PARTING THOUGHTS

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Paul LeMahieu, senior vice president at the Carnegie Foundation for the Advancement of Teaching, once joked to me, “Foundations don’t have enough natural predators.” He’s right. Like most foundation presidents, I spent my career on the other side of my current desk, where many people were willing to tell me when my bad ideas were bad. But, when I joined the Foundation in 2000 as senior vice president for program, I seemed to get a bit smarter.

I am going to retire in September 2013, having had the privilege of serving as the William T. Grant Foundation’s president since 2003. The quality of my ideas will quickly revert to form, so it seems sensible to get in front of the curve. Here, I examine the merits of several ideas that marked my tenure. Some were good, but others could have benefitted from foundations being lower in the food chain.

As I prepare to step down, I am pleased with what we have been able to accomplish. At the same time, I have embraced, along with our Board and staff, the drive to get better every day. I hope this essay is useful to my colleagues and to others who share that goal.

A FOCUS ON SETTINGS
One of the first changes the Foundation made after I became president was to shift the focus of our research program from the strengths and abilities of young people to the settings that—in theory—produce those strengths and abilities. I came to the Foundation following 11 years as a senior vice president at MDRC, a social policy research firm. There, I developed a great appreciation for the importance of consistent, positive findings as a lever for change. The prime example was MDRC’s studies on welfare reforms in the 1980s, which consistently showed that relatively low-cost changes in practice, such as job clubs or job search assistance, could help low-income mothers move from public assistance to employment. When you find such robust and repeated success, especially for low-cost interventions, few care why they work. It is, however, very unusual to identify approaches that produce consistent improvements across a broad range of situations. As a case in point, the MDRC work I led on education and youth interventions produced fewer winning strategies—in most cases, attempts at reform did no better than current services in the community.

At the Foundation, our thought was that interventions focused on individuals might be too weak or transitory to withstand the other influences in their lives. Hence, we shifted to settings. While our primary motivation was a desire to produce better results, we were also being strategic. This shift was designed to leverage federal funding for research and evaluation on youth policies and programs. Much of that funding remains focused on individual outcomes (e.g., student achievement, at-risk behaviors) and on the individual differences in skills, attitudes, and biology that influence those outcomes. Our thinking was that while improved individual outcomes were (and remain) our goal, an important path to those outcomes is through the social settings in which young people spend their days—classrooms, households, youth programs, and neighborhoods. If the federal evaluations showed mixed results, we wanted to create a body of work to help explain why so that we all could do better.

While I remain convinced that it is intellectually and practically powerful to focus on these daily
environments, I and my colleagues have found it surprisingly hard to communicate our interest in settings clearly. At first, I saw this as a routine product of change. When a funder shifts its focus, applicants push the boundaries to see if there is a match between their interests and what the funder will support. That is a normal aspect of the philanthropy “market,” and we anticipated our shift in priorities would cause some confusion. But, that confusion endured long enough to warrant a closer examination. Part of the cause is that the focus on settings is atypical in certain disciplines. Many applied developmental scientists are experts at theorizing and studying individual-level phenomena: how motivation or self-concept affects subsequent performance, how individuals respond to differential incentives, how intervention programs affect individual outcomes. They are, however, much less comfortable thinking about how organizations and systems help shape those outcomes. In addition, measurement and quantitative analysis techniques are much further advanced at the level of individuals than at the level of settings or groups of individuals. Part of the problem may be how we view the world. Clinical psychologists have learned that one of the “fundamental attribution errors” humans make is to assume that the behavior of other people is largely determined by their individual tastes and capacities, even though we believe that our own behavior is affected by our current circumstances.

Perhaps for all these reasons, many of the national surveys and data sets that researchers use contain considerable data about individuals but very little information about their settings beyond basic demographics. The National Longitudinal Survey of Youth and the Early Childhood Longitudinal Survey generate good longitudinal data about children’s risk behaviors, achievement, and attitudes, but almost nothing about what occurred in their classrooms, youth programs, homes, and neighborhoods.

Given our interests, it is hard for applicants to propose projects meant to explore setting influences without collecting new data. Primary data collection is expensive and takes more time than some career trajectories allow. Because of this, I have worried that our priorities preclude some promising young researchers from applying. Fortunately, the availability of data relevant to our interests is slowly improving. Newer data sets such as the National Longitudinal Study of Adolescent Health (Add Health) gather some setting-level information, such as student social networks in schools.

Better information on settings would also assist efforts to synthesize evaluations of interventions in order to understand what works, such as the Campbell Collaborative, the What Works Clearinghouse, and meta-analytic reviews by academics. These reviews do a good job synthesizing individual-level effects but a weak job explaining variation in those effects across studies. We think that one reason this is true is because the evaluations they summarize do not report on setting-level features, and thus it is not possible to see if such features help explain why some programs or strategies work (or work some of the time) while others do not. For example, it is common to find that a
change in policy or practice is effective in some situations but not others. And, it is easy to imagine that the differences might be due to the characteristics of the community or organization where the change was tested. Yet, such information is rarely collected or included in research and evaluation reports.

All this has led to difficulties for our staff and applicants—too much applicant effort developing and proposing projects that do not have a setting-level focus and lots of staff time reviewing such letters of inquiry and trying to be clear about why we reject them. The problem is likely to remain until the interest in settings is more common among researchers and funders. Interestingly, practitioners understand our interest in settings immediately. They see their practices influenced by their situations and understand that these practices are the critical link to youth.

INTERVENTIONS AS A WAY TO ADVANCE THEORY

Having been introduced to large-scale intervention studies at MDRC, I ascribe to the Kurt Lewin idea that “if you want to truly understand something, try to change it.” This approach sees studies less as evaluations of particular interventions and more as a tool to advance an understanding of how the world works. Because we wanted to better understand how settings influence young people, we supported interventions meant to change setting-level features and examined the effects on those features and what happened to youth outcomes. For example, if the theory is that classrooms affect youth in large part through the interactions between teachers and students, interventions should try to change those interactions in ways the theory suggests. If the interactions and youth outcomes change in sequence, that is support for the theory and increases the chance that you have identified a critical mechanism. If the interactions change with no accompanying change in youth outcomes, then something is theoretically amiss. And, if the interactions do not change, you need a different intervention. This approach exploits positive and negative findings to better understand why things work as a way to improve youth development and well-being.

Because we have this orientation, we no longer fund evaluations of interventions or programs, unless the study is clear about the setting-level theory being tested. In my early tenure, we supported conventional program evaluations and our shift away from them was confusing to many. It was also off-putting to those who argued solely for identifying what works. We see a change occurring as policymakers and evaluators try to make sense of the cascade of null findings that come from impact evaluations of promising programs and policies. For example, recent conferences have had themes such as “why do intervention effects vary?” and “how to explain contradictory findings.” Such questions demand better theory about the settings involved.
TOOLS AND METHODOLOGICAL IMPROVEMENTS AS LEVERS FOR CHANGE

Although we thought there were good reasons to study settings, the state of the science at the time made it impractical. As one concrete indicator, when we issued an RFP in 2003 for interventions to improve classrooms and youth programs, we only funded 5 projects from 350 letters of inquiry. The proposals suffered from three main problems: (1) researchers did not have widely available empirical information and tools to estimate how many settings they needed to include in any individual study to produce reliable results, (2) measures of setting-level phenomena such as adult-youth interactions were not well-developed, and (3) the conventional analytical methods and related software for determining if a change at the setting level was causing a change at the individual level were inadequate. Statisticians and psychometricians had been working on these problems with some success, but their methods, and more importantly, widely available software and measurement tools, did not exist. This was a classic chicken-or-egg problem. It was difficult to develop such tools without ongoing studies to empirically test new approaches that would be codified in tools, and without the tools, it was hard to figure out how to design and conduct such studies. This led to what may be one of my best ideas, nine years of support for a collaboration between Steve Raudenbush, Howard Bloom, and their colleagues.

Since 2003, Steve and Howard have worked with our grantees and others to address knotty methodological problems and generate empirical information that informed and tested their solutions. They incorporated this information into software for planning setting-level experiments, created procedures for improving and estimating the reliability and validity of setting-level measures (including tools for classroom observation and the observation of activities in youth programs), and developed new methods for causally linking setting- and individual-level effects. The analytical methods and empirical guidance for planning group-level experiments are built into the new Optimal Design Plus software, available for free on our website. Improving setting-level measurement and estimating the causal relationship between various elements in a theory remain works in progress. What is clear is that incorporating improved methods in accessible and easily used tools is a productive way to change research and practice. For example, until the Optimal Design software was widely available, analysts and funders routinely underpowered setting-level studies. Now, the new methods are becoming standard fare in graduate courses, the tool is referenced in federal research procurements, and more studies are designed appropriately. In a similar vein, until analytical software easily handles new approaches to causal analysis, scholars will continue to use outmoded approaches that they can execute with existing software. And until reliable, valid, and (most importantly) cost-effective measures of setting-level processes exist, few scholars will use such measures in studies.

The impact of setting-level measures on practice has been equally dramatic and instructive. In January 2008, we issued an RFP with the Spencer Foundation to develop more reliable, valid, and cost-effective ways to measure classroom practices and funded seven teams. We organized two meetings a year for these grantees with Steve and Howard serving as consultants. One result was methodological work that clarified that setting-level tools are not as reliable as they need to be to inform high-stakes decisions about the work of individual youth workers or teachers. In addition, the cost can prohibit wide adoption in research or practice. These issues are playing out on a broad stage as states and school districts work to revamp their teacher evaluation systems.

While there are considerable and justifiable controversies around how to identify, reward, and retain
individual staff, there is much less controversy about using measures to try to improve practice in a lower-stakes policy environment. For example, in addition to the work on measures of classroom practice, we focused on improving measures of the quality of program practices in after-school and other youth programs. I wrote about this in my Annual Report essay last year, noting that one of the measures we helped develop was being used in approximately 60 systems encompassing 3,000 program sites. As an indication of practitioner hunger for such tools, those numbers have grown in one year to 74 systems and 3,250 sites.

RESEARCH TO PRACTICE
In one of my first Annual Report essays, I suggested a logic model for our work that had researchers producing findings that then flowed to practice. When our Board and others asked me to tell them about examples from our work, I realized that few existed. As a Foundation whose tagline is, “research to improve the lives of young people,” our track record demanded better. It has not been easy to make progress; the world is complex, with many things affecting young people. But, I now believe part of the problem is how we were thinking about the role of research.

“Research to practice” is a bad idea, even though it fits the prevailing model that many hold about how to achieve evidence-based policy/practice. Research to practice presumes that researchers are the experts (they are in some ways, just not in all ways), and that they will find solutions that can be exported to practice. When that does not happen, the model implies that the solution is a better “translation” of findings or stronger mandates to adopt what researchers learn. Those solutions, however, don’t fit the data. Over the last 20 years, the research community has made great improvements in its methods and modes of translation, and it has not made enough of a difference for youth. My sense is that we have many examples of great schools, classrooms, and youth programs that look like what research suggests, but we still do not know how to create them at scale.

“Research and practice” is a better idea, and I still see a stronger connection between research and practice as critical to improving youth performance and well-being. For that connection to lead to improved practices—by teachers, youth workers, social workers, police—the model needs to be less linear. At minimum, the relationships between practitioners and researchers should be reciprocal, with researchers and practitioners working together over time to improve understanding of persistent problems and test promising solutions.

During my tenure at the Foundation, we have tried many strategies for creating productive relationships between researchers and practitioners. These include mentored fellowships for researchers and practitioners to embed themselves in cross-role work; support for ongoing “learning communities,” in which researchers and practitioners learn from each other; funding for research studies to better understand how practitioners acquire, interpret, and use research evidence; and advocacy for a greater focus on issues important to practitioners. We are seeing progress in these areas, some of which is discussed in the essay by Vivian Tseng in this Annual Report. I have taken to saying “practice to research” to catch people’s attention and make a point. Whether or not this greater emphasis on reciprocity and practitioners will result in improved outcomes for young people remains to be seen.
COMMUNICATION THROUGH NETWORKS

In the past 10 years, we have changed how we think about communication. Consistent with the evolution in thinking about research and practice, I came to the Foundation understanding that communication implies a two-way street. But, it was not clear how to take that instinct and turn it into a strategy.

Every day, the surface mail brings journals, reports, policy briefs, and various offers. The volume is stunning. My assistant culls it, forwards about one-fifth to my attention, and I discard all but those few articles that catch my eye. These get read on the commuter train, hopefully.

Like most people, I want to stay up-to-date on issues and events that are important to me, and I rely on a rather small network of listservs, blogs, and personal contacts to do so. All the rest is a blur. But, in the fields in which we work, I feel reasonably in touch. My behavior is not unusual, and we have tried to think through what it implies for our communication strategies. We decided that the key is to identify the people and organizations that are the information brokers in various areas, and stay in touch with them. It is not difficult, thanks to the explosion of communication channels.

When I joined the Foundation, we were often approached by scholars asking for media training. Most of the requests were driven by an interest in getting coverage for one’s work in the general press. As much as anyone, we like to get mentioned above-the-fold in a major daily. Yet, we have adopted a different communications strategy that is closer to how each of us uses our networks. When we emphasize an area (e.g., after-school programs, measurement of teaching, methodological improvements, studying the use of research), we create a list of the highly networked advocates, intermediary organizations, practitioners, and scholars working in that area. We then listen to these people and target them using various communication channels (e.g., emails, e-newsletters, etc.) and our limited face time.

Our experiences underscore the importance of personal relationships and face-to-face contact. For example, from 2003–2011, we aligned our advocacy, communications, and program development funding around the topic of improving the quality of after-school programs. I devoted considerable time to writing and speaking about that issue, but I also frequently traveled and met with key people. Pam Stevens’s recent review of our work in this area showed that such “influentials” thought our focus on after-school was useful, and I was persuaded to write a capstone essay about our work, which originally appeared in last year’s Annual Report. Subsequently, we sent my essay and Pam’s report in a special email to 2,175 people, including 272 influentials. Only 25 percent of the total group opened the newsletter including 64 of the influentials. Some smaller number clicked on the essay, let alone read it. I hoped for more but, on reflection, I had to think about little beyond my own habits. When I need to know something, I reach out to someone I trust.

In addition to sending the essay, I presented its storyline at a dinner run by Grantmakers for
Education for its subgroup of funders working on out-of-school time. Since the mailing and dinner, I have heard from many people that they found my essay useful. To a person, they had not read it until a colleague—many of whom were at the dinner—mentioned it to them.

Our web analytics and anecdotes from experience point in the same direction. Have something to say and say it in writing to make it portable. But, also say it in person to influential people in networks, or better yet, the brokers who connect networks.

**SHIFTING STAFF TIME TO MAKE A DIFFERENCE**

Every organization has to decide where to deploy its resources, and we made a dramatic shift in our allocation of staff time during my tenure. When I came to the Foundation, I was committed to doing more than picking winners. There are successful individuals and organizations that will do a great job with any foundation’s support. Funding such applicants is attractive and very low risk, since the foundation can always claim—rightly or not—some role in their success. But, if a foundation’s goal is to make a difference, it has to identify applicants who will not do as well without its support, and provide what is necessary to advance their work.

At the outset, I saw this as looking for and funding the diamonds in the rough. I wanted to find promising applicants who were outside the networks and circumstances that support success. I and other members of the Foundation’s Senior Program Team spent a fair amount of time “prospecting” for such people, working with those we found to help develop viable proposals. We attended many conferences and visited universities explaining our interests and held exploratory meetings with potential applicants. And, we funded some projects that would not have gotten through our review process without the up-front support. All this effort came at some cost, though.

Most importantly, we paid very little attention to people after we gave them grant funds. We also had a hard time pointing to notable successes—instances in which we changed a grantee’s trajectory. We were funding some very promising researchers, but we were doing little to change their support system in a meaningful way. Money was not enough.

Greg Duncan—a friend and former collaborator, grantee, and member of our William T. Grant Scholars Selection Committee—observed during the annual meeting of current Scholars that the most important part of the process was the informal time we built into the meeting, during which Scholars interacted with each other. It was in these moments that people were admitting to each other what they did not know and getting some help, safe from the scrutiny of senior scholars. Greg was right—but where to get the resources and staff time to do more work with people after the grant award?
While the shift was not immediate, and we learned a lot about how to do it over time, for the last 10 years, we have continuously moved staff time and other resources from pre-award to post-award activities. I and other senior staff each have a portfolio of grants related to our expertise. We are committed to reading and commenting on grantee reports within 30 days (and nag each other when that standard is not met). We also seek out grantees (often in lieu of potential applicants) when we go to meetings or travel. As a result, we see links among grantees, and do what we can to foster collaborative work. A prototype for this shift is the many changes we made in the purpose and structure of the annual meeting for our Scholars Program grantees. That meeting is now run entirely to build the already impressive capacities of that group: lots of workshops, fewer outside experts, more discussion of works-in-progress. We also hold about 10 similar meetings a year with other groups.

Creating useful “learning communities” has been difficult because most grantees are convinced, at least initially, that funders want to hear that all is well and that funder-sponsored meetings are an exercise in “pitch.”

We make a joke out of it, telling grantees that the surest way to continue our support is to divulge everything that is not going well—or that they do not understand—and then commit to working on those issues.

While I think of this shift as a signature of my time as president, it has had some costs. Unquestionably, we fail to recognize and support some of the riskier projects we used to develop, and applicants get very little in-person attention prior to being funded. In addition, staff are taxed by the logistics of a small organization holding so many meetings. (We do receive invaluable help through a grant to the Forum for Youth Investment.) As we made this shift, I had some concerns. One was that grantees would attend because they felt they had to. While I am sure this is sometimes true, we have now run this type of learning community for people funded by others, and they come back. In addition, an external review of our work in after-school found that these meetings were very valuable. The other concern is how to sustain support and engagement for participants between meetings. Some certainly occurs, but when it does, it may be a case of the rich getting richer. (Teams with relatively greater resources and capacity reach out for more.) Leveraging and sustaining the work that goes on in these annual or semiannual meetings is a problem we haven’t yet solved.
IN CLOSING

It has been a great privilege to work at the Foundation for 13 years, with the responsibility of using its resources wisely. Time will tell if that has been the case. I am glad I took the job late in my career, because it was easier to recall how hard it is to raise support for good work and to see applicants and grantees as our clients rather than the instruments of our vision. Of course we tried to be instrumental and support work that, as a whole, helped to improve the lives of young people. But, as we did that work I often had Edward Meade, Jr. in my ear. For 30 years, Ed was a senior program officer at the Ford Foundation, leading its work on education. Before his death in 1994, he took me under his wing. During my tenure as vice president and dean at Bank Street College of Education in the 1980s, I sought grants from Ford. I was awed by Ed’s experience and very appreciative that he would meet with me. He told me to never forget that he had an easy job (and frankly, so did I). The hard work was being done by people in the organizations and communities trying to make changes for the better.

Among his many accomplishments, Ed commissioned Paul Nachtigal and colleagues to do a review of Ford’s work in education from 1960–1970, and the team produced the classic “A Foundation Goes to School” in 1972. Paul worried in the report’s introduction: “This document may appear to be overly critical, especially to those project directors and foundation personnel who had investments of time and professional status in the projects.” None of that concerned Ed. He told me that the report’s main conclusions guided his work for the next 20 years. As Ed taught me, too often foundations underestimate the complexity of change and overestimate the role that they and other outsiders can play. Our responsibility is to continue enabling the efforts of individuals, groups, and institutions doing the heavy lifting on enduring problems. Their success is our success. Our role is to help.

Robert C. Granger, Ed.D., President

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