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Re-Engineering Philanthropy: Field Notes from the Trenches

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Not long ago, in an interview with a philanthropic trade publication, I let myself get carried away for a moment and told the interviewer that I had come into philanthropy "with a chip on my shoulder." It was probably not the most felicitous choice of phrase. For some time after, I could sense a lot of my colleagues — some of whom cast me among the curmudgeons of the field — glancing at me sideways and muttering to themselves, "Well, I guess that explains it."

In reality, I don't know that I measure all that high on the curmudgeon scale. And I would never have come into philanthropy had I not formed a real respect for the best in philanthropic practice, and for the people who take the risks to make those practices effective. So I hope the image of Bailin with a sort of giant Pringle on his shoulder would have struck at least a few of my friends as a mistake. And in any case, that was hardly the image I had intended to convey in the interview.

The reason for the chip remark is that I took the presidency of the Edna McConnell Clark Foundation after 17 years at an operating nonprofit institution Public/Private Ventures, which I helped found. P/PV was and remains an ambitious grant-seeker, so my years there put me in the begging line at a good cross-section of American foundations, as many as 60 to 75 a year. That is not necessarily the best vantage point from which to develop a rosy idea of modern philanthropy.

Standing in that queue, I often saw the dark side of philanthropy that many previous speakers in this series have described: "Autocratic, ineffective, and willful." "Elitist." "Cloistered." "Arrogant." "Pampered." These are all words used by previous speakers here, so I am happily spared the need to repeat them for this audience today. But in truth, in the years when I was passing my hat around the foundation world, I did see all these qualities now and again, and I understood — and still understand — what the critics are referring to.

Waldemar Nielsen, in whose honor I'm privileged to speak here, is remembered as the founder of that critical tradition, the first to speak truth to money. Although he no doubt had predecessors, perhaps none had yet written so fearlessly and with so wide a scope as Wally did about the insularity and neglected potential of American foundations.

Some of the speakers in this series, and the quotes to which I've just referred, build on Wally's legacy of blunt appraisal, and of prodding foundations to live up to their promise. Still, I would like to use my time today to argue that the modern foundation remains the "strange and wonderful social invention" that Wally Nielsen described, and that it can, when prodded, be the versatile, adaptive engine of reform that that he so admired, even in the midst of his sharpest critiques. More specifically, I'd like to use my own experience, both as grant-seeker and now as grant-maker, to give some examples of kind of self-criticism and reinvention that philanthropy is capable of, even amid all its foibles and shortcomings.

Some people have said that we are in a period of especially profound re-thinking about the purpose and potential of foundations, about what grants are for and why grantmaking institutions are valuable. Lester Salamon has even argued that the re-engineering of the nonprofit sector in recent years parallels a similar overhaul that American businesses put themselves through in the 1980s (1). While the cause and effects for philanthropy will differ much from the story of U.S. industry, the sense of upheaval could be just as great, and if so, the results might be just as healthful in the long run. I also believe that the current soul-searching, while not brand new, may be more intense at this moment than at any time since the late 1960s and early '70s. Those were the years in which first Congress and then philanthropy's own leaders, ably summoned by John Filer, jolted us into thinking afresh about why we exist and what good we do. Some of the answers we came up with in those years were drawn from what was at that point still recent experience — the fresh memory of the Great Society and later of the different, but still ambitious, domestic policies of the Nixon Administration. It was an era of activist government programs largely formulated in foundations, or in foundation-sponsored research programs and think-tanks.

But that era, and the peculiar social compact that underlay it, are long gone. And with them, I think, have gone some of the ideas of society and philanthropy that were born in that last great exercise of self-scrutiny. It's time for a new re-examination. And in my view, some of the harsh words spoken in this lecture series indicate how bluntly, how forcefully, and how honestly people in this sector are already taking on that challenge, and how far it's likely to lead us.

Re-examining the Foundation's Direction

But when you look at the other items on that list, especially with the benefit of six years' hindsight, you can see what made the trustees believe the time had come for a change. The problem, in a nutshell, was this: In all of those programs — in education and justice and child welfare and neighborhood improvement — we were trying to reform huge, complex, entrenched, multi-billion-dollar public systems with a staff of 25 people and around $25 million a year in grants.

Yes, the staff was terrifically talented and distinguished in their fields — something that I came to admire and value more and more, even as we completed our involvement in programs that they had created. And no, they were not making decisions on their own, but were guided by panels of first-rate advisers. And yes, other foundations also supported many of our grantees, even if not in the same activities or in a coordinated way.

But even so, we were proceeding as if we had some independent leverage over social systems that had been many decades in the making — systems that were fortified by all the ramparts of bureaucracy and regulation, and thickets of intergovernmental agreements and contracts, and moats of public dollars. We were fighting battles that had tested the power and wealth of serial U.S. Congresses and presidencies. It was a battle of Homeric proportions fought with Lilliputian resources. How could we ever imagine that we could accomplish anything so significant in our lifetimes? And how would we even know if we did?

My goal, as things evolved over the first few years, was to wind down these efforts responsibly, and in their place assemble a program that recognizes a humbling but undeniable fact: Foundations do not, in the main, make change in this society. Grantees do. That was not so clearly understood in the 1960s, but it is now — even if many who espouse that view don't act like they believe it. Today, foundations make change not primarily through the force of their own ingenuity, but on the strength of their grantees' achievements. Foundations succeed when their grantees grow stronger, achieve more, and gain stature for leadership.

And what was our foundation doing to make any of those things happen? The sad answer was *not much*. And we were far from alone.

We were, instead, trying to inject new ideas — quite often, ideas of our contrivance — into the work of grantees whose own ideas, and whose actual mission, may have been quite different from what we had in mind. We were constantly wheedling and negotiating with them, tugging here and prodding there, compromising a little on this issue, strong-arming them hard on that one. Quite often, as I learned in my years as a grant-seeker, the result was a bad fit for both the foundation and the grantee. Much of the time, the end result was not much more than the reluctant implementation of a compromised idea. Both the grantee and the foundation were essentially acquiescing in a marriage of convenience, but rarely of honesty or trust. Still, it brought some money in the grantee's door. And it produced a program that the Foundation could cite with pride (if not necessarily precision) in its annual report. So everyone was reasonably happy. And the great social systems rolled on, pretty much unaltered.

A New Way of Grantmaking

Specifically, we imagined concentrating on a single field — we picked youth development. We imagined finding organizations within that field that are delivering effective services, that want to do more, and to do it better, and to reach more people. We would not fund projects or pieces of projects; rather we would help those good organizations expand, manage themselves more effectively, track their results faster and more accurately, and in general achieve more, for more people, with more information about the outcomes. We'd be accountable not for some long-term change in a giant, amorphous system — but for how useful we were in the growth and performance of a given set of organizations, for a given population of young people.

The first reaction some people had to that idea was: *Sounds like general operating support*. Grantees will love it, people told us, but it's very expensive and risky. A good general-support grant needs to be big and unconstrained, and when you make a big bet with no strings attached, there's a decent chance you'll come up with nothing to show for it. That wasn't the approach we had in mind, but that's how the idea sounded to some people at first. (I happen to think, by the way, that general operating support is greatly undervalued at a lot of foundations. There needs to be more of it, and I applaud foundations who give their grantees more leeway. Our approach is slightly different — but it does provide a lot of leeway, and it intentionally shoulders the corresponding risks.)

The next reaction, even more negative, was: *Sounds like venture philanthropy*. The management professors and theoreticians will love it, people told us, but it's at best an interesting metaphor, not a plan of action. Follow it too far, and you'll find yourselves sitting on grantees' boards, meddling in their management, and interfering with the very things you claim to like about them: their independence, their flexibility, and their ingenuity. Plus, you'll premise the whole thing on an "exit" plan that may well never materialize. Venture philanthropy wasn't what we had in mind either, but I understand why it sounded like that to some people.

The course we actually chose is something we kind of haltingly call Institution-Building — though I'd welcome nominations for a better name. It borrows some of the best elements of the two other models, but it's not the same as either one. We felt that this approach would work well with youth development because the field — if you can call it that — has, as yet, no giant national system built around it. It has a wide but shallow network of nonprofit organizations — most of them small, many of them young and fragile — experimenting with dozens, maybe hundreds, of different ideas and approaches. It has a mission of unquestioned importance: the healthy development of young people, particularly those in low-income communities. And it has a few nonprofit institutions that seem to be accomplishing things of importance — too few that you can conclusively prove as successful, and certainly nothing that's reaching the majority of people who could benefit from it. But things that look like they have potential, that would benefit from more ambitious evaluation, and that could be reaching many more people than they now do.

Focusing on Four Outcomes

- *Educational attainment or achievement*. Young people in the program would more often advance to the next grade, attend school regularly, and show measurable improvement in learning.
- *Preparation for work or self-sufficiency*. Participants would develop demonstrable skills; they would find and hold down jobs, earn income, prepare to become self-reliant and assume the other the responsibilities of adulthood.
- *Civic engagement*. More young people would be participating in and contributing meaningfully to the life and development of their communities.

- *Avoiding harmful behavior*. This could include things like staying off drugs to preventing too early pregnancy, but we expect that it would be coupled with positive goals as well, at least one of the other three that I've just mentioned.

We then chose to work in New York City and Boston, geographic areas close enough to our offices so that we could pay close, competent attention to the work we were supporting. We looked for organizations at the early to middle stage of growth— with revenues somewhere between $1 million and $10 million—so that our investments would have real meaning in the context of the grantee’s total budget.

We then began our new work with a small allocation in what we called a “Growth Fund,” where we could try out our ideas and learn quickly from mistakes and successes. We worked with a few current and former grantees that were already committed to youth development, and sought out a small number of others that shared these basic criteria:

- A demonstrated, or at least apparently effective service or product — something they produce consistently that has real value.
- Strong leadership and management.
- A financially sound and operationally viable organization.
- An ability to track its own successes and use data to make decisions.
- A general compatibility with our new style and way of doing business.

We had no illusions that we had, in-house, the expertise we would need to weigh these criteria accurately, much less to work effectively with organizations that fit the profile. Like most foundations, we had a staff with much more substantive expertise than organizational or management experience. That, too, was partly a carry-over from the old days of idea-driven philanthropy. In the old model, it wasn't important to know how to run big organizations, manage finances, create implementation teams, and the like. Foundations thought *Great Thoughts*, and other people worried about organizational charts. We were determined that that was going to change, and that eventually we would have a staff that blended substantive expertise with a solid grounding in the management disciplines. Today, we’re much closer to that goal, though time will tell whether we yet have the balance right.

But even with a new staff and, just as importantly, a new internal culture, we would never have all the expertise in our own offices to provide everything that grantees would need and want from us. So we enlisted the help of the Bridgespan Group — at that point a relatively new management consultancy set up specifically to advise nonprofit clients. Bridgespan gave us valuable assistance in designing and testing our model, formulating and revising our strategy, and undertaking the due diligence by which we applied our selection criteria and chose grantees. And later they became even more helpful in working directly with the grantees we ultimately selected.

The business of selection surprised us—first by its complexity and later by its overwhelming importance. The due-diligence approach we used was a real departure from the Request for Proposals model we had followed for most of our history, with its reliance on long, written proposals and remote, abstract criteria for selecting grantees. In our new approach, the due diligence review entailed 100 to 200 hours of work per organization, looking deeply, on-site and side-by-side with the grantee, at what they do; their financial health; their organization, their leadership and management, how they measure outcomes and use the information to improve their work; and generally how well we and they could work together.

Some of this was fact-gathering, a more extensive and more practical kind than we had ever done before. But the complexity arose not just from getting good financial statements, or from figuring out exactly how decision-making and accountability worked within a given organization. Those are always hard questions, and foundations don't ask them nearly often enough. But they can be answered fairly objectively if you do enough work.

The complexity — and in the long run, the fundamental challenge to our approach — came in the subjective decisions that we and the grantees both had to make as the process unfolds. Is this organization really ready for and committed to growth? Is this management and staff going to stick with us through the hard work ahead? Can we work together — not just deal respectfully with one another, which any grant relationship ought to entail, but actually work on some of the same things, side-by-side. Are they and we going to be able to share information and impressions, exchange advice and criticism, and be honest enough to think together, rather than dancing around one another the way most grantmakers and grantees normally do? The answers to those questions aren't on any balance sheet. You can't figure them out in a round of 20-minute interviews. It took us much longer than we thought.

But more importantly, the approach has worked well for us. We've netted a dozen organizations that show signs of real quality, a reliable product, a plausible case for effectiveness, a willingness to test and measure their work and to improve it along the way, and then, over time, a determination to grow to serve more of the young people in their communities or target areas. Getting that selection right is probably the most important thing we have done to raise the odds of success. All the right techniques applied to a fundamentally bad match would have gone nowhere. Or worse, it probably would have done real harm, certainly to the grantees, and probably to us.

The Search for Compelling Organizations

It's worth noting that we didn't start with a prescription for how youth development should work — some preferred model that we wanted to test and refine and try to replicate. Nor did we set out with some "broken" system in our cross-hairs, and try to undo it. We didn't look for grantees whose programs conformed to our idea of what works or what tasks are most important. Most of us know less about youth development than our grantees do, and we'd be hard pressed to wow any of them with our expertise. What drew us to a given organization and its approach to youth development was, first of all, its *theory of change*. In simplest terms, that means the organization can tell you, at both a micro and a macro level, exactly what they do, with whom, at what cost; why that activity logically leads to measurable results; and how they define and recognize success when they see it. If they can't yet prove their case, they can at least make it concretely, and they can formulate it as a reasonable hypothesis that can be tested — and, if we're successful, will be tested.

They had one other thing that we consider essential: interim measurements that point toward success. Not all of them were capable, yet, of capturing these measurements in their day-to-day operations — either because the labor and technology for doing so was beyond their budgets, or in some cases, simply because no one ever asked for that information. But they could tell you what those measurements would be, and they had, in many cases, at least partial data to show that they took those measurements seriously.

Finding those organizations, with those qualities and characteristics, has turned out to be absolutely crucial to everything that follows. As we continue to refine our new work, one of the things we still seem to be spending most of our time doing is getting the selection process right. We want to know, from the outset, if a potential grantee has a compelling product — something that has withstood some testing, that seems persuasively linked to a plausible *theory of change*, .that can work at greater scale, and that, in general, seems to be improving young people's lives. Not surprisingly, the products and services that met those standards turn out to be quite different from grantee to grantee. Each represents a solid, persuasive approach, but not always to the same thing. Some deal with older children, some younger. Some work with schools, some keep out of the schools. Some concentrate on cognitive and intellectual development, others on social or emotional growth, many blend all of these together.

While there is no underestimating the value of due diligence (it underpins our whole approach!), it is not nearly the whole story. After due diligence comes another key piece of work: We begin a more formal relationship with grants for business planning. Many grantees have strategic plans — often very good ones, but also almost always formulations of vision rather than of activities, resources, and performance targets. Our goal is to help them put on paper something more like a plan for setting up and running a small business: how they would proceed from where they are today to the level of growth and impact that they envision for themselves, how they would pay for it, and how they would know when they arrived. This is the area where Bridgespan has proven to be particularly helpful.

As we see it, the business plan should clarify, both for the grantees and for others, what level of growth they want to achieve, by what time, and with what means. We assure them, as they set out to answer these questions, that they could plan on significant support from us if the plan is credible, and that we will let them know, as the planning proceeds, what role we could take in any given game-plan. We not only fund the planning process, we provide the services of expert business planners and consultants, work side-by-side with grantees in asking questions and weighing answers, and make it clear that we, too, are looking for possibilities in this process, rather than seeking to implement a vision of our own.

These are some examples of the kinds of questions and challenges the business plans take on, and the way some of our grantees are responding:

- One organization that has a solid record of success in a small, tightly defined part of its neighborhood is determined to serve the rest of the neighborhood. To guard against diluting the quality of its services, the business plan sets up a phased expansion — while also prescribing steps for improving the service and management in the area where they're working now. We gave them one of our largest grants — $5.5 million dollars over three years — to help them prepare for and manage that expansion. In the first couple of years, they have dramatically increased the number of young people they're serving, restructured the board, hired key department heads, and reorganized their management. They now have a new evaluation system and have raised $30 million (and broken ground) to finance a new building. The result is not just a bigger program, but a better organization. Neither would have meant much without the other.

- Another organization is already operating in three states, and wants to expand to several more. But the group decided first to make all its current program sites financially independent. In a second stage, the business plan lays out steps for increasing the number of young people served in each of those current sites. Only in the third stage — after a deepening of management in stages One and Two — do they actually move into new locations. Already, less than two years into the business plan, they have nearly tripled the number of young people they're serving in their flagship program in Massachusetts, and doubled the number in their two locations outside of the state. Just as important, the outcomes are keeping pace with the enrollment: Last year, 71 percent of the children in the program showed significant improvement in writing skills, and 81 percent of the parents said their children were doing better in school thanks to the program. Our grant, of $2.75 million over four years, has helped fund the management improvements and strengthened their ability to gather data like these quickly, to make sure that growth and quality continue to go hand-in-hand.
- A third organization is going to widen its mix of programs, and to reach more young people with the services it already offers. But their plan calls for two major interim steps: restructuring the organization to manage a bigger, more diverse operation; and creating a reliable system to measure the outcomes of the various services, to know what's working and what isn't. We've given them $1.75 million, with which, among other things, they've launched the evaluation mechanism.

The most important lesson from this work is that the process of business planning seems to be almost as valuable as the finished product itself. For instance, as a first step, each organization needs to clarify its *theory of change*, and then examine the whole program in light of that theory. That exercise has some remarkable, immediate effects for some organizations. One or two discovered that they were doing work that bore little or no relationship to their *theory of change* or core business. Those activities weren't leading to success, but they were using valuable resources. In other cases, organizations found that parts of their organizational structure — the number of people assigned to certain functions, the amount of money raised for this or that — didn't correspond to the priorities defined in the *theory of change*. Their goals were pointing one way, but their use of resources was pointing another.

That wasn't our conclusion, it was theirs. Even before we started discussing how the organization would grow or change, what it would do differently, or how we could help them accomplish any of that, the business planning process helped them see what they were currently doing in a new way.

However, in all candor, I must say that the business-planning process has been much slower, more difficult, and at times more contentious than we had expected. It has been hard work for us, and harder still for the grantees. We found, time after time, that the questions we were asking had not been asked before. Many grantee organizations had grown up mainly on the inspiration of remarkable individuals, or with practices and programs rooted in longstanding tradition, or with a long line of support from contributors who had a preferred way of doing things. Calling those activities into question, even if the questions were merely “how much does this really cost?” and “how does this help you achieve your goals?” — the very act of scrutinizing some things was painful and difficult. Yet even the organizations that went through the most difficult of these exercises ended up finding them highly useful, and are today living solidly by their business plans.

Besides mapping the process of change and defining the end result more clearly, the plans also set measurable interim goals or benchmarks. The point isn't just to have something to measure, but to let the organization know whether it is on course for a larger success, or whether it needs to adjust.

What We Have Discovered So Far

Doing this work effectively — everything from searching out prospects to conducting due diligence to supporting business planning — has brought about at least two major changes in the way we are staffed and organized and how we operate. The first is that our staff now includes several new senior employees with the new title Portfolio Manager. The title reflects a fundamentally new kind of job in the foundation world: someone who is helping organizations manage themselves effectively. Their job isn't to devise some ideal new way to approach youth development, it’s to help organizations design and implement their own ideas for improving services, achieving more, and growing. Portfolio Managers’ backgrounds include finance and management, business and strategic planning, organizational development — disciplines that have to do with effective operation and performance, skills honed usually in the for-profit sector. But all of them also have some relevant experience in the nonprofit sector, with the particular challenges and opportunities that nonprofits face.

The second big change in our organization was the invention of a fundamentally new kind of evaluation unit. Lots of foundations have staff for evaluation, and they take it seriously. But it tends to be a post-mortem exercise, more of an autopsy than a checkup on a living patient. That’s certainly the way evaluation had always worked at our foundation, and in that respect we were fairly typical. Even when evaluators were working steadily during the life of a program, their job was really to gather information for a final assessment, not to influence what went on from day to day. In the end, the data were usually inadequate for drawing any firm conclusion about our intervention in complex systems, so the result was often much longer on equivocation than on useful judgments. And before long, it would go into the last box with all the rest of the files, just as the last drawer was being emptied.

What we now call our *Office of Evaluation and Knowledge Development* concentrates on diagnostics and vital signs, not post-mortems. Its job is to help grantees develop their own systems to measure what they're accomplishing in the present tense, and then use that information as soon as it's available to manage and improve the program. When these information and data systems are in place, organizations will have timely answers to questions like: How many people are participating in Program X, and how does that compare with the goal? Do young people who spend more time in component Y perform better at outcome B than those who do not? What's the rate of staffing and turnover in Program Z? Are those numbers what they were projected to be by now?

More significantly, the evaluation staff is involved in our work with grantees from the very start. The department takes part in due diligence and in business planning. Its involvement reflects how we are using all our resources at the Foundation toward achieving the twin goals of operating more effectively and adding as much value as we can to the work of our grantees.

The evaluation unit is also responsible for evaluating our work: measuring the contribution we make to our grantees and the field, the yield on our investments, the success and shortcomings of our grantmaking approach. It's overall goal is to develop useful knowledge for our grantees, the field, other parts of philanthropy, and public policymakers, based on both what our grantees accomplish and how useful we are to them.

Much of what I've been talking about has involved expanding, reaching more people, attempting and producing and accomplishing more. So before I go much further, I'd like to say a word in defense of that beleaguered idea, growth. In his critique of venture philanthropy earlier in this lecture series, my friend Bruce Sievers summed up a lot of people's misgivings about growth when he took on the synonymous idea of "going to scale." While we don't view our work as venture philanthropy, scale is an idea that's important to us. In fact it's central to much of what we do with our grantees. So I take Bruce's caution seriously and feel it deserves a careful consideration.

"The drive to 'go to scale,'" Bruce said here last year, "potentially conflicts with the pluralistic, responsive character of civil society. While some efficiencies may be generated by large-scale service delivery systems, they almost inevitably lose a sense of local guidance and committed participation that accompanies it." (2).

That's an important concern, succinctly worded. I'm just not convinced that it is a real threat to youth development at the moment — nor that it will be for a very long time to come.

The graver threat, I think, is a system of values so weighted toward the philosophical and sentimental — or so committed to remote effects that are years or decades away from being apparent — that we lose any basis for knowing whether what we are doing is worthwhile or not. Our No. 1 concern is not growth per se, but performance. If forced to choose, I'd results over scale any time. But one essential way of judging overall success is to compare the magnitude of what we accomplish to the magnitude of what we are trying to change. Perhaps small is beautiful. But beautiful or not, small is small. And the problems we are trying to solve are large. The very act of measuring our accomplishment relative to the size of the problem drives us to ask: Is this enough? Not just have we done good, but have we done enough with the resources at our disposal?

If a program does something valuable for young people — if we find that it genuinely promotes healthy development and constructive citizenship — how do we argue that such a program should not reach more people? Do we really place a value on keeping that program away from some larger number of beneficiaries? Is it more important for the program to stick to its simple, homespun roots than to achieve more, do better, reach farther? That is not, I know, what Bruce was arguing. But some have taken the anti-growth argument so far that it seems, at its extremity, to defy the whole reason philanthropy exists, which is to advance human welfare, not just maintain it.

Bruce's deepest concern, I believe, starts with some sad lessons of over-hasty and ill-considered growth — organizations that were pushed to expand too fast, to dilute their program to suit too many different places and purposes, and to exceed their managers' capacity to control what happens at the front lines. Quite often, working through the business planning process with grantees, we've encountered that rush toward growth among some of them. More than once, an important benefit of business planning has been that it puts a brake on some organizations' growth plans, by helping them confront the things they need to resolve before growth can be responsible and effective. The lesson I take from that experience is that you can be mighty cautious about growth — even to the point of postponing it or slowing it down — and yet still be committed to it as crucial for long-term success.

That's why at the Clark Foundation, we deliberately start with business plans, with interim management and growth targets, with a careful scrutiny of assets and management strengths and available resources — precisely because we are determined to avoid the perils of careless growth. We work with grantees that are already committed to expanding. We listen to their vision of what lies on their frontier, and then we help them examine that vision, develop tools for building and managing and measuring it, and when those plans become solid and persuasive, we invest sizable amounts in bringing them to reality, in measured phases. Some grantees will stumble. We will make some mistakes. But when that happens, I'm pretty sure it won't be because we valued growth more than quality and effectiveness.

All of this rests on a *theory of change* of our own. It starts with the observation that young people get more of what they need for healthy development and constructive citizenship if they live in communities with strong institutions to guide and mentor and get them involved. There are many kinds of such institutions. But in some places, particularly low-income communities, those institutions are few, weak, small, and unsteady. Build the institutions, help them manage themselves better and smarter, give them the means to do more for more people, and you will significantly raise the odds that young people will grow up to lead healthy and stable lives. By making substantial, long-term investments in outstanding organizations with demonstrably effective programs, strong leadership, and a commitment to growth, we can eventually create better outcomes for both the young people and the communities in which they live.

But there's a further step to the theory. All of these institutions — including the strong and stable ones, and all the ones in better-off communities where resources aren't as scarce — all of them could be learning more, innovating more, improving and diversifying what they do, if they were part of a stronger, more well-knit field of youth development. The thinness and weakness of the field holds back progress as much as the weakness of particular organizations — arguably more so. Our foundation isn't big enough to build a field of youth development on its own. In fact, the field will be built not by any foundation, but by the institutions in the field. If we really find and invest in some of the best of those, and help them build their stature and ability to lead, and their opportunities to connect and work with one another, we will be influencing the formation of a better field — indirectly, but concretely. That is, in any case, the theory that guides us now, and on which we are basing our next few years' work.

Questions We Hope to Answer

I'd be remiss today if I didn't make clear that we are still in the early chapters of this story. We're still refining our work — you should hear some of the healthy, and at times heated, discussions we're having about our *theory of change*. I'm quite convinced that a lot of what we're doing now will be quite different in a year or two from what it looks like today. There are, in fact, a number of really critical questions that we won't be able to answer for some time. Among all the big question-marks on our horizon, these four strike me as especially important.

First, will we have the patience and the attention span to stick with this all the way? I believe that everyone working on this program now is in it for the long haul. But the work we're contemplating is long, and any individual career is comparatively short. Officers and trustees of our foundation will come and go before all these business plans have come to fruition, with firmly established measurements of success. National priorities will change, emergencies will erupt, opportunities will spring up to tempt us. But the Foundation has a history of taking on risks and staying the course. I believe strongly that we can keep our focus. But I could be wrong.

Second, will other funders step in to join us, and to support our grantees' growth plans? At this stage, while the plans are just being written or during the early stages of implementation, our grants are bearing a substantial share of the increased cost. But the plans are too ambitious for us to fund alone. We do have some funders collaborating with us now, but it's only a start. And foundations can be very territorial about grant relationships. If the grantees or their business plans become too indelibly "branded" as Clark Foundation undertakings, other funders may not step in when major new investment is needed. We need to prevent that, but I can't yet tell you how that will work.

Third, for all the new activity these organizations are contemplating — not to mention all the organizations we've yet to select or invest in — won't some public money eventually be needed? We believe that better youth-development service in the out-of-school hours is an essential public activity. It's reasonable to expect that activity to be funded with additional public dollars. Eventually, the Clark Foundation is not going to be the permanent source of support for all that activity, and quite possibly no amount of private philanthropy will be sufficient to fund all of it. Yet at this moment in history, is there a realistic hope of new public money for any domestic priority? I simply don't know.

Fourth, assuming we do hold to our commitment, we will be devoting all the foundation's resources to this field. That will mean finding many more grantees — and many of those will be less advanced, less experienced, less organizationally strong than the ones we're working with now. This simply isn't a field rich in sturdy, time-tested institutions. Can we work with a future generation of grantees that likely will require a lot more help and patience than did this first round? Can we confidently spot potential even when the actual performance has been only brief and small? Again, I think the answer is Yes, but we'll have to wait and see.

Now, I began this discussion by saying that I regard this moment as a time of significant self-examination in philanthropy, and potentially a time of change in the way at least some foundations operate. I proceeded to tell you a story about that sort of change in one foundation. But I want to make clear that this is not a story about how the philanthropic sector ought to change, or what it ought to look like in the future. It is the story of one foundation, pursuing one method of investment, for one area of need. It's an important method that we feel has been underdeveloped, and that deserves a chance to prove itself. But philanthropy and the needs it addresses are far too big and broad to have any one right way of doing things. Ours is just one new approach.

What makes the story relevant is not that we've landed on the right answer. We frankly don't know yet whether we even have a good answer, although we're certainly encouraged at this point. What is significant to my broader point about philanthropy is that the kinds of questions we've been raising — Are we effective? Are we as effective as we could be? Does our work make the greatest use of the resources at hand? — those questions are now being asked elsewhere in the foundation world, at least episodically, more searchingly than at any time since the 1960s and '70s. If other institutions are coming up with answers that are different from ours, that's natural, and it's good. A healthy capital market, in philanthropy no less than in business, is always better off with more diversity and experimentation, not less.

I know from the amount of interest and inquiry we've received, from the amount of debate in the media, and from conversations with colleagues elsewhere, that philanthropy could look much different in ten or twenty years' time from the way it looks today. How certainly that will happen, how it might shape up, what principles will guide it, and how diverse and versatile a sector this will be — those questions can't be answered yet, and won't be for some time.

But for all its rough edges, philanthropy has the ability — and an extraordinary freedom — to examine itself and to change. It could do more of that, and it could move faster, and I hope it does, sooner rather than later. Forums like this one at Georgetown's Center for the Study of Voluntary Organizations and Service help keep up the momentum for change by shining the spotlight on progress in philanthropy, while also calling attention to where it falls short. We all owe a thanks to Virginia Hodgkinson for making this series possible and keeping it going strong.

Let me conclude my talk today by saying one last thing. As I've indicated, our story of change is still in its early stages. It will be years before we'll fully know the outcomes of our work. But I can promise that we will dutifully report on our progress, whether good or disappointing. In return, I ask all of you to keep watching. To keep listening. To keep asking questions. And, please, let us know what you think about what we're doing.

Endnotes

1. Lester M. Salamon, "The Resilient Sector: The State of Nonprofit America," in Lester M. Salamon, ed., The State of Nonprofit America, Washington: The Brookings Institution, 2002.

2. Bruce Sievers, "If Pigs Had Wings: The Appeals and Limits of Venture Philanthropy," address to the Waldemar A. Nielsen Issues in Philanthropy Seminar, Georgetown University, November 16, 2001, unpublished manuscript, p. 13.