sort of trinity. And Labaree invites everyone to focus our gaze at the moon instead of the finger: "Some researchers [...] need to keep asking themselves. What kind of society do we need and how can schooling help us realise 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?"

I do not know if in Italy these categories may prevail, perhaps the idea of change would be stronger, an inspiration that could explain the action of many past Italian educators from Don Giovanni Bosco to Don Lorenzo Milani, from Maria Montessori to Antonio Gramsci, but I do not know if there is one that has also led students of educational sciences. In any case, keeping the idea and the hope of change is a good rule for all teachers to hold together practical work and theoretical research.

I want to conclude my writing instead of using "amen" as Labaree does, with the ritual phrase, in our Catholic churches: "The Mass is ended, go in peace", remembering that the Latin motto "*Ite missa est*" indicates not only the end of the ceremony but the invitation to continue the mission, even outside the church. Leaving the language of metaphor it is a good thing that both, results of our work and these ideas about the ultimate meaning of research, are discussed, compared and, if necessary, criticized within the larger community of social sciences.

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Marc Depaepe
After the ten commandments ... the sermon?

It was bound to happen. After some supposed authority boldly announces ten command-ments for good research practices in his domain (Depaepe 2010), another one suddenly pops up – in this case a 'real' authority – who cannot resist the urge to give a sermon on almost the same subject. The historical but often repressed relationship between theology and pedagogy, repeatedly trumpeted by people like Fritz Osterwalder (2003), must have stuck in the subconscious of the historians of education. The religious metaphors prompted by the discussion about my opinion article (and which will undoubtedly resurface in com-ments on Labaree's research recommendations) pull no punches. Without going any further along the slippery path of what falls outside rational thinking, let me start by examining the most striking similarities and differences between Labaree's recommendations and myself. As intuition and emotion supposedly exert influence on each other – according to Wundt our subconscious even works like an unknown person who creates and produces for us, and ultimately tosses us the ripe fruit (see e.g. Dijksterhuis 2007) – I hasten to point out that David and I have no private battles to fight against each other. Insofar as I have got to know him through his books and articles (see e.g. Labaree 2007), and in the meantime I have had an opportunity to see him at several conferences and seminars, he champions scientific ideas with which I find it fairly easy to concur. So it will come as no surprise that I heard his 'ser-mon' with considerable empathy.

Similarities

The fundamental message that I read from this reflects what I have repeatedly argued, name-ly that historical research in education tends to be unruly. It does not immediately yield the results that policymakers and politicians want to hear. Nor does it butter up to rank and file teachers and others involved in education and upbringing. Due to this critical distance it erects a barrier against the hypertrophy of one-sided, utilitarian-designed educational re-search, which is generally based solely on empirico-analytical and statistically-quantifying thinking and demonstrates its merits through the highest possible quote indexes and impact factors. Historical contextualisation is and will remain necessary, if for no other reason than to understand the effects triggered by these seemingly innocent mechanisms in putting into operation and measuring the scientific output of persons, institutions and research domains. In other words, the traditional, more interpretational approaches to educational sciences, like the historical although also the philosophical (and perhaps even social) ones, may have become marginalised, but they are and remain indispensable in the forming of 'critical' intellectuals.

Therefore, I amply endorse Labaree's advocacy of 'irrelevant' research, or at least 'irrelevant' at first sight, because the reasoning presented above sufficiently demonstrates just how rele-vant the irrelevant can be. After all, relevance and irrelevance are categories tied to a certain (societal) criterion, standpoint or perspective (outside the actual research). Yet the paradox

is that this kind of ostensibly 'irrelevant' research is in danger of losing relevance the more it is consciously deployed to further the extrinsic criterion, standpoint or perspective. Applied to history, I formulated as the ninth commandment for educational history research that from the pedagogic past there are few concrete lessons to be learned for present-day educational practice and theory; when history is used for purposes other than intrinsic ones, I believe it ceases to be history. Consequently, it appears nonsensical to want to embed all forms of educational research in the short-sighted but nevertheless mandatory straitjacket of 'efficiency' that the neoliberal utilitarian ideology carries with it, for example through the appraisal and remuneration of research projects. Even though the market-driven perspective of the 'knowledge economy' may over time prove deadly for our disciplines – the historical or philosophical research traditions within educational research – it is not without danger when it comes to the seemingly 'relevant' (and thus 'correct') research in education. As Labaree shows with considerable persuasiveness, this is a field where not everything that shines is gold, and a lot, an awful lot, of that kind of research fails to fulfil its purpose, something with which I am compelled to concur when I read the press overviews of university research into education that received 'media' exposure in Flanders. I will spare readers a recap of the not always subtly chosen headlines, but it looks very much as though taxpayers' money is constantly being used in order to state the obvious. So it is clear that even seemingly 'relevant' research is subject to the same relevancy/irrelevancy paradox. As Labaree rightly comments, here again we see that 'the pursuit of relevance leads, ironically, to irrelevance'.

Finally, as a history researcher, I am attracted by Labaree's advocacy of a revaluation of the narrative in educational research, and particularly by his call to devote greater attention to the stylistic qualities of reporting. When it comes to this latter point, researchers all too often let themselves be guided by the equally mandatory requirements of editors who favour dull formats of standardised structuring and use of language and, consequently, promote an often unappealing 'uniformity' of articles. Books are definitely no longer a priority within the dominant paradigm of educational research. Nevertheless, one of the intrinsic joys of the trade lies in the creative use of language – the constant struggle to convey an idea correctly, or to put the right word in the right place. A creativity far removed from any obsession to fill in colours in pre-printed lines is certainly in its place here. The same certainly goes for the pursuit of beauty. But the packaging can obviously never be more important than the content. And it is precisely on that point that I want to make some observations.

Differences

One difference concerns the finality of the researcher's 'story'. In my view, a researcher must not simply produce a good story, he must also have the ambition to tell the best, the most acceptable story about what he or she is working on, and in relation to the history of education this usually means a story with multiple perspectives. An approach like this requires effort. Therefore, I would not encourage young people to be 'lazy'. Just as 'be lazy' cannot be an excellent research recommendation, the motto 'be wrong' can also prove problematic, certainly when considered independently of the context in which formulated – a danger certainly not inconceivable with such slogans. Under no circumstances may the historical 'story' produced through research be pure fiction. Its credibility depends in large measure on the validity of the truth. Even though the dividing line between historiography and literature production may have blurring increasingly since the 'linguistic turn', it is difficult to

place the writing of history (of education) – even the post-modernistic history – outside the perspective of an entitlement to truth (see Depaepe 2008). That is why Labaree immediately has to add to his rather astounding principles of professional practice – 'be wrong, be lazy, be irrelevant' – that is necessary to 'think of your work as an effort to balance the values of truth, justice and beauty'. For even he, as a historian, cannot step outside the truth category. We probably don't need to rush to take literally the slogan of 'be wrong, be lazy, be irrelevant', but rather we should see it as a metaphor for an alternative approach, outside mainstream educational research. Robust pronouncements like this – as part of a sermon – always have to be qualified, because the exaggerated figure of speech is inherent in every sermon. One last word about 'being irrelevant', which I referred to in the first part of my response. Relevance is something that research can attain only through the vehicle of an 'interesting' formulation of the problem – and I believe that insufficient allowance has been made for this in Labaree's advice, also in relation to the truth and beauty categories. All good research, including history of education research, stands or falls with a good question (see Depaepe/Simon 2009).

In spite of all this, I have not yet come to the most important comment that I have on Labaree's opinion article. What I find difficult to understand is that Labaree appears to want to dump all research about education in one heap and even appears to want to regard the history we are working on – both he and I – as 'educational research'. Research into education is not a monolith subject to some kind of 'educational research method'. What Labaree (and I) are endeavouring to do in our work is to write historical stories, and in my view those stories do not need the 'educational' label in order to survive – even if they are produced in 'schools of education' (as is generally the case in the United States) or in pedagogic institutes or departments of educational science(s) (as is the case in Europe). Their finality is ultimately 'historical' in the sense that they seek far more to make a contribution to the writing of social and cultural history than to the educational sciences (which in the German tradition are traditionally defined as action sciences). So why not differentiate between empirical research, which is apparently the target of the criticism of Labaree, and the many other forms of reflective research (historical, philosophical, comparative, or social) dealing with the phenomenon of upbringing and education?

As history researchers it is best for us to stick to our own business; what others make of it does not need to be our primary concern – live and let live is what I say. That is why all of my methodological, theoretical and historiographical comments were made mainly from the perspective of educational history. Within that confined framework, I have always argued in favour of methodological diversity. For if there is one domain where there is a need for a breath of fresh air, then this is certainly the one. At the same time, it is not so easy to say what educational history research should look like in the future; it is far simpler to say what it should not look like (see e.g. Depaepe 2004). But we won't go there.

In my opinion it is only when we as historians enter the domain of the history of educational research (conceived as part of the history of science) – something that Labaree certainly did successfully in our Leuven research community on the history and the philosophy of the educational sciences (e.g. Labaree 2010) – that we will be able to say something meaningful, based on our own competence, about the aberrations of empirical research. Ways we can do this include demonstrating how closely the preference for quantifying and operationalising upbringing phenomena via 'parameters' is related to a socio-Darwinian tinted, meritocratic view of society. By carrying out this kind of history of science approach, we might win both ways. On the one hand, we will be able to prove effectively our usefulness as guardians of cri-

tical insight, while on the other we will demonstrate to the younger generation of researchers how research that we consider 'good' can be carried out concretely (see e.g. Tenorth 2011). After all, the proof of the pudding is still in the eating.

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Andreas Hoffmann-Ocon
Die Erziehungswissenschaft ist kein Tempelbezirk der Dichtung

Wie kann man angemessen auf eine Predigt reagieren? David Labaree greift nicht im engeren theologischen Sinne auf diese Textsorte zurück, sondern spielt mit seinem Artikel darauf an, dass eine Predigt einen Monolog darstellt, der meist mit dem Stilmittel des Moralisierens und Appellierens die Zuhörenden zu einer Umkehr ihres Handelns und Denkens auffordern möchte. Dabei kann Labaree mit dem Impetus des Forschungserfahrenen vor dem Hintergrund des US-amerikanischen Universitätssystems sprechen. Sein Text kritisiert die in dem akademischen Leben eingeschliffenen Routinen erziehungswissenschaftlicher Forschung, und seine Ratschläge und Vorstellungen zielen auf den wissenschaftlichen Nachwuchs mit

der Aufforderung, sich in einem gewissen Sinne dysfunktional zu den wissenschaftlichen Konventionen zu verhalten. Aus seiner Sicht entwickelt sich Gelehrsamkeit in einem wissenschaftlichen Werk dann, wenn zum einen die Schlüsselprinzipien „be wrong, be lazy, be irrelevant" und wenn zum anderen neben der Wahrheitssuche gleichberechtigt auch die Werte der Gerechtigkeit und Schönheit verfolgt werden.

Als erste Vorbemerkung möchte ich festhalten, dass ich Labarees Predigt brilliant finde. Die zweite Vorbemerkung bezieht sich auf ein kurzes Innehalten und Nachdenken darüber, ob die von Labaree implizierten Bezugspunkte im System der Erziehungswissenschaft und ihrer Disziplinentwicklung in den USA dieselben sind wie im deutschsprachigen Raum. Meine Sichtweise fußt auf einer schwer hintergehbaren Standortgebundenheit mit dem Eindruck der erziehungswissenschaftlichen Disziplin in ihrer historischen Genese im deutschsprachigen Raum. Trotz nachweisbarer Phasen des transatlantischen Wissenschaftstransfers ist die Disziplin aufgrund selbstverschuldeter Abkoppelungen gesamthaft nicht ohne weiteres mit dem US-amerikanischen Verständnis der „Social Sciences and Humanities" zu vergleichen. Mit der Annahme, dass hier verschiedene Kontexte vorliegen, möchte ich auf einige Leerstellen der Predigt hinweisen, sie diskutieren und – wenn möglich – auch inhaltlich „füllen". Dabei lasse ich mich von der Annahme leiten, dass Labarees Fiktion eines idealen Nachwuchswissenschaftlers deshalb einem akademischen Heroismus Vorschub leistet, weil der imaginierte Doktorand auf Voraussetzungen angewiesen ist, die er nicht selbst hervorzubringen vermag.

Zur Kritik der Schlüsselprinzipien

Zunächst möchte ich die von Labaree angesprochenen Schlüsselprinzipien zum Aufbau einer professionellen Wissenschaftspraxis kurz kommentieren. Falls Labaree bei seiner Auswahl der Schlüsselprinzipien daran gedacht hat, wie erziehungswissenschaftliche Kreativität erzeugt werden kann, stellt sich die Frage, ob man die Erfolgsfaktoren eher genietheoretisch oder organisationssoziologisch bzw. systemisch bestimmt. Genauer gesagt: Denkt man beim schöpferischen Schreibprozess in den Erziehungswissenschaften an den einsamen Stubengelehrten, der sich mit einem krummen Rücken über seine Bücher beugt und in Form eines abduktiven Blitzes etwas vollkommen Neues entdeckt, oder an Forschende, die sich in der Gruppe mit einer Atmosphäre wechselseitiger Anregung, ähnlich einer Jazzband, improvisierend in intellektuelle Höhen schrauben. Daran knüpft sich die Überlegung, ob erziehungswissenschaftliche Kreativität eher in Distanz zur Hochschule – systemtheoretisch gesprochen „in loser Kopplung" – oder in enger organisatorischer Verzahnung freigesetzt werden kann. Weiter zuspitzend müsste geklärt werden, ob es flexible Hochschulorganisationen gibt, die sensibel für erziehungswissenschaftliche Erkenntniszuwächse in individualisierten Situierungen sind und die Forschenden zwar mit einer guten Bibliothek ausstatten, aber ansonsten vor den „Untiefen des akademischen Normalbetriebes" auch abschirmen können. Dass Hochschulen, welche ihre Forschenden im Klima steuerungstechnologischer Machbarkeitsvorstellungen stark in bürokratischen Strukturen innerhalb starren hierarchischen Systemen „einbauen" und selbst eher als „geschlossene Organisationen" funktionieren, ideenhemmend wirken können, ist aus organisationssoziologischer Perspektive bekannt (vgl. Kern 2008).

In dem Abschnitt „be wrong" beschreibt Labaree den individuellen Schreibprozess als Sinnmitte der Wissensproduktion in der Erziehungswissenschaft. Hier entdecke ich eine Leerstelle: Die erziehungswissenschaftliche Disziplin ist derart stark ausdifferenziert, dass in ihr

sowohl Forschende eingebettet sind, die sich in ihrer Logik eher der Arbeitsweise von naturwissenschaftlichen Teams mit ihren „Exzellenzketten" angeglichen haben, als auch Personen, welche eher auf einsame intellektuelle Prozesse setzen und weniger auf die tägliche Zusammenarbeit mit Kolleginnen und Kollegen angewiesen sind. Erziehungswissenschaft kann zugleich als Geisteswissenschaft, als quantitative und qualitative Sozialforschung betrachtet werden. Dabei wäre untersuchenswert, ob aus dem Nebeneinander oder der Verschmelzung verschiedener Forschungstypologien eine Typologie mit eigener Dignität erwächst. Selbst in der historischen Bildungsforschung ist der Differenzierungsgrad hoch. So erscheint es plausibel, dass ein Team, welches auf Zeitreihen gestützte Datenhandbücher herausgeben möchte, seine Erkenntnisleistungen anders zu Papier bringen wird, als eine Person, die eine Biografie einer Pädagogin verfasst. Soll aber der individuelle Schreibprozess beleuchtet werden, ist Labaree beizupflichten, dass das Abweichen von einem ursprünglichen Plan konstitutiv für die Forschung ist. Anders ausgedrückt: Da interessante Forschung oftmals einem chaotischen Prozess entspringt, sind die institutionellen Steuerungsbemühungen in Richtung auf reine Programmforschung mit der ihr innewohnenden Erratik zurückzuweisen.

Dass wissenschaftliches Schreiben ein Hochseilakt ohne Sicherheitsnetz sei, ist eine (zulässige) Dramatisierung Labarees. Klar, gerade in einem Dissertationsprozess kommt der Zeitpunkt, an dem man sich mit seinem Textentwurf angreifbar machen muss. Niemand muss sich jedoch ohne Hilfe auf dem Drahtseil zur Spitze des akademischen Zirkuszeltes begeben. Der Seiltanz sollte erstens nicht sofort vor dem ganzen Publikum aufgeführt werden. Für den Erkenntnisfortschritt ist es hilfreich, wenn zentrale Passagen der eigenen Schrift in einer kleinen Gruppe zwecks einer Textrevision diskutiert werden – zum Beispiel in dem bekannten Setting des Kolloquiums. Eine weitere, zweite Übung, die nicht ganz frei von Folgeproblemen ist, könnte daraus bestehen, den auf Papier festgehaltenen Befund gegenüber dem Publikum als vorläufig zu präsentieren. Ein Großmeister dieser Schreib- und Publikationspolitik war Niklas Luhmann, der 1985 – ein Jahr nach dem Erscheinen des Werks *Soziale Systeme* – behauptete, dass jenes, was er zuvor geschrieben habe, noch alles Nullserie der Theorieproduktion gewesen sei (vgl. Luhmann 1985). Tatsächlich ist die Angst vor der Kontextfalle insbesondere für Doktorierende enorm. Bei genauer Sichtung der selbst ausgewählten Materie wähnt sich jeder angehende Geisteswissenschaftler oder jede Sozialwissenschaftlerin in einem Roman von Umberto Eco mit unendlichen Verweiskontexten. Jede Begriffsbestimmung nötigt zur Nutzung von weiteren Begriffen, die wiederum klärungsbedürftig sind. Um diese missliche Lage aushalten zu können, liefert Eco mit seinem Konzept des offenen Kunstwerks (1973) selbst ein sedierendes Heilmittel. Demzufolge wird Kunst als eine Art und Weise beschrieben, ein bestimmtes Material zu strukturieren: Mehrdeutigkeit des wissenschaftlichen Werkes werde durch Begriffsbestimmungen nicht restlos auflösbar sein. Vielmehr sollte jeder Autor die Grenzen bestimmen können, innerhalb derer sein Werk eine Mehrdeutigkeit aufbauen kann und vom aktiven Eingriff der Rezipienten abhängen wird. Auch Labaree ist bemüht, beruhigende Botschaften an den wissenschaftlichen Nachwuchs zu vermitteln: Der akademische Text soll kein unumstößliches Lebenswerk sein, da die wissenschaftliche Gemeinde ohnehin als Korrektiv für eine zu schmale Datenbasis oder zu simple Argumentation in Aktion treten werde. Sicher, Forschende können mit ihren weiteren Texten in Widerspruch zu ihrem Frühwerk geraten. Trotzdem stellt sich die Frage, ob nicht auch die Kohärenz eines individuellen Werks bedeutsam für das Prestige im wissenschaftlichen Feld ist. Der Historiker und Schriftsteller Doron Rabinovici konstruiert in dem Roman *Andernorts* (2010) die Figur eines sozialwissenschaftlichen Hochschullehrers,

der indirekt in dem Essay eines Konkurrenten um eine Professorenstelle zitiert wird. Der Hochschullehrer empört sich nun über eine aus seiner Sicht unhaltbare Aussage, ohne zu bemerken, dass er der eigentliche Verfasser dieser bereits mehrere Jahre zurückliegenden Überlegungen ist. Von seinem Konkurrenten darauf aufmerksam gemacht, dass er seinen eigenen Text nicht wieder erkennt, gerät jener, der seine wissenschaftlichen Texte geschliffen in Deutsch, Hebräisch, Englisch und Französisch formulieren kann, weiter in den Verdacht, „Import-Export-Geschäfte" mit akademischen Ideen zu betreiben. Um nicht zu einer solch bizarr-komischen Figur im Hochschulbetrieb zu werden, ist es ratsam, die Deutungshoheit über seine eigenen Schriften im akademischen Diskurs zu behalten.

Im Abschnitt „be lazy" mokiert sich Labaree über den akademischen Ratschlag, ein wissenschaftliches Schreibprojekt erst zu beginnen, wenn jeder relevante Artikel dazu bereits gelesen wurde. Wie lässt sich die Gefahr bannen, sich im Minutiösen zu verlieren? Der Vorschlag Labarees zielt auf das wissenschaftliche Storytelling, nicht zuletzt auch deshalb, um eine stimulierende Idee in die wissenschaftliche Kommunikation einzuspeisen. Stimulierende Ideen könnten jedoch verschütt gehen, wenn von Doktorierenden stereotyp verlangt wird, dass sie zu Beginn ihrer Studie ein abstraktes und Furcht einflössendes Gebäude errichten, das die Rezipienten in den Schatten stellt und eher Ausdruck einer Versicherungsanstalt mit Vollkasko-Angebot darstellt (vgl. Narr/Stary 1999). Der Philosoph Marc-Alain Ouaknin (1990) verweist bei einer ähnlich gelagerten Problematik auf die rabbinische Gelehrsamkeit von Rabbi Nachmann, der fragte, ob es eine Möglichkeit gebe, dem Diskurs seines Gebiets zu entfliehen, um nicht von vorneherein durch die reiche Kenntnis von Texten und Auslegungen vorheriger Generationen befangen und paralysiert zu sein: Wenn ein Gelehrter einen Begriff erneuern will, wird er aufgrund eingehender Lektüre durch sein Vorwissen dergestalt erschüttert, dass er keinen neuen interessanten Satz aussprechen könne. Nur wer sein Wissen einschränken, eine Leere erzeugen könne, werde Sätze mit neuen Bedeutungen unter Verzicht der ohnehin bekannten einleitenden Bemerkungen formulieren können. Labaree rät dem wissenschaftlichen Nachwuchs, den verfolgten Interpretationsansatz nicht zu komplex werden zu lassen und nach Wegen der Simplifizierung zu suchen. Dies entspricht einer Gratwanderung und darf nicht dazu führen, dass sozialwissenschaftliche Klischees Rückwirkungen auf erziehungswissenschaftliche Texte haben. Ohnehin wird die Wissenschaftssprache oftmals als zu abstrakt bemängelt, selten wird jedoch etwas als „zu konkret" kritisiert. Liegt hierin möglicherweise das Bedürfnis nach autoritativer Klarheit und eine ausgeprägte Skepsis gegenüber wissenschaftlicher Eigenlogik vor, die auch in außeruniversitären Praxisfeldern wegweisend ist?

Mit Recht fußt Labarees Zugriff unter dem Punkt „be irrelevant" auf der Überlegung, dass die übertriebene Suche nach Relevanz und Brauchbarkeit erziehungswissenschaftlicher Studien für die Anforderungen weiterer Gesellschaftssysteme in die Irre führt. Unentwegte Suchbewegungen in die Richtung von tagespolitischer Nützlichkeit könnten zu einer Form von „embedded sciences" führen, also zu einer Forschung, die nur noch Instrumente für Projekte bildungsadministrativer Stellen entwickelt und nicht mehr ein kritisches Potenzial zum Beispiel gegenüber den schulpolitischen „Reformmaschinen" entfalten kann. Auch wenn der postindustrielle Nützlichkeitsakademiker als negative Projektionsfläche gilt, bedeutet das nicht, dass die Konstruktion der erziehungswissenschaftlich Forschenden als verwertungsferne Reflexionsmandarine einen tauglichen Gegenentwurf darstellt. Bereits der Wandel von der „Ordinarienuniversität", die im deutschsprachigen Raum bis in die 1960er-Jahre vorherrschend war, zur Gruppen- und Gremienuniversität, welche offen für die Orchestrierung gesellschaftlicher Anliegen durch die Hochschule war, folgte anderen Vorstel-

lungen. In Zeiten der New-Public-Management-Hochschulen sollte als Korrektiv derjenige Forschende gefragt sein, der noch die Kraft findet, Impulsgeber für Grundsatzdiskussionen zu sein. Insbesondere die Erziehungswissenschaft sollte den Auftrag annehmen, Transparenz unter komplexen Bedingungen zu schaffen und beispielsweise Anspruchsgruppen des Unterrichts- und Schulwesens dazu verhelfen, dies als ein komplexes System von Einrichtungen zu erkennen, in dem das Handeln einer einzigen Lehrperson bloß ein Faktor im ganzen Wirkungsfeld darstellt.

Zur Kritik der Ausbalancierung der Werte

Zu Beginn möchte Labaree zeigen, dass selbst wissenschaftliches Schreiben kaum nur aus extrinsischen Motiven heraus gelingt. Vielmehr beruht Schreiben auf Selbstklärungsprozessen, die neben der notwendig kalkülhaften Positionierung im Feld der Erziehungswissenschaft eine Rolle spielen. Jeder muss für sich bezüglich des langwierigen Aufbaus eines akademischen Habitus eine persönliche Legitimationslegende entwerfen. Diese Andeutung Labarees – auch in welchem Fall es besser ist, Taxi zu fahren als sich im Wissenschaftsbereich korrumpieren zu lassen – hat im deutschsprachigen Raum eine lange Tradition: Sie beginnt spätestens mit Friedrich Schillers Unterscheidung von Brotgelehrten, die Sklavenseelen im Reich der Freiheit seien, und philosophischen Köpfen, die die Zusammenhänge der Universalgeschichte erkennen. Ihre Fortsetzung erfährt sie anhand des Spotts gegenüber der Institution des Privatdozenten, der mittellos und in sozial prekärer Lage unentwegt dem Sinnversprechen der Forschung folgt (vgl. Rebenich 2002, 62).

„Die drei großen Ansätze der Erziehungswissenschaft seit 1945 – die geisteswissenschaftliche Position, die erfahrungswissenschaftliche Position und die gesellschaftskritische Position – sind nicht einander notwendigerweise ausschließende Wissenschaftsrichtungen. Jede dieser Richtungen erhält erst in der Zuordnung zu den beiden anderen ihre wissenschaftliche Funktion oder genauer: Erziehungswissenschaft kann [...] nur durch die Integration der drei gekennzeichneten Ansätze ihren Aufgaben gerecht werden" (Klafki 1971, 385). Mit diesen Worten leitet Wolfgang Klafki den siebten Abschnitt seines inzwischen schon historischen Aufsatzes *Erziehungswissenschaft als kritisch-konstruktive Theorie: Hermeneutik, Empirie, Ideologiekritik* ein. Der ganze Abschnitt von Labarees Text über die Balance zwischen den Werten Wahrheit, Gerechtigkeit und Schönheit erinnert an ein ähnliches Anliegen. Wenn ich unter Empirie Wahrheit und unter Ideologiekritik Gerechtigkeit subsumiere, bleibt mir nur noch in unzulässig reduktionistischer Weise (vielleicht zeigt Labaree Verständnis), die Nutznießerin der Hermeneutik, die geisteswissenschaftliche Pädagogik in die Nähe der Schönheit zu rücken – zumindest einer Schönheit der Verwilderung. Wolfgang Brezinka bezichtigte diese Strömung, einer präskriptiven, relativ ungenauen Alltagssprache verhaftet geblieben zu sein und die Vorliebe für inhaltsleere dunkle Worte sowie systematisch irreführende Formulierungen nicht überwunden zu haben (vgl. Brezinka 1971, 60). Die Angst, zu dieser Strömung hinzugerechnet zu werden, führte sicherlich in einigen Fällen dazu, dass zur Formulierung der Doktorarbeit eher holpriges Dissertationsdeutsch genutzt wurde. Labarees Anspruch, dass akademisches Schreiben ein Medium für persönlichen Ausdruck und künstlerische Schöpfung sein sollte, ist eindeutig zu weitgehend und erinnert doch zu sehr an die eingangs bereits erwähnte genietheoretische Haltung. Die Hochschule ist kein Tempelbezirk der Dichtung und frei nach Blixa Bargeld sollte der akademische Nachwuchs Zweckform-Texte schreiben und keine Oden für die Nachttisch-Schublade. Selbst Dichtung bringt man

im Sinne eines Klempners an: Wo's tropft, da muss die Dichtung rein (Bargeld 1994). Auch wenn sich Bildungsgeschichtsschreibung literarischen Genres annähern sollte, handelt es sich nicht um eine „fiction-making operation" (vgl. Nóvoa 2001). Wer dies behauptet – Labaree tut es jedenfalls nicht –, wäre unbelastet von der in der historischen Bildungsforschung notwendigen Unterscheidung zwischen Narrationen und Fiktionen.

Während auf der Ebene erziehungswissenschaftlicher Strömungen eine Integrationsidee sympathisch erscheint, aber beim jetzigen Stand der Disziplinentwicklung aus wissenschaftstheoretischer und methodologischer Sicht kaum beherrschbar wäre, adressiert Labaree eine Balanceidee direkt an den wissenschaftlichen Nachwuchs. Diese Balanceidee und die Schlüsselprinzipien des dysfunktionalen Systemverhaltens mögen auf den ersten Blick plausibel sein, können aber für einzelne junge Forschende Brisanz annehmen und zur Überbürdung werden. Der tatsächliche akademische Nachwuchs kann mit der Projektionsfläche des akademischen Nachwuchses systembedingt nicht mithalten, da Labaree von Voraussetzungen ausgeht, die für den Wissenschaftsbereich unabgegoltene Verheißungen sind und eher im künstlerischen Bereich sich etablieren können. Es bringt mir keinen Spaß, auf die Euphoriebremse zu treten, aber angelehnt an Siegried Bernfeld (1925/1990) möchte ich den Blick freimachen für das, was den akademische Nachwuchs als reichlich vertrackte Situierung auch erwartet: In der Dichtkunst sind die Figuren mit ihrer individuellen Leistung geschlossen. In der Erziehungswissenschaft nicht. Forschende sind in das System der Hochschule eingeflochten. Sie stellen sich gegen das System, dann denken sie Neues, denken gegen das Übliche, gegen das Gewohnte ihrer Zeit – es ist ihre Tragik oder Komödie, dass ihr Tun erst der Gefolgschaft bedarf, um vollendet zu sein.

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A Sermon as Counter-Sermon on Educational Research

As neophyte educational researchers, we often receive sermons of many a kind, from advisors, peers, committee members, editors, with regards to our professional conducts. With or without our consciousness, these sermons define, discipline, and normalize our life and make us particular kinds of researcher-subjects.

In what he calls the 'counter-principles' of educational research, David Labaree preaches a seemingly antithetical sermon: Be wrong; be lazy; be irrelevant; and balance the values of truth, justice, and beauty. In an advisory, corrective language, Labaree seeks to inoculate the researcher apprentice against the taken-for-granted rule of thumb. To me, his concern is more philosophical than technical. It presents an ethical dilemma over how to conduct oneself professionally, and raises the question of what is at stake for education researchers to juggle various standards of worthiness.

Labaree's first call (Be wrong!) is a choice between being 'safely boring' and 'speaking at the very edge of your data' at the risk of wrongness. What is at issue here is more than a willingness to make mistakes. It touches upon a broader philosophical debate on how one might understand 'data' and 'evidence' in social science research – not as proof of a 'right' (versus 'wrong') story, but as in need of new modes of analysis and critique. To be wrong, therefore, is not necessarily wrong, as it entails interstices between the objective and the imaginary, positivism and poetics, which do not index back to a universal core and an already-signified space of truism/fallacy.

Let me explain. French philosopher Jacques Rancière employs the logic of 'what if' in his speculative philosophy (Ruitenberg 2008) to open up a capacious understanding of 'data' not as committing thoughts to what we think we see and know (empirical realism), but what it might be (anticipatory realism). Instead of taming the 'data' into truth-claims based on a right-wrong nexus, the willingness to remain unknowing defies order and speaks of undecidedness, delay, and detour as an enabling condition for educational research. Feminist methodologist Patti Lather, in a similar vein, calls for "getting lost" as a way of knowing, "a move towards a thought of dissensus rather than consensus," and "a less comfortable social science" (2007, 1ff.). Therefore, if I could further Labaree's exhortation, 'to be wrong' invites a willingness to embrace a less comfortable social science where the refusal of truth-claims is enacted through both employing and troubling canonical categories (such as power, agency, subject) as universal explanatory variables.

Secondly, while earlier generations of researchers were prodded to work hard, according to Labaree, it is no longer clear that diligence could single-handedly produce research of high caliber. Instead, he urges for 'one-handed' scholars who do not belabor over complexity (Be lazy!) but tame stories to be 'focused and lean' with analytical tools 'as thin and sharp as a razor.'

At the risk of misreading Labaree, however, I would argue we need a new kind of diligence. Today, the evidence-based research tradition is met with serious challenges, as a mutation has

taken place in educational debates from concern over truths to irreconcilable polycentricity (see Baker 2009), from certitude of conceptual framework to ethical undecidability (see Rancière/Panagia 2000), from structuralist orientation to emergentist chaos (see Osberg/ Biesta 2007). Therefore, we need a new kind of diligence that inhabits a montage of sensibilities without reducing complexity to a rhetorical unity. A diligence that is not preoccupied with intelligibility or one kind of story but is both looking forwards and looking sideways for new meanings and possibilities, troubling the presumption of one world totality and the centrality of reason. Academic stories are always already unsettled, contested, and incomplete. As anthropologist Anna Tsing reminds us, attention to friction and fragmentation provides sensitivity to the messy, unpredictable, sometime awkward encounters with which we live; and it is necessary to "begin again, and again, in the middle of things" (2005, 2).

This ties to Labaree's next urge to pursue a course of scholarly irrelevance. In his view, relevance promotes short-sidedness because the need to offer 'what works' in order to tailor-fix a problem compromise the understanding of broader sociohistorical contexts within which the problem arises. In addition, one man's meat might be another man's poison such that relevance is always already a relative term. I would venture even further that while academic pragmatism (Be relevant! Problem solve!) offers description and diagnosis of problems, we need to question the certainty with which we so confidently prescribe what is good for all and what it means to be human through the seemingly benevolent mission of education. Besides, the current orientation towards 'what works' often neglects the systems of reason that make certain 'problems' intelligible (Popkewitz 2008).

Let me give an example. In the study of dropout within the policy of compulsory education, we often encounter canonical critique of what factors contribute to the policy failure, such as culturally irrelevant curriculum, test-oriented pedagogy, poorly trained and underpaid teachers, decrepit infrastructure, familial poverty, parental educational background, and so on. The issue of dropout is often framed as an obstacle to the progressive vision of the nation. Research reports often conclude with policy recommendations for 'best practices' to curb dropout. Rarely do we see analysis that troubles the very normative ideals and teleological visions at work in educational programming. Dropout suggests the limit-points of schooling as a destabilizing site and renders compulsory education a fragile attempt at inclusion when definition and orientation to inclusion are not shared in the first place. When school pedagogy constitutes 'legitimate' ways of perceiving social life and prevents other possibilities of knowing, what it means to educate is packaged in enclosed binaries of literacy/illiteracy, cultured/uncultured, science/superstition, modernity/traditionalism. This forecloses the lived domains of rituals, worldviews, aesthetic-social interactions that have long carried educative functions and that suggests there is more than one way to 'know,' more than one way to represent the world, and more than one imaginary domain called the world. Thus being irrelevant, to me, entails, counter-intuitively, troubling commonsensical concepts (such as literacy, quality, justice) and thinking about schooling and education in ways we thought unthinkable.

Finally, at the climax of the sermon comes the existential question: how to live an integrated career that is not bifurcated between a form of work and a way of life. Here Labaree preaches truth, justice, and beauty, notwithstanding the slipperiness of each word, as the promise of the fullest satisfaction. Yet paradoxes enter. Firstly, Labaree urges educational researchers to 'sort truth from fiction' against political and ideological demands. He claims that truth can be obtained by focusing on the technical issues of schooling and by winnowing out half-

truths, misconceptions, and lies. Yet, there is a different aspect of 'truth' that makes it contested whether seeking truth is germane to one's scholarly worthiness. Truth-claims are both indispensable and inadequate, which requires us to live with 'the epistemological paradox of knowing through not knowing' (Lather 2007, 136) and remain cautious of the limits of the foundational concepts of our analysis.

Secondly, Labaree encourages scholars to seek morally desirable ends and balance seeking truth (with analytical skills) and seeking justice (with philosophical commitment). Yet, in the collective appeal to make schooling more inclusive, equalitarian, and liberatory, educational researchers must heed another caveat: that truth and justice are ultimately incalculable and unknowable (Benjamin 1977). It is motivated by broad possibilities of the imaginable and by allowing the subject to emerge and to be always emerging, not by reifying existing discursive norms in which the subject is decided and codified.

Conventional understanding of justice and truth is couched within a language of rights and freedom, yet such liberatory language often obscures an uneven battleground in which certain conceptions of rights and freedom are favored over others. There is something beyond the secular belief in the progressive function of education, beyond the coherent projection of schooling as emancipatory, beyond the normative understanding of who the child is and should be (e.g. problem-solving, technology-savvy, scientifically-literate, oriented towards life-long learning). That is to say, instead of simplistic appeals to truth and justice, we need to re-orient ourselves to other possible modes of doing, being, and seeing. The readily accessible notions (such as justice, truth, reason) become a site of immanence and bear the necessity of their own critique, rather than being the unquestioned foundations of educational research.

Lastly, Labaree urges for scholarship that beautifies. To beautify, he says, is not only an external embellishment of the world but also an inward search for creativity on the part of the researcher. Labaree rightly points out that the aesthetics of research comes down to writing. Instead of producing 'tone-deaf, lifeless, and esthetically repellent' reports, scholars ought to be trained to detect rhythms and music in the existing literature and contribute in such a way that is meaningful, worthy, and aesthetically pleasing. Although Labaree does not go any further, the problematic of writing, for me, belongs to a philosophical domain more than a stylistic choice. How can writing, necessarily circumscribed by grammar, narrate the inexhaustible richness? This is fundamentally an ethical question that obliges us to further ask: What becomes possible when we refuse to produce tidy texts and when we write with provisionality? What if writing becomes a site of doubt rather than a site of affirmation? What if writing outside the normalized/routinized structure of categories becomes an ethical imperative? It behooves us to reconsider writing, the vehicle of our report and the rite de passage to scholarly merit, as a site where the relationship between the writer-researcher and the researched is not between the representing and the represented, but a mutual process of immanence and becoming. This, to me, is the heart of the aesthetics of scholarship.

In sum, although we need a kind of vigilance in following Labaree's sermon as counter-sermon on educational research, his concern is genuinely appreciated for a shift in sensibilities and reconceptualization of foundational concepts often unproblematized in educational research. Free of academic jargons, peppered with humor, spontaneity, and light-heartedness, the 'Sermon' bestirs us, both emerging and acclaimed education researchers, into (re)thinking and (re)action.

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Kate Rousmaniere
"Data are noise, and our job as scholars is to find the music"

In my early years as an assistant professor, right out of graduate school, I had a reoccurring dream in which I had to make a momentous decision. The scene was always the same: I was standing at a crossroad at the bottom of a valley, with two paths winding out of the valley in opposite directions. Standing on the two oppositional paths were two equally oppositional people, or sometimes a group of people, each of which was beckoning me to join them. On one path was my dissertation supervisor, my dean, or sometimes a person who I knew to be a famous American historian, each of whom solemnly urged me to follow them. On the other path were different groups of new friends and colleagues I had recently met at conferences, including a number of European academics with whom I was currently working on what became the "Silences and Images" workshops on new methods in social history. That group also beckoned me, usually in the midst of laughter and on-going conversation. If the symbolism of these figures was not obvious enough, in one night's dream, the first group was replaced by a pile of old and dusty books, and the second group by a lively twister of swirling air.

Even rudimentary dream analysis skills taught me that the scene represented my own personal struggles in my new professional field. At the time, I was completing my first book,

City Teachers, a revision of my dissertation, and I was employed at my first university job. I was beginning to spread my intellectual wings into new areas of sociology, anthropology, and cultural studies, and new historical methods of oral and visual history and textual analysis. To me, the crossroad dream symbolized in a rather dramatic way that my new intellectual interests, represented by the swirling air, was a decisive step away from my own traditional training in historical research.

At the time, I suspected that following these new interests came at some risk; indeed, a few colleagues had advised me to build my career on traditional American social and intellectual history of education, and not to take on anything too new before I earned tenure. And in the early 1990s, traditional meant straight-forward historical narrative firmly based on archival research. Social history methods had expanded to new types of sources (personal letters, census and other statistical data, architecture and geography), but many historians still held oral history as an experimental methodology that was useful only as a way of providing anecdotal information to fill in data gaps. Similarly, visual analysis was largely descriptive and illustrative. A few scholars were beginning to explore other methodologies with the objective not just of providing new data but of changing interpretive analyses of history. For example, the new approach of life history framed biography in the terms that the subject defined, however contradictory. New methods of visual analysis explored representation, production and meaning. Walter Benjamin's montage, Michel Foucault's archeology, the concept of discursive analysis, and other elements of post-modern scholarship were all infiltrating the edges of historical scholarship and methodology in this period, offering new ways to think about and do history.

But these approaches were still quite new and radical, and the obsession of most historians remained collecting data: how to find new data, how to record it, how to validate it.

Although I had conducted oral histories for my dissertation on the history of teachers, I had used them primarily as supplementary information sources, to fill in the gaps of the looming problem that was my study: how to write the history of school teachers when there was so little archival evidence about and by teachers. That problem in and of itself interested me: given the absence of traditional sources, I had begun to look for different types of sources, and to look at traditional sources in different ways, paying attention to images, design, mood, shadows and hints of meaning. For that work, I read new scholarship on history and memory, and studied alternative narrative formats that included imagery and fiction.

Ironically, this type of work drove me back to my undergraduate studies in the classics, a seemingly most traditional field, but one which had inspired me to think creatively about history through the close analysis of fragments of Latin phrases, and the imagination of the ghosts of daily life while walking through the Roman forum. So I had already suspected that while data was necessary for historical scholarship, the absence of data opened up an entirely new door of scholarly questions.

By the mid-1990s, I had met Martin Lawn and Ian Grosvenor in Birmingham and we planned a series of small international workshops on new methods in the social history of education. We called the work "Silences and Images" to describe our collective interest in the "silences" of history, or what the sources don't tell us. In the workshops we pushed ourselves to develop new interpretive analyses of the history of the classroom, drawing on innovative sources, including our own imaginative, emotional, and personal memory responses. We worked collectively to question the nature of sources, developing a critical understanding of validity and authenticity. The work was exciting, refreshing, and risky.

David Labaree's inspiring essay reminds me of these early days of exploring alternative approaches to history, and I am particularly struck by his tongue-in-cheek challenge to historians to adapt our approaches to data by taking the risk to "be wrong" and "be lazy." History, Labaree reminds us, is not just the stringing together of data, but the construction of data to form an intellectual argument. The relationship between intellectual ideas and data is not necessarily a linear or causal one, but rather interactive and creative as historians shape an argument through their own interpretive use of data. To do this work, historians need the skill of data collection and analysis, but they also need confidence in their own assertions and interpretations. It is easy, relatively speaking, to take public validated data and retell a traditional narrative. It is more risky to take private or unstable data and create a new narrative. To take that risk, we need to loosen up our obsession with data, and I think this is where Labaree urges us to think about being "wrong" and "lazy." Step away from the database; take a vacation from the archives; take a break from the computer. The heart of the historical story may not ultimately evolve from the data, but from the absence of data, echoing the advice of musicians from Debussy to Jimi Hendrix to Miles Davis to focus not on the notes, but what's between the notes. Or as Labaree writes: "Data are noise, and our job as scholars is to find the music."

Playing between the notes leads to the beauty of personal expression. In the "Silences and Images" meetings, the absence of evidence opened up suggestive possibilities for creative thought and writing. At one meeting, as we sat in a reconstructed 19th century classroom, we considered how our own memories as children at school could inform our historical perspectives. In another meeting, we asked participants to create a montage of snap shots of a contemporary school, leading us to shape and reshape the possibilities of meaning through visual imagery. In these meetings, we were reaching beyond the edges of the data, looking for shapes and melodies and emotions that our past reliance on hard data had obscured. Exploring the silences of history became a creative challenge, allowing for what Labaree describes as the work of connecting history with emotion, personal experience, and sensory and aesthetic expression.

Deeply implied in Labaree's essay is another personal message: that of gaining confidence in one's work. It takes confidence to take a risk, and I felt that personal challenge in my nighttime struggles over which path to choose. In my own experience, it was professional collegiality that helped me gain the confidence to choose my path. Others may find inspiration in other resources, including such sermons as David Labaree's. I wish we had more avenues for such inspiration in our historical field, and more ways to connect our creative struggles with those of painters, novelists, and musicians by temporarily loosening our desperate grip on data.

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Jörg-W. Link
„Wenn et Bedde sich lohne däät"

Diesmal kommt die Botschaft aus Stanford und die Predigt hat zwei Teile. Der erste ist originell, hält die Gemeinde wach und regt zum Mitdenken an. Für den zweiten braucht man einen guten Messwein oder besser noch einen guten Scotch. Denn im zweiten Teil der Predigt werden die Ratschläge väterlich und pastoral, pathetisch und traditionell und – seien wir ehrlich – teilweise auch langweilig. Für die jungen Gemeindemitglieder, an die sich David Labarees Predigt in erster Linie richtet, mögen sie dennoch hilfreich sein. Manch ein Doktorand wird die Predigt vielleicht konfirmiert verlassen und sich auch der Wahrheit, Gerechtigkeit und Schönheit verschreiben. Auf jeden Fall wird er wissen, welchen Regeln das Leben seiner wissenschaftlichen Gemeinde folgt. Und das ist auch gut so.

Es ist vor allem der erste Teil der Predigt, der meine Aufmerksamkeit weckt und einen Kommentar evoziert. Diesen Kommentar schreibe ich aus der Perspektive eines Bildungshistorikers und mit großer Sympathie für die Wissenschaftskultur, die Labaree vor dem Hintergrund 30jähriger und durch *trial and error* geprägter Erfahrungen als Forscher dem wissenschaftlichen Nachwuchs predigt. Mit kritisch-sympathisierender Sicht werde ich auch deutlich machen, wo ich Gefahren in Labarees gut gemeinten Ratschlägen sehe. Den zweiten Teil der Predigt werde ich nur durch Assoziationen ergänzen, die das Traditionelle in Labarees wissenschaftlichem Ethos zeigen, das aber nicht weniger wichtig ist – nur eben nicht originell.

„Be wrong; be lazy; be irrelevant; and think of your work as an effort to balance the values of truth, justice, and beauty." So fasst Labaree seine Predigt über erziehungswissenschaftliche Forschung in einem Satz zusammen und meint damit *„counter-principles of professional practice"* zu benennen. Das sind sie zum Teil in der Tat. Welche Botschaften kommen bei mir an?

Be wrong: Wenn du in deinem Projekt auf der Grundlage deiner bisherigen Quellen und Materialien eine interessante These bilden konntest, dann teile sie der *Scientific Community* mit und warte nicht darauf, bis du sie von allen Seiten absichern kannst. Vielleicht ist die These falsch. Macht nichts. Denn der Einwand deiner Kollegen bringt dich in der Sache weiter. Und insgesamt kommt die Sache an sich so auch weiter voran.

Be lazy: Geh als Historiker nicht noch in das 156. Archiv, in dem vielleicht noch die Abschrift einer Durchschrift des Schreibens liegt, das du längst kennst, die aber noch ein paar Randnotizen des Bearbeiters enthalten könnte. Geh lieber spazieren, streck die Nase in die Sonne und lausch in dich hinein. Was hast du schon gefunden? Worum geht es? Sind die Zusammenhänge nicht eigentlich klar? Welche interessante Perspektive ergibt sich wirklich aus deinem Material? Was ist das Neue? Schreib sie auf und mach sie bekannt. *Be lazy* könnte man didaktisch auch mit dem „Mut zur Lücke" (Specht 1948/1993) übersetzen und dabei das stark machen, was man in der Unterrichtsplanung „didaktische Reduktion" nennt. Was ist wirklich wichtig? Was ist das Zentrum meines Themas? Was kann, was muss ich weglassen, damit der rote Faden klar wird und meine Botschaft ankommt? Was will ich als Lehrer auf welchen Wegen und mit welchen Mitteln vermitteln? Und wenn ich als Lehrer nicht nur inhaltlich sinnvoll und angemessen, sondern auch für den Lernprozess der Schüler effektiv

arbeiten will, dann benötige ich ein angemessenes didaktisch-methodisches Arrangement, in dem sich die Lernprozesse der Schüler entfalten können. Übertragen auf die Predigt von Labaree ist das dann *beauty* im Sinne der zweiten Trias.

Be irrelevant: Wie gut, dass wir als Bildungshistoriker ohnehin zumeist nicht in Tathaft für die Lösung aktueller pädagogischer Probleme genommen werden. Diejenigen, die als Politiker oder praktische Pädagogen unter Handlungsdruck stehen, erwarten von bildungshistorischer Forschung in den seltensten Fällen Expertise für den pädagogischen Alltag. Dass das eine falsche Haltung ist, will ich hier nicht weiter diskutieren. Deutlich macht das aber, dass wir als Bildungshistoriker fast automatisch das Privileg haben, irrelevant im Sinne Labarees zu sein. Hab den Mut, antizyklisch zu forschen, auch wenn es dafür gerade keine riesigen Förderprogramme gibt. Erfinde nicht immer wieder das Rad neu, nur weil es gerade von überall gefordert wird. Think big! Kümmere dich um die Dinge, die dich selbst wirklich interessieren und schiel nicht auf vermeintliche Konjunkturen und Moden. Längerfristig ist das sinnvoller.

Wahrheit und Gerechtigkeit sind natürlich auch für Bildungshistoriker entscheidende Qualitätsmaßstäbe für ihre Forschung. „Der noble Traum der Historiker", so Thomas Nipperdey, „bleibt es, eine Vergangenheit aus ihren eigenen Möglichkeiten zu begreifen und nicht aus unseren Möglichkeiten oder unseren Perspektiven" (Nipperdey 1986, S. 232). Dahinter steht die Frage, wie objektiv Geschichte, das heißt wie nahe sie an der historischen Wirklichkeit sein kann und damit wie wahr sie ist. Das Testverfahren für diese Wahrheit sind bei den Historikern die Quellen. Die Überprüfung an den Quellen „sichert ein gewisses Maß an Objektivität" (ebd.). Nun ist es eine historische Binsenweisheit, dass es immer viele Geschichten und damit viele Wahrheiten gibt. Das gilt für historische wie für gegenwärtige Wirklichkeiten. „Die Geschichte gibt nicht ein angebliches Ganzes, das ist eine Illusion, sondern sie erzählt einen Strang der Realität in einer Retrospektive" (ebd., 221). Durch die Überprüfbarkeit an den Quellen kann Geschichte jedoch in dem Sinne „objektiv" sein, dass sie sich auf die wirkliche Vergangenheit bezieht und intersubjektiv Geltung hat (vgl. ebd., 233). Zweifellos ist das eine Form der Wahrheit und Gerechtigkeit, um die es auch Labaree geht. Als Historiker möchte man hier in seine Predigt hineinrufen, dass das so neu doch alles nicht sei. Aber darum geht es auch nicht. Es geht Labaree zu Recht um eine andere Wissenschaftskultur.

Eine Predigt konfrontiert nicht selten die unvollkommene Wirklichkeit mit dem hehren Ideal. Und die Wirklichkeit erziehungswissenschaftlicher Forschung scheint weit von den Idealen in Labarees Predigt entfernt zu sein. Dies gilt offenbar ebenso für die anglo-amerikanische Forschung, aus der die Predigt stammt, wie für die deutschsprachige, aus der der Kommentar stammt. Ich jedenfalls habe eine solche Wissenschaftskultur selten erlebt. Glaubenskriege, Jahrmärkte der Eitelkeiten und Selbstgefälligkeiten dagegen häufig. Würde David Labaree so von der Kanzel predigen, wenn er es anders erleben würde? Sicher nicht ohne eigenen Erfahrungshintergrund weist er darauf hin, dass der Diskurs auf erziehungswissenschaftlichen Konferenzen *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*. Sich dies immer wieder bewusst zu machen, ist nicht nur für Nachwuchswissenschaftler (über)lebenswichtig.

„Data are noise, and our job as scholars is to find the music" – kann man die Aufgabe eines Forschers mit einem schöneren Bild beschreiben? Wir komponieren und die Melodie sollte auch schön sein. Unsere Aufgabe sei es, so Labaree, das Wahre und Gerechte auch als Kunst-

werk zu gestalten. *Beauty* sollten unsere Texte ausstrahlen und wohl klingen: *"find the music in the way words come together and in the way they surge to a crescendo and then subside gracefully into silence"*. Warum gelingt das anglo-amerikanischen Kollegen immer so viel besser als deutschen? Warum sind so viele Texte der deutschsprachigen (Erziehungs-)Wissenschaft so unlesbar? Das gilt für quantitativ orientierte Arbeiten ebenso wie für qualitativ orientierte.
Weil dies der eher langweilige Teil der Predigt war, ließ ich mich von der Melodie aus Labarees Predigt wegtragen und wurde an dieser Stelle erinnert an Kurt Tucholskys Ratschläge für einen schlechten Redner (Tucholsky 1930/1975). Schlechten Rednern rät Tucholsky unter anderem: „Fang nie mit dem Anfang an, sondern immer drei Meilen vor dem Anfang! [...] Denn das hat der Zuhörer gern: dass er deine Rede wie ein schweres Schulpensum aufbekommt; dass du mit dem drohst, was du sagen wirst, sagst und schon gesagt hast. Immer schön umständlich. [...] Sprich mit langen, langen Sätzen – solchen, bei denen du, der du dich zu Hause, wo du ja die Ruhe, deren du so sehr benötigst, deiner Kinder ungeachtet, hast, vorbereitest, genau weißt, wie das Ende ist, die Nebensätze schön ineinandergeschachtelt, so dass der Hörer, ungeduldig auf seinem Sitz hin und her träumend, sich in einem Kolleg wähnend, in dem er früher so gern geschlummert hat, auf das Ende solcher Periode wartet [...] Fang immer bei den alten Römern an und gib stets, wovon du auch sprichst, die geschichtlichen Hintergründe der Sache. Das ist nicht nur deutsch – das tun alle Brillenmenschen. Ich habe einmal in der Sorbonne einen chinesischen Studenten sprechen hören, der sprach glatt und gut französisch, aber er begann zu allgemeiner Freude so: ‚Lassen Sie mich Ihnen in aller Kürze die Entwicklungsgeschichte meiner chinesischen Heimat seit dem Jahre 2000 vor Christi Geburt'... Er blickte ganz erstaunt auf, weil die Leute so lachten." Für gute Redner fallen Tucholskys Ratschläge kürzer aus: „Hauptsätze, Hauptsätze. Hauptsätze. Klare Disposition im Kopf – möglichst wenig auf dem Papier. Tatsachen, oder Appell an das Gefühl. Schleuder oder Harfe. Ein Redner sei kein Lexikon. Das haben die Leute zu Hause. [...] Merke Otto Brahms Spruch: Wat jestrichen is, kann nich durchfalln."
Zugegeben: Tucholsky hatte nicht die Wissenschaft im Sinn bei seinen Ratschlägen. Doch auch Wissenschaft – das ist trivial – findet in schriftlicher und mündlicher Rede, findet im Diskurs statt. Sowohl Tucholskys als auch Labarees Ratschläge würden ihr gut tun. Nipperdey sprach von einer „Kommunität der Forscher" (1986, 225), deren wesentliche Elemente Diskussion und Kritik seien, und die einer „Basisethik" (ebd.) folge. Wenn ich mir indes vorstelle, dass ein junger Kollege Labarees Ratschläge befolgt und in einem Vortrag oder einem Artikel bewusst *wrong*, *lazy* und *irrelevat* ist, weil es ihm um einen fortschreitenden diskursiven Dialog in der Sache geht, dann dürfte die Reaktion des deutschen Publikums oft eindeutig ausfallen: Ihm wird verbal der Kopf abgerissen, hat er sich doch nicht an die Rituale wissenschaftlicher Präsentationen gehalten und die Bühne eben nicht genutzt zur selbstgefälligen Selbstdarstellung seiner wissenschaftlich wasserdichten Resultate. Im Ergebnis dürfte eins klar sein: Als Forscher ist er irrelevant. Vermutlich können es sich nur etablierte Forscher leisten, nach Labarees Ratschlägen *wrong* und *lazy* zu sein.
In Labarees Predigt steckt auch ein Plädoyer für große, inspirierende Perspektiven (*"something ... that might be enlightening"*), historisch gewendet: für einen „Mut zur historischen Synthese", wie ihn Hans-Ulrich Wehler vor längerer Zeit auch schon einmal formulierte.[3]

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3 Wehlers Text vor dem Hintergrund von Labarees Predigt zu lesen, ist insgesamt aufschlussreich, tauchen doch hier Gedankengänge auf, die denen von Labaree ganz ähnlich sind: Etwa wenn Wehler an Nipperdeys *Deutsche Geschichte 1800 bis 1866* den Stil als „klar, kraftvoll, geschmeidig" lobt, „oft eher eine um Verständnis werbende

Solche groß angelegten und neue Perspektiven eröffnende Synthesen erfordern indes nicht nur Mut, sondern auch eine profunde Sach- und Quellenkenntnis, über die Nachwuchsforscher meist noch nicht verfügen können. Doch der Weg zu solch groß angelegten Synthesen ist auch mit Stolpersteinen gepflastert, die aus Labarees Ratschlägen entstehen können. Ein Beispiel dafür ist Jürgen Oelkers (2011) jüngste Veröffentlichung zur Reformpädagogik. Er unternimmt hier unter anderem den Versuch, die deutsche Reformpädagogik grundsätzlich neu zu bewerten. Zweifellos handelt es sich bei seinen Analysen der pädagogischen Praxis in vier frühen Landerziehungsheimen um „something … that might be enlightening"; und zweifellos kann man Labarees wrong und lazy in diesem Buch finden. Doch der Versuch einer Neubewertung der deutschen Reformpädagogik scheitert letztlich, weil der Autor viele bekannte Details aus der vorliegenden Forschung und den Quellen ignoriert, um seine neue große Perspektive zu zeichnen (vgl. Link 2012). Aber ohne Detailkenntnis der komplexen Gelingens- oder Scheiternsbedingungen unterschiedlicher historischer Schulversuche bleiben synthetische Großperspektiven lückenhaft, oder die Perspektiven gehen schlicht an historischen Realitäten vorbei. Das mikrohistorische ‚Klein-Klein' (Kocka 1994, 34) bleibt als Verstörung der großen Perspektiven unverzichtbar. Ohne „dedication to complexity" droht das Scheitern großer Perspektiven. Leichtfertig sollte man Labarees Ratschlägen demnach besser nicht folgen, es sei denn, man beabsichtigt eine bewusste wissenschaftliche Provokation. Aber auch das können sich wohl eher nur etablierte Wissenschaftler leisten; und auch nur dann, wenn die Diskussionskultur so verfasst ist, wie sie Labaree predigt. Dann ist es nämlich ohne wissenschaftlichen Totalschaden möglich, was Labaree zu Recht für sinnvoll hält: „Let them point out where you are wrong."

Die Wissenschaftskultur aus Labarees Predigt setzt nämlich eine konstruktive Diskussionskultur voraus, in der die Bedeutung der Sache im Mittelpunkt steht und nicht die Bedeutung der Forscher. Einzelerkenntnisse fügen sich hier dialogisch zu einem Mosaik zusammen und führen zu einem gemeinsamen Erkenntnisgewinn – oder aber sie werden dialogisch korrigiert und befördern den Erkenntnisgewinn dadurch ebenfalls. Einst bezeichnete man das mitunter auch als herrschaftsfreien Dialog. Das freilich setzt Gemeindemitglieder voraus, die sich auf einen solchen konstruktiven Diskurs um der Sache willen einlassen wollen und nicht nur eine Gelegenheit suchen, um ihrerseits von der Kanzel das einzig Wahre, Gute und Schöne zu predigen. Nun wird niemand ernsthaft behaupten wollen, dass es eine solche Diskussionskultur nirgends gäbe. Gleichwohl begegnet man ihr zu selten. Aber wenn eine Tagung – oder auch ein publikationsbasierter Diskussionsprozess – durch eine solche Diskussionskultur geprägt ist, dann sind die Ergebnisse in der Sache meist tatsächlich konstruktiv und weiterführend. Für die deutsche Bildungsgeschichte kann man dies für die Jahrestagungen und für die Nachwuchstagungen der Sektion Historische Bildungsforschung in der DGfE durchaus behaupten und belegen. Und doch sind diese Kulturen weitgehend singulär – und die Gemeinde ist überschaubar. Kooperation statt Konkurrenz in der Sache, Deutungsvorschläge statt Deutungshoheit – das wären (naive) gemeinsame Überzeugungen in einer solchen Wissenschaftsgemeinde, wie sie Labarees Predigt entwirft. Die Botschaft hör ich wohl, allein mir fehlt der Glaube. Und da halte ich es dann mit dem Kölner Maler und Musiker Wolfgang Niedecken: „Wenn et Bedde sich lohne däät, wat meinste wohl, /

Rede in lebhaftem Vortragsstil als eine langatmige wissenschaftliche Schreibe"; und auch wenn betont: „Es ist ein Buch voller origineller Einfälle, voll unvoreingenommener, überraschender Perspektiven, voll scharfsinniger Interpretationen" (Wehler 1988, 70).

wat ich dann bedde däät, bedde däät. / [...] / für die zwei Philosophe, die schänge, / en nem Elfenbeinturm en Klausur, / die sick Minschejedenke sich zänke, / uss Erbarmen e Stoßjebet nur, e Stoßjebet nur."[4]

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Jeffrey Mirel
Thoughts on David Labaree's "Sermon on Educational Research"

Sermons are not a genre that I am comfortable with because they usually demand a leap of faith that I am unwilling or unable to make. But this was *not* my experience with David Labaree's *Sermon on Educational Research*. Indeed, I was increasingly delighted by the strength of his arguments, the thoughtfulness behind his suggestions, and the clarity and elegance of his prose.

David's sermon is inspiring and I look forward to discussing it with my graduate students for years to come. In these discussions, I assume that one of the main questions my students will ask is, "How can we put these suggestions into practice?" The rest of my commentary on the

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4 „Wenn das Beten sich lohnen täte, / was meinst du wohl, was ich dann beten täte. [...] Für die zwei Philosophen, die schimpfen, / in einem Elfenbeinturm in Klausur, / die seit Menschengedenken sich zanken, / aus Erbarmen ein Stoßgebet nur." – Das Lied *Wenn et Bedde sich lohne däät* stammt von der Kölschrockband BAP; Text: Wolfgang Niedecken; Musik: BAP; veröffentlicht 1982 auf dem Album *Vun drinne noh drusse*.

sermon will focus on that question looking at each of the four ways that David argues new educational researchers should approach their work: be wrong, be lazy, be irrelevant, and try to balance truth, justice, and beauty. I will discuss each in turn.

Be Wrong. Writing well is a fundamental skill for success in educational or any other form of social science research. While there are some amazing scholars who seem to effortlessly produce a cascade of brilliant, readable books, essays, and reviews, the fact is that for the vast majority of us writing is slow and often painful. Even Ernest Hemingway, the Nobel Prize winning author, supposedly used a bull fighting metaphor to describe his feelings about facing a blank piece of paper. It was like confronting a "white bull."

Why is writing so hard? As David explains, writing forces us to think deeply about our work. This is the essence of "being wrong." It means that we must recognize writing as an iterative process of drafting and re-drafting that ideally sharpens our thinking and shows us where our data and/or our arguments break down. Discovering these weaknesses is often a painful experience. But to use a common phrase in the education lexicon these are "teachable moments" that can make us better thinkers and writers.

I would like to add one other aspect to David's principle of being wrong. Besides sharpening arguments and identifying weaknesses, being wrong can also be equally helpful in another key aspect of thinking and writing – understanding audience. Most new researchers begin their careers with ready-made audiences for their work, namely their dissertation committee. But as soon as they leave that comfortable academic nest, problems arise. Suddenly a veritable forest of audiences appears – students of their own, editors of journals they want their articles to be in, foundations that they hope will fund their research, colleagues who will assess them for promotion and tenure, acquisition editors from presses they hope will publish their books, even the passenger they chat with on a long plane ride who asks what they do. As educational researchers, they have to learn how to reach all these audiences and master intellectual "code switching" on a broad scale. This is another skill that one learns mainly by getting things wrong, sometimes embarrassingly so. But as with drafting and re-drafting, the mistakes one makes in terms of audience can lead to precious knowledge about how to make work clear and accessible.

Be Lazy. How much research is enough? When should we decide that we have looked through all the right archived boxes, viewed and reviewed our videos, or modeled and re-modeled quantitative data? These are questions that are at the core of David's second principle, be lazy. Being lazy does *not* mean that we should be negligent or superficial in our research. Rather, I believe it means that we must recognize the point at which doing research actually can become an impediment to good scholarship. Put another way, research can become a drug whose potency is compounded by the belief that the more we take of it the better our work will be.

I know this problem first hand; I am a research addict. My most recent book is a study of the role that education played in the Americanization of European immigrants in the first half of the twentieth century. Transforming the vast, divergent multitude of European immigrants into "good and true" Americans was one of the most successful educational initiatives in the history of the United States. For my purposes, it was also one of the best documented. Indeed, Americanizers left a mountain of data describing the goals and instructional methods of Americanization efforts on the federal, state, and local levels. In addition, at a key point in my researching, I stumbled upon two large archived collections of immigrant newspapers in Chicago and Cleveland, which were translated into English in the 1930s and early 1940s.

These newspaper collections provided stunning glimpses into how immigrants from all the major ethnic groups (e.g., Czechs, Germans, Greeks, Hungarians, Italians, Jews, and Poles) responded to the Americanization movement. In short, the materials I found were an education historian's dream.

As it turned out, however, it was also a writer's nightmare. With so much data to retrieve, read, categorize, and analyze, even with the aid of several brilliant and diligent graduate student assistants, I never believed that I had enough. For example, the Chicago foreign language newspaper collection was only available for me on 67 reels of microfilm that were housed in Chicago (a five to six hour car drive from my home). Needless to say, it took months to go through this collection. Worse, because by this time I was a serious research addict, I eventually went through the microfilms twice to make sure I had all citations correct.

The most insidious part of this research addiction was that it gave me the perfect answer when people asked why my writing was going so slowly. "Give me a break," I would say, "I'm still doing the research." What I did not say was that doing the research was a lot more fun than trying to shape this sprawling amount of data into a clear, compelling, and meaningful story.

As in many drug addiction stories, this one ended with what is called in the US, "an intervention." In interventions people who care about the addicts join together to demand that addicts recognize the mess their addiction is making of their lives. In my case, the intervention was not formal but rather came in the form of concerned questions from a small group of friends and colleagues who did not buy my claim that there was still more research to do. They knew that I had more material than I could ever use and, more importantly, they reminded me that the best way to discover where I really needed more research was writing my way into it (see "Be Wrong").

Eventually, I kicked the research habit, painfully, and wrote the book.

Be Irrelevant. This principle is certain to make many educational researchers gasp. After all, who wants their work to be irrelevant? Who wants to be unable to answer the "So what" question? But, as with his earlier principles "Being Wrong" and "Being Lazy," David provides a thoughtful critique of the conventional educational wisdom, which places a great deal of importance to being relevant.

One of the main problems with seeking relevance, at least in the United States, is that American educational research frequently seems to be driven more by fads that promise relevancy than by the deliberate construction of a well-supported body of knowledge. For example, in just the last two decades, research designed to improve urban public schools in the US has devoted considerable attention to such topics as Afro-centric curricula, multicultural education, bilingual/bicultural education, establishing local school councils, devolving more power to school principles, encouraging mayoral control of school systems, promoting smaller high schools, creating charter schools, enacting voucher programs, and facilitating comprehensive school reform. All of these initiatives have risen and many have fallen in terms of their relevance to American urban education. While most of these topics still have promoters, many have lost the sense of relevance that they once had. Today research on charter schools is ascendant, but will it be a decade from now? Will researchers who now are spending great time and attention to charter schools, wake up one morning in the not too distant future and find that their work is no longer cutting edge or relevant? Probably. Educational research is a very fickle enterprise. As David cogently argues, "the pursuit of relevance leads, ironically, to irrelevance".

He suggests that we *could* be "more useful" if we focused "on developing general understandings of education" rather than concentrating "on developing solutions to current problems". This is a compelling argument. But unlike the previous two principles, this one needs some fleshing out. Many scholars in the sociology and history of education, for example, do examine educational issues from within the broader social and historical contexts that David extols, and many of them still seek relevance.

In other words, it is unclear about when seeking relevance is okay and when it is not. Tacitly, David poses the challenge of distinguishing "more useful" from relevant. In doing so he gets at fundamental aspects of the research endeavor – to think deeply about the problem one is investigating, to consider immediate and long term implications, and to frame and inquire into the problem in ways that get at issues that transcend fads.

Balancing Truth, Justice, and Beauty. This is the loveliest principle in David's sermon, a principle that calls on educational researchers to balance truth, justice, and beauty in their work. These values do not get a great deal of attention in the preparation of educational researchers nor are they, in my experience, common topics of conversations that scholars have about their work. Yet, seeking and enacting truth, justice, and beauty are probably why most people doing educational research choose this field in the first place. These values are, after all, what in David's words, makes the scholarly life "intrinsically worthy".

As I noted in the beginning of these comments, my main interest is how new education scholars can put David's principles into practice. In regards to "Truth," I want to expand on the two problem areas that David argues must be handled with care – politics and ideology. Both of these areas have great potential for corrupting our research and moving us away from rather than towards the truths we seek as educational researchers.[5] This is especially true when we deal with issues that have strong links to education policy. Unfortunately, in the last two decades, discussions of education policy in the United States have been increasingly divided along ideological and partisan lines. In many cases, you can predict the conclusions of articles on key policy issues just by looking at the names of the authors. This creates a toxic environment that threatens to undermine both good research and good policy.

Can we avoid such toxicity? Yes, but only if we are honest about our political and ideological biases, recognize how these biases can influence our research, be fair and honest in dealing with the work of people whose research and policy position we question and, be willing to listen to the data especially when they challenge strong and deeply held convictions. Most importantly, as social scientists we must be willing to change our minds when the evidence supports such changes.

Perhaps no one better exemplifies this willingness to follow the evidence than does the intellectual odyssey of American historian of education Diane Ravitch. For the first three decades of her career, she was seen as one of the most formidable advocates of such "conservative" reforms of public education as charter schools. But by about 2006, as the evidence grew that these reforms were not working, she transformed herself into one of the most formidable, "liberal" defenders of public schools (Ravitch 2010).

As for "Justice" there is little to add to what David has said. In my experience, many of the people in doctoral programs in education are there because they want to improve schooling especially for poor and minority children, in essence seeking to expand the reach of educational justice. Unfortunately, in the last two decades, many American educational reforms

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5 On these issues in regards to research in American history see Novick 1989.

noted above, reforms that have sought to make the country more just and democratic have not greatly improved the quality of education that poor children receive. In some cases, reforms made things worse.[6] Given that situation, David's reminder that social scientists must be sensitive to the irony of unintended consequences and willing to confront *all* educational research with "healthy skepticism" is both accurate and wise.

Introducing "Beauty" into discussions of educational research is at once the loveliest and the most difficult suggestion that David makes. As he explains, most educational researchers work in a world in which truth and justice often intersect and interact, but beauty rarely makes an appearance. Nevertheless, he argues that there *are* places where beauty clearly belongs in the world of educational research, most notably in how we write. Specifically, David maintains that, "Academic writing can be and should be a medium of personal expression and artistic creation".

Given all the other demands that educational researchers must respond to, is this a feasible principle? Can mentors really teach students to be better writers? Can students assimilate what mentors have to teach? I believe they can, but only if the mentors own writing exemplifies the same clarity, accessibility, and elegance that they want students to learn. We also have to provide time within the curriculum for students to regularly work on their writing.

Every other year, I teach a seminar devoted to research and writing in the history of American education. I begin the seminar by having my students read George Orwell's brilliant essay *Politics and the English Language*. David's sermon reminded me of why I like Orwell's work so much. Orwell, like Labaree, sees writing as a search for truth, justice, and beauty.

More than six decades after *Politics and the English Language* was first published, its ideas and warnings about the political misuses of language remain fresh and powerful. He offers readers something akin to a shot of intellectual espresso. In the essay, Orwell offers numerous examples of bad writing (some of which sound as if they could have been lifted from academic journals today). The most memorable of these examples is Orwell's translation of a famous verse in *Ecclesiastes* from "good English" into "modern English." The "good English" verse begins stating "I retuned and saw under the sun, that the race is not to the swift, nor the battle to the strong". In "modern English" the verse becomes "Objective consideration of contemporary phenomena compels the conclusion" (Orwell 1953, 163).

Orwell also suggests a set of "rules" that he believed could encourage better and more beautiful writing. For instance, he argues that writers should "never use a long word when a short one will do" and try to use "active" rather than "passive" voice (ibid, 170). These rules seem elementary, but it is amazing how many educational and social scientists ignore such advice and write impenetrable prose that is read by smaller and smaller audiences.

I started this response to David Labaree's sermon, by mentioning my nervousness about sermons as a genre. But the more I read it the more I found myself nodding in agreement, uttering frequent amens, and occasionally a few hallelujahs.

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6 On the weak educational outcomes of charter schools in the United States, see http://credo.stanford.edu/reports/ MULTIPLE_CHOICE_EXECUTIVE%20SUMMARY.pdf

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