

Institute of Education Sciences

Fifth Annual IES Research Conference

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11:30 a.m.-1:00 p.m. Plenary Luncheon Address

Dr. Charles M. Payne, University of Chicago

Introduction

(John Q. Easton) I'm really pleased to have this opportunity to introduce my long-time friend and colleague, Charles Payne. I first met Charles in the mid-nineties, when he was a faculty member at Northwestern University and an active member of the steering committee of the Consortium on Chicago School Research, where I worked until last year. At that time, Charles was very involved in what was called the "Comer School Reform Project," working very hard to form stronger ties between parents and their children's schools and teachers, while documenting and learning to understand better the struggles in these challenged communities. In Charles's book, "So Much Reform, So Little Change," he describes a concept he calls, "Program-itis." That's the tendency of schools to think that they can solve their problems by buying new programs. Too often, these are schools in highly impoverished neighborhoods with too much student turnover, too few strong leaders and teachers, all of whom are under tremendous pressure to turn around years of failure. But as we've learned from all of these depressing failures, you can't string together a bunch of disconnected programs and call it a "school improvement strategy." Some of you have probably heard me use this same concept in my talks about IES as I stress our need to better understand the processes and principles that undergirds strong schools. Throughout these years that I've known Charles, he's really pushed me in a way; sometimes maybe feeling a little uncomfortable; really to think about the role of race in school reform and in school research. You know, I had to ask myself, "am I helping things here by pointing out (yet again), how much schools differ in their quality depending on whether the school is integrated or overwhelmingly African American?" When our research demonstrates that teachers in predominately African American schools trust each other less, and report taking less collective responsibility for students than other schools; is this productive or destructive? Am I just adding fodder to stereotypes? Ignoring important cultural and historical antecedents that I don't understand, and blaming teachers for larger social problems? Well, with Charles, as subtle guidance, I came to believe that; yes this information could be useful for schools. But for it to be useful, I needed to accept some personal responsibility for being there to help people work through this evidence, and commit to supporting them in their own searches for solutions. Charles is the Frank P. Hixon Professor in the School of Social Service Administration at the University of Chicago. He is widely recognized as an expert in the field of urban education, school reform, social inequality,

social change and modern African American history. He has authored, co-authored or co-edited six books including his two most recent; "So Much Reform, So Little Change" and "Teach Freedom," the African American tradition of education for liberation. He's won numerous awards including the Senior Scholar Grant from the Spencer Foundation and the Alphonse Fletcher Senior Fellowship to study urban schools. Charles received his bachelors degree in African American studies from Syracuse University, and his doctorate in sociology from Northwestern University. Charles Payne.

Address

I want to thank John Easton for that very generous introduction.

In all the years I've known John, he has been the epitome of the engaged and committed scholar; crossing all kinds of social boundaries and always with his set of tables in hand. So he's a very appropriate person to be in the position that he's in right now. Last April, *Education Week* published an article that was almost a "teasing" kind of article, and it was teasing IES.

The theme of the article -- I think of this as kind of a "sub-genre" of educational journalism-- was that no matter what IES, home of the gold standard of educational research, no matter what they studied, their most common finding was, "no effects." The title: "No Effects Studies Raising Eyebrows," and the first sentence, (a wonderful sentence), "Like a steady drip from a leaky faucet, the experimental studies being released this school year by the federal Institute of Education Sciences, are mostly producing the same results. No effects, no effects, no effects." As is customary in this genre of journalism, they went on to point out that learning we were having no effects was costing us a great deal of real money. One study they cited cost 15 million dollars. Of the eight studies that they referred to which had been released by IES in the previous academic year, six produced results pointing to few or no significant positive effects on student achievement. Those studies included: Studies of school based mentoring programs in elementary school, commercial software for teaching mathematics, various routes of certification for teachers, teacher induction programs, interventions for boosting literacy instruction for disadvantaged preschoolers, and professional development initiatives in reading. That's a fairly broad spectrum across which to find no effects. For whatever reason, they did not mention "Reading First," which demonstrated to everyone who had wondered, that it's entirely possible to spend six billion dollars and not raise reading comprehension.

That kind of article has a certain kind of superficial plausibility, but it really represents a certain kind of "cherry picking." If you look at one set of facts, that's a plausible case. You can look at another set of facts and make a very different case to the effect that educational researchers have never had as much influence as they have right now. That's different from saying they have a lot of influence. It is to say, that it is better than it used to be.

There are certain things we've come to take for granted.

We now have a national consensus that schools matter for poor children,

that they do make a difference. As recently as 15 years ago, I don't think there was any such consensus among researchers.

I think as close as we're ever going to get to a national consensus is our belief that the quality of teachers and teaching matters, although we may still differ over how we're going to measure those things. We have had a national focus on "On Track" indicators, by that name or some others, which seems to be a part of a very promising movement toward increasing graduation rates in inner-city schools. We have a national passion for creating place-based research centers. The last I knew there were at least 20 places that already had consortium like entities up or in the process of planning them. You can make a case for the irrelevance of educational research by looking at one set of facts, and you can make a case for the increasing presence and importance of educational research by looking at another set of facts, to the degree that we are becoming more a part of the conversation.

Why is that true? I don't think it's that complicated. I think the overall quality of the work really is better today than it was 15 years ago.

The methodological bar has been raised. We use better samples. There's a lot more worrying about adequate grounds for causal influence.

There are people who give economists credit for much of this, this invasion of economists into the educational field. We used to be free of them.

I make it a point of principle not to give economists credit for anything because my experience has been that it only encourages them.

However one wishes to assess the two different ways of looking at the influence of educational research, the question is---how do we become more impactful going forward?

I'm going to make three or four points about that, some of which are just re-enforcing points which were made this morning. I honestly think the most important point which was stressed by everyone this morning is that it's crucial educators work and think in tandem with practitioners. I've seen several examples of that lately. Let's just cite one. If you talk to teachers that I'm in contact with in Chicago about what their primary concerns are, they certainly have an awful lot of concerns about instruction, about academic issues, about family issues, but one of their central concerns is just the behavior of the students that they teach. This is certainly after the fourth or fifth grade, although to my surprise this year we found out that kindergarten teachers in certain neighborhoods in Chicago feel that their classrooms are out of control.

That's the one level I would not have expected to find this. And yet if you look at the corpus of research on urban schools, behavioral issues are there, but they're not prominent. They're not a central concern. They're not something that gets the level of attention that governance issues would get, that teaching issues would get. Of course it took us a long time to get around to teaching.

I don't want to encourage us to back off of that at all, but I do want to say that if we think about the day to day issues from the viewpoint of the people in the schools, the issue of student behavior-- just getting them in their seats-- would at least get a more central focus

than the research community has given it thus far. A different kind of example:

I was in a meeting with a group of researchers from the Chicago Consortium a week before last. Someone mentioned that there was a study which said that you can predict 90 percent of the students in Chicago high schools who are going to drop out on the basis of their attendance patterns in second and fourth grade. Now among that group of consortium researchers, there are a number of folks who are in an on-going conversation about finding the elementary school equivalent of the "on-track" indicator in high school. What's the leading indicator other than fourth grade reading scores that will tell you who is on what kind of trajectory? None of them had heard about the research that was being referred to. None of them had thought about attendance as a leading indicator. They were trying to find something in the test scores, which is what we study. In that same meeting, there were a group of people from our community schools program. While all the researchers were talking about "gee, this is news, we need to think about this," they were saying, "You didn't know about that? Everybody knows that." "Here, we'll send you some stuff." After the meeting, they sent us some stuff. Which is to say, the issue of attendance, which has not been a central focus for researchers, is much closer to the agenda of a group of practitioners.

And to use a different kind of example: I've said many times that I think the most impressive and powerful body of research on urban schools in the last decade comes from the Consortium on Chicago School Research.

On the other hand, you can search that entire corpus of work and ask the question, "What do we know about the role of accountability in changing schools?" and not come out with an obvious answer. I'm not saying that none of that body of work touches on that question. But it's clearly not a central part of the focus of what I consider the most important body of work on urban schools over the last couple of decades. Yet if you talk to practitioners and certainly you talk to the people who have lived in the school districts which have changed the most in the last 10 years (witness Beverly Hall's talk at AERA a couple of months ago) for them accountability is one of the primary concerns...how to make it real, how to translate it into effect. And if you talk to school reformers on the ground, it's the same thing. Again, I'm not saying that accountability hasn't gotten a lot of attention elsewhere, but I'm saying that in one of our most important bodies of work, an issue which is so much a part of the day-to-day life of practitioners is not prominent. Again, all of that is to say that being in constant respectful dialogue with practitioners will change the questions that we give priority to, and some of the new questions might be more powerful than the ones we have been spending most of our time and resources on.

The second point I want to make is beware of the under-socialized conception of schools. By under-socialized conception of schools, I mean any way of thinking about schools that sees them primarily as collections of individuals rather than as webs of social networks. When the unit of intervention is the individual, I worry. Merit pay can be presented in various kinds of ways. We can contextualize it in various ways and explain it in various

ways. But some of the ways in which people think about and justify merit pay actually border on the insulting. There is a kind of underlying image of teachers as gerbils, and if you just put the right food pellet in the right place, the gerbil will go to where you want it to go to.

I think as part of a larger package of reforms, merit pay may have value. But I am saying that part of the problem I see with it is the assumption that the crucial issue is the motivation of individual teachers. I can tell you stories from now until...about un-motivated teachers reading papers in the class room, teachers who hide behind the contract. All of that stuff is there, but it doesn't tell you who they are. It tells you how they react in an environment that is unsupportive and under-resourced, and is dysfunctional at both organizational and at personal levels. In some ways, both the students and the teachers are reacting to that environment in their own ways. To reduce the problem to their motivation without considering the organizational context which is diminishing that motivation, is a simplistic way to think about the problem. My suspicion is that the greater power is not in the individual, but is in the social unit. The greater power is not in making the individual faculty member move, but making the faculty itself think collectively. That's reinforced for me by the fact that over these long decades, one of the consistent findings from the consortium has been the degree to which teachers take the collective sense of responsibility for children is a powerful predictor of which urban schools are going to perform well.

My third point is just to beware of interventionist research. Research which says that there's some discreet thing that we can measure, that can be introduced and it's going to make large differences in the outcomes for children. Eric Hanushek was pretty close to saying this earlier. The question of what works is just too simplistic a question. It has to be the question about what works in what context? So one of the things that means is that we need to spend a lot more time than we do thinking systematically and analytically about the broad daunting category of context. What do we mean by context?

The essential supports framework from the consortium is usually thought of as a framework for understanding how schools change. But you know, we can also think of it in a different way. Suppose you said that in the essential supports -- school leadership, parent/community ties, professional capacity of the faculty, school learning climate and instructional guidance -- suppose we argue this is a theory of context. That we have a good empirical base for believing that those things matter and that when we want to study particular kinds of interventions, such as this reading package and this way of structuring teacher leadership, we need to study the intervention in terms of the context. That means that no matter what particular intervention we're trying to study, we're trying to do it in a way which allows us to come away knowing something about the context as defined by those categories. There are other ways to think about what the categories, what the domains might be, but this is a set of ideas that we know have a certain set of empirical backing. If we were to do that (and I understand how much more difficult that

would make everyone's lives) it would mean that we would be able to tell much more complicated stories about what we found. Whether we found no effects or some effects. If I were to take time to make one other point about making the research more useful... On the one hand we have the "no effects" phenomena, on the other hand we have the "two points on a standardized tests" phenomena. That is, we do a really rigorous piece of research and we find that this particular intervention gives us test scores that are two percent better. I'm not entirely sure what that says. If you're going to say that we have reason to believe that this is going to be an on-going cumulative phenomena, and that two percent is going to be added every year, I might be convinced. I think we all know that's not the case for most of the things we study. We don't know. Even in some of our most rigorous research evaluations, what will be the long term effect in the actual situation, not in an imaginary situation, of the intervention that we're talking about? For me, validation should be connected to what we have the most reason to believe is really going to change kids lives. So I'm really comfortable saying that large increases in test scores puts you in another track. I get that. I'm really comfortable with the idea that if children graduate high school and if they have a broader set of post secondary options open to them, that validates an intervention. The idea that something that just gives us small increases in test scores has been "validated." I think we need to re-examine that. There's a discussion going on in Chicago about a group of small schools (I think most of them are charters) which have been quite successful in really tough neighborhoods. They have increased the high school graduation rate, the college going rate, and so far as one can tell from preliminary data, the college persistence rate. Yet those schools are under attack because the test scores are mediocre. So there's actually discussion in some circles, as to whether support should be pulled from those schools, because they're not successful. Meaning they're not moving test scores. That's sort of a bizarre way to frame what schools are about.

If I had time to make a fifth point, it would be that we need to honor the process of implementation as much as we honor that old question, what works? Finding out what works may not be as important as finding out how it's going to work in context-- the particular kinds of resources, the particular kind of politics that teachers and principals have to fight through to implement what we think works. We need to give principals much clearer ways to think about the process of implementation. I will not even (given the afore mentioned constraints of time,) try to make my sixth point. But if I had time, the point I would have made is that we need to think about for whom are we doing this work? Who are the most important audiences for the work that we do? What audiences do we need to reach that we're not reaching now? One of the most fun things that John and I did before he left, was to organize a series of seminars for parents and community leaders about what the research says. It was good for both sides. It was also a very powerful exchange because they come at this with a certain kind of passion and with an insistence that you make what you know relevant to their kids. They will push you on that in a way that we academics

are not necessarily accustomed to. Given the fact that they kept coming back, and in large numbers -- we had 80 or 100 parents at some of these things -- I think they were getting something out of it too. Now, can I be certain that they did anything with that knowledge once it was transferred? Not really. We had some cases in which parents went into meetings with board of education officials and made it very clear that they knew more about the research than the officials did. That was fun. Whether it actually does anything or not, we can be fairly certain that we have a better chance of that information having some impact if you put it in front of that audience, than if we simply allow our research to languish in journals and get exchanged in conferences. I really will stop with that, and I'll simply say that there's no easy way, there's no linear crossing between research policy and practice. But I do think that it is true, that the work we do is becoming an increasing part of a process that can help see that the most vulnerable children get the schools, and we hope thereafter, the lives that they deserve. To do that, maybe the most crucial point is that we have to stop talking so much, just to one another. Otherwise we do run the risk of going back to that *Education Week* article. We do run the risk of creating a rigorous science of what does not work. Let me conclude with that, and we'll take some questions.

Brief Q & A

[Jack Mostaf](Carnegie Mellon University) I want to respond to your eloquent call for studies of context.

It's hard to get statistically reliable results, even just of main effects.

It seems that to get statistically reliable answers to these deeper questions about context, is going to require more powerful research methods to squeeze something more than conventional statistics can. Can you speak to that issue of where more powerful methods are going to come from, to answer those very important questions about context?

(Charles Payne) I think that's one of the reasons I'm so hopeful about this... frankly what I think is almost a national mania for creating place-based research. We can do it in Chicago. I think we're going to be launching a project this Spring in Chicago, where we'll take certain ways of evaluating teachers and we're going to be able to have conversations down the road about whether those new ways of evaluating teachers made a difference (actually, the consortium has just published something that seems to be having any impact), and we'll be able to put that in the context of what we know about the organization and professional culture of those schools because we have the data base. It is daunting, but the fact that we have such a national mania for creating longitudinal data bases in schools, at least gives us the possibility that every time we go and do a study, we don't have to independently get the context variables as well. The problem though, and I don't know what the conversation is, but what I've always worried about is that I don't know how much intellectual coordination there will be across these various centers. I don't know the degree to which they're going to be using a common conceptual framework

and common indicators. But the possibility is there for us now. I think.

[Dave Judkins] (Westat) I think that finding out what works for whom and what circumstances might require infinite sample sizes. But it seems to me that the way to figure out what works in a particular context, is to let the people in that context do the research themselves. In other words, following the philosophy of W. Edwards Deming, statistical process control.

Teaching local principals and arranging teams of teachers to use statistical methods to do their own mini experiments and find out what works for them locally, using statistical process control. I wonder if you have thoughts on that?

(Charles Payne) For me, that's a "no brainer." That's one way to build human capital. We're trying to shape a project now at Chapin Hall.

Chapin Hall does most of the pre-school assessments in Chicago.

We're asking them to do a much more elaborate assessment in one of the neighborhoods we're working in, the Woodlawn neighborhood on the south side of Chicago. We're asking them to train community residents to be the interviewers. We'll precede that with some process that tries to give them lots of exposure to research. Especially by the research on early childhood care. Well beyond what they need to do the research.

We want them to have that kind of knowledge because we're hoping that long after the research is done, they become a group of parents and neighbors in the community who have a certain kind of sophistication about how to think about those issues. All I can say is "yes." The more of that, the better.

[Michael Orosco] (University of California, Riverside) I'm your every day classroom based researcher. I go in and look at teacher-student interactions. The one thing that we've lost in this decade is that we use the word "context." But to this day, I don't think that IES has funded one qualitative study. A lot of the context is in the qualitative. It's not in the quantitative.

Any thoughts about that?

(Charles Payne) I absolutely defer to my colleague, John Easton.

(John Q. Easton) I will say that IES has a long history which is continuing, of funding mixed method studies. IES has a long history of funding mixed method studies and I think we'll probably continue to do that.

(Charles Payne) And we certainly hope that part of that 60 million dollar budget increase that we heard about this morning will be directed specifically toward mixed method and qualitative work. At least I hope so. Are there any other questions? If not, thank you very much.