Lyle Spencer Research Awards
Program Statement

In the Spring of 2014, the Spencer Foundation introduced an ambitious new grant program, The Lyle Spencer Research Awards: Advancing Understanding of Education Practice and Its Improvement. This program is the successor to our long-standing field-initiated major grant program.

With this program, we aim to reinforce our commitment to intellectually ambitious research, oriented ultimately to improving the practice of education, and independent of any particular reform agendas or methodological strictures. This is not at base a change of direction for a foundation that has always aimed to foster creative and open-minded scholarship; it is however an emphatic assertion of our determination to search for and support the most challenging, original, and constructive scholarship and research we can find. We intend through this endeavor to press our colleagues in the research community to raise their level of intellectual ambition, to encourage work that is more thoughtful, more critical of prevailing assumptions, more self-critical about their own work and its limitations, and more relevant to the aim of building knowledge for improved educational practice.

We aim to give excellent work undertaken in this spirit more visibility and greater opportunity for support. We have noted that the more open-ended type of investigation we seek has been under something of a shadow in recent years, as foundations devoted to "strategic philanthropy" have increasingly used research instrumentally, to help pursue their pre-determined goals. This is certainly a legitimate way for a foundation to operate, but when it becomes widespread, it tends to curtail the range of questions and possibilities that get explored. We have also taken note that the popularity of doing research to learn "what works" has a tendency to squeeze out deeper questions of how, when, for whom, and why an intervention works. The current climate is often also not welcoming of work that explores systematically whether some set of predefined purposes or those outcomes measured in a "what works" experiment are the right ones to focus on. Questions of what and how to measure, and what and how to value the things we do measure, need more attention than they get.

It is vital to make clear that in seeking to support work that is less tied to particular policy agendas or particular research methods, we are not aiming to pursue knowledge for its own sake or taking the view that "anything goes." We believe that in fact the kind of searching inquiry that we aim to promote and support is not only quite demanding but also deeply relevant, indeed essential, to the "lasting improvement in education" that our founder Lyle Spencer challenged his foundation to promote. We want the scholars who seek our support to have convincing, well-reasoned answers to the question: How in the end does the work you are undertaking contribute to making the practice of education better?

In foregrounding the notion of "education practice" we risk misunderstanding. We do not intend that this program should seek only directly applied research about interactions between teachers and students, important as such research is. Successful educational practice depends upon an infrastructure that includes the conditions under which teachers practice, the curricular frameworks that guide their work, the provision made for their training and professional development, and so on. We envision a large-minded conception of educational practice that encompasses the institutional, policy, and normative frameworks within which instructional practice is enacted. Moreover, we recognize as well that instruction is not confined to the classroom but is an aspect of life in settings that can range from the workplace to the playing field, any of which may in the right circumstance provide the basis for rewarding study.
To clarify this point about connecting to practice – connecting the dots, as it were – we suggest that a significant share of the successful proposals we will fund under this initiative will fall into one of three broad categories.

- Studies that focus more or less directly on teaching and learning processes themselves, at the classroom level (or in instructional settings outside classrooms).

- Studies that pay attention to the larger policy and institutional environments within which educational transactions take place. Key to our interest here is attention to the conceptual and empirical links between elements of the infrastructure and the actual character of educational practice. Studies that aim at better understanding how different ways of defining, measuring, and rewarding teacher performance affect teachers' professional goals, time use, and understanding of success would fall into this category.

- Studies that help develop research tools that can support advancing the kinds of research we have identified here. The development of improved measurement tools, stronger theoretical frameworks and analytical methods, and the development of new databases and the archiving of data bases we have – none of these will make educational practice better today, but we regard advances on these fronts as essential to that lasting improvement in education that we seek.

These three categories might be labeled studies of instructional practice, of the educational infrastructure that supports or hinders effective practice, and of the research infrastructure that supports inquiry into educational practice. These categories are definitely not meant to be either exhaustive or mutually exclusive, but we think they do helpfully map the terrain. One point we emphasize particularly: scholars whose work is identified in one of the three general buckets we have roughly identified often come out of very different research traditions across which communication is often wanting. There are good reasons for specialization, but we want to stress that in our initiative we see these strands of work as deeply related. Failure to examine the connections or appreciate their importance is in fact one of the problems we aim to address through this initiative.

We are convinced that there are excellent opportunities for interesting, eye-opening, and rigorous work to be done by those who push at the boundaries of prevailing research and policy frameworks. As a way of pointing toward possible opportunities, we present here a few stylized examples of kinds of work that are of interest.

- Often reports on experiments have focused scholarly attention on the average effect of an intervention on all those who were treated (or at least who were selected for treatment), even though in large studies different experimental sites often display quite different outcomes. The variation in outcomes across experimental sites as well as across population subgroups may yield important information about how, why, and for whom interventions "work" – information that is too often submerged in average effects.

- The attention focused on seeking out successful interventions seems to have diverted attention from another type of causal research, which is to explain what caused certain things to happen. Why, for example, did the black-white test score gap fall significantly during the 1980s and then roughly stagnate for many years thereafter? Claude Steele’s celebrated work on stereotype threat is a fine example of seeking an explanation for an observed phenomenon: that particular groups of people (African Americans or women on math tests; white men in basketball) do worse than expected under test conditions. Steele’s insightful work has since led to interventions to combat the effects of stereotype threats, but it is important to note that these interventions were based on knowledge gained in explaining observed phenomena.
At a time when causal research is gaining in prestige and effectiveness in education scholarship, we may sometimes overlook valuable work on description, measurement, and conceptual clarification. Growth in understanding often depends on developing ways to measure or describe phenomena of interest clearly, reliably, and insightfully. The terms we use to label phenomena can often carry unacknowledged theoretical weight. Talk of “teacher” quality invites thinking about personal characteristics; “teaching” quality directs attention to the work of teaching. Labeling as “non-cognitive” those qualities that are not captured in tests of academic learning presupposes a quite narrow conception of cognition.

There are, finally, many challenging questions that can be found in the borderlands of empirical research and normative questions of value and purpose – the “why” of education, one might say, along with the how and where and for whom. Problems that range from the comparative merits of different ways of defining and measuring educational inequality to the limits of parents’ authority to determine the terms of their children’s education provide rich opportunity for fruitful investigation.

We hope these few illustrations convey something of the wide range of questions and approaches we are open to. But as open as we intend to be about problems and techniques, we do want to be clear that our consistent aim is to support work that has the clear potential to contribute to improved education practice, understanding practice and the conditions of practice in the large-minded way we described earlier.

Hallmarks of the research we seek to fund include conceptual and empirical attention to educational practice and a determination to approach research with well-reasoned and constructive skepticism toward the unexamined assumptions that shape current beliefs, actions, and research agendas.

**Submission and Review Process Overview**

We have designed the award process for grants under the Lyle Spencer Research Awards program with the aim of identifying work that offers a strong prospect of making a meaningful advance in the understanding of education practice and, through that improved understanding, ultimately making education better. We anticipate each review cycle of the program to be highly selective and we know that we will be comparing proposals that will take up a variety of problems using a range of research methods. It is our view that the selection among proposals should be driven in the first instance by the power of the ideas and by the potential influence of the proposed work. “Why is this worth doing?” is the first question. “Can you really do it?” is the next, and plainly also vitally important, question. These considerations shape several distinctive features of our proposal review process.

- Our selection process across very different proposals requires the exercise of judgment across a range of fields and methods. We think that balanced and well-informed judgment across these fields is best accomplished by a **review panel** whose members reflect a range of topical and methodological specialties, but whose strength lie fundamentally in their respect for a broad range of powerful research approaches, as well as in their judgment about what kinds of projects may lead to work that will make a lasting difference to education.

- In order to allow the panel to compare the full range of proposals for a given year, we operate a single annual application and award cycle. The next application cycle will begin in May 2015, with awards being announced in August 2016. In early 2015, detailed application guidelines and a timeline of the 2015-2016 review cycle will be posted.
Our proposal review process begins with a Letter of Intent (LOI). Applicants will be asked to provide a statement of roughly 1,500 to 2,000 words about their plans, along with an estimate of their budgetary needs and background information about the research team’s past work. Our main focus in reviewing the letter of intent is on determining whether we can find in it a powerful and practicable idea for work that has an excellent chance to advance our understanding of, and prospect for improvement of, education practice. This first step helps us assess whether a proposal is likely to be successful before asking applicants to produce the lengthier materials that will be required at later stages of the application process. LOIs will be used to identify which applicants will be invited to submit essays (described in the next bullet) about the driving ideas behind their proposed work. (More information on the Letter of Intent can be found here.)

Because our interest is fundamentally in the power and potential influence of the ideas that drive a proposed project, and only secondarily (but also critically) in the technical quality of the proposed research project, we will invite research teams, following an initial screening via the LOI, to prepare a well-reasoned essay of about 2,500 to 3,000 words describing the problem they propose to study and their proposed approach for addressing it. This essay should explain in a way that is accessible to those who are not specialists in its particular field of inquiry why the problem to be studied matters, how the proposed work will advance our understanding of it, why it is reasonable to expect that the work will be influential, and why the team undertaking the work is positioned to do the job well. Research teams that are assessed positively at this stage of the process will be invited to develop a fully-fleshed out proposal that will be subjected to peer review in the area of the team’s specialization. (More information on the Invited Essay can be found here.)

We recognize that work that may have a lasting impact on the understanding and improvement of education practice comes in all sizes. Some work we would value greatly may be “cheap” in terms of dollars – as, for example, may be the case in areas like philosophy of education or psychometric theory – and we do not value it less for that reason. But other powerful work is intrinsically more expensive, and in recognition of that reality, we are prepared to fund proposals of up to $1 million, roughly double what we previously funded in our major grant program.