Giving Voice To Ideas:
The Role Description Plays In The Diffusion of Radical Innovations

by

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ABSTRACT

One of the more stable findings in the "diffusion and knowledge utilization" literature is that simple innovations, those compatible with the existing practices in a field, are spread more easily than those which challenge standard practice. Yet it is the more radical innovations that hold special promise for advancing the practice of a field.

Using an action research methodology, the author studied the diffusion of radical innovations in two very different programmatic settings, first in an undergraduate affirmative action program on a university campus and later in a philanthropic-driven effort to fund charitable work with recoverable investments rather than grants--a practice that is called "program-related investing." The two programs together served as test cases--one as a precipitating paradox and the other as a conscious experiment--in overcoming barriers to the diffusion of an important category of innovations: innovations that require individuals to practice in new ways and acquire new skills, that cause some disruption to the broader organization and that involve the "soft" technologies of knowledge rather than the "hard" material technologies.

The literature treated diffuser's descriptions of their innovations as self-evident, whereas the author found that diffusers of these radical, practice innovations unintentionally gave incomplete and in some cases misleading descriptions of their work. An argument is made that effective description must do more than represent the original innovation with some accuracy. It must enable diffusers to teach those aspects of their practice which are difficult for them to make explicit by including opportunities for practicing side-by-side, whether these be through simulated practice worlds or actual ones. It must also enable appropriate transformation of the innovation. This can be best accomplished by structuring a dialogue between diffusers and (potential) users to lift up multiple descriptions of the practice. It is the process of comparing such descriptions that
allows diffusers and users to build up an understanding both of the essence of the innovation and of ways in which transformations may preserve or damage this essence.
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I dedicate this dissertation to

Steve Cohen.

with love.

Hope is an orientation of the spirit,
an orientation of the heart.
It is not the conviction that something will turn out well,
but the certainty that something makes sense,
regardless of how it turns out.

Vaclav Havel

"Ideas and their manifestations as practices or products are at the core of social change."

G.Zaltman; R.Duncan; and J.Holbek
When I think of this dissertation, I am reminded of a movie I once saw, provocatively titled *A Perfumed Nightmare*. This movie was unusual in that it was shot by an amateur, a member of a Philippine village isolated from modernizing contacts until the US Army Corps of Engineers built a bridge across the raging river near his home. One of the engineers brought the camera and then left it, along with extra film, with the young villager.

We have many similar accounts of how such places change once they are in regular touch with modern society, but this is an insider’s view of this process. My favorite shots are the jarring reminders that people of different cultures attend to different aspects of their scene. In one, a wide angle of the village, we see a villager at the edge of the frame urinating into the bushes—a commonplace event, no doubt, but one that would have been discreetly edited by someone with Western sensibilities. In another, a shot of a water buffalo grazing goes on and on with the kind of attention young children get in family movies.

There is an artifice in this film, of course: the person who cuts and narrates this movie is no longer the naïve villager who shot much of it. He has been changed by his contact with modern society, and he is now reflecting back on who he was from the perspective of who he has become. Yet, film has a fixative quality, quite different from memory, which means that the film retains important clues to how the naïve self viewed things.

My dissertation is similar in that the questions driving the research arise out of my previous practice in an undergraduate affirmative-action program on a university campus. Thus, the dissertation begins with what I call a "reflection case," in which I describe a six-year experience with diffusing one innovation and reflect on the successes and failures. This case has the same issues as the film, and, luckily, also has the benefit of some fixative pieces,
in this case writing done at the time. The principal data is a paper that I wrote my first weeks at MIT at my advisor’s, the late Donald Schon, urging. Whether Don was prescient or simply interested in the particulars of my experience I will never know, but that piece has been an invaluable guide to how I once saw things. Augmenting my own writing are a series of reports written to funders during the six years and retrospective interviews of my colleagues.

The suggestive, if not systematic, empirical data from that case then is placed in the context of the literature on the diffusion of innovations. I learned, among other things, that our innovation would be categorized as “radical” and “complex,” characteristics that were associated with low rates of diffusion. While much of my experience confirmed existing theory, something was missing from the literature: very little was written about the role of description, which I hypothesized was a central issue in the program’s initial diffusion failures and its later success. More disturbing, the literature offered an explanation that contradicted my intuitive sense of what going on. It was this: both our initial failures and later success were explained by the amount of “linkage” we had achieved, where linkage is defined as “a regularized pattern of interaction between systems, which in a real sense forms a bond between them.” (Havelock, 1969, 2-10)

Subsequently, the Ford Foundation offered me exceptional access to test whether the approach fashioned in the affirmative-action program would be of use in the dissemination of other radical, practice innovations. Together with Don Schon, I designed an action research “experiment” to test my hypothesis about description, which also served as a directed diffusion program for the Ford Foundation around an innovation known as the "program-related investment" (PRI). This setting allowed me to document a diffusion process from two angles -- that of the diffuser and that of the adopter--though it is primarily their dialogue which is put under the microscope. The Ford Foundation initiative is the major case on which the dissertation rests. These two programs together served as test cases -- one as a precipitating paradox, the other as a conscious experiment -- for overcoming barriers to successful diffusion of an important category of innovations:
innovations that require individuals to practice in new ways and acquire new skills, that cause some disruption to the broader organization and that involve the "soft" technologies of knowledge rather than "hard" material technologies.
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CHAPTER ONE:

INTRODUCTION AND THEORY

One of the more stable findings in the “diffusion and knowledge utilization” literature is that simple innovations, those compatible with the existing practices in a field, are spread more easily than those which challenge standard practice (Schon 1971; Rogers 1983; Hutchinson & Huberman 1993). Yet it is the more radical innovations that hold special promise for advancing the practice of a field. My research is directed at this special class of innovations. On the one hand, my research is concerned with a practical question: What strategies are effective in diffusing moderately-radical innovations? On the other hand, it is concerned with building a general theory of diffusion that can account for the special issues presented by these innovations.

Innovations can be radical in more than one way. For individuals, radical innovations can require that they use skills acquired in standard practice but applied in new ways, or they can require mastery of new skills. For technology, a radical innovation can be complex "tightly bundled", or both—"tightly bundled" meaning that it is not easily separated into modules or otherwise reduced (Rogers 1983, Leonard-Barton 1988a). For organizations, these dimensions of skill and technology are overlaid by dimensions of radicalness that relate to corporate structure such as an innovation’s “span”: the number of people affected by the innovation; and the innovation’s “scope”: the number of operational units that must alter their output or input operations to accommodate the innovation (Leonard-Barton 1988a, 7-8).

My research focuses on innovations in two very different programmatic settings: an undergraduate affirmative-action program on a university campus and a philanthropic-driven effort to fund charitable work with recoverable investments rather than grants—a practice that is called “program-related investing”. These innovations require individuals to practice
in new ways and acquire new skills; they cause some disruption to the broader organization; and they involve the abstract or "soft" technologies of knowledge rather than the "hard" material technologies. In short, these innovations require people to change how they practice, which means both learning something new and letting go of aspects of their previous approach.

For one concerned with the diffusion of radical, practice innovations, there is a recent literature review which is a particularly useful place to begin. In 1993, the National Science Foundation (NSF) asked Janet Hutchinson and Michael Huberman to review the "diffusion and knowledge utilization" literature. The stated objective of Hutchinson's and Huberman's review was to present state-of-the-art diffusion strategies for educational innovations, particularly those strategies that would enable the diffusion of radical innovations in mathematics instruction based on a constructivist approach to learning. But Hutchinson and Huberman go beyond the practical questions of strategy and argue that diffusion is best understood as a teaching/learning process. Therefore, just as the constructivist approach to mathematics instruction adds substantially to a student's understanding of mathematical concepts, they hold that a constructivist approach to diffusing innovations will add substantially to a user's understanding of the innovation.

In their review, Hutchinson and Huberman assert that mathematics educators and theoretically-oriented researchers in the "knowledge use" field had made a parallel intellectual journey. Underlying the earlier practices of both fields was a belief that direct communication could produce a template--of mathematical concepts or research results, respectively--in the minds of the respondent. Later research on students' misconceptions and also on the implementation of innovations documented severe distortions of the original message, distortions which led researchers to question theoretical models that cast learners (users) as passive recipients of knowledge. For many, this led to embracing constructivism, because, as one mathematics researcher argues, it was "[t]he philosophical approach that argued most vigorously for an active view of the learner" (Comfrey, 1990, p.108). Of course, this philosophical shift from an objectivist view to the "fundamentally
interpretive character of our experience of the world" did not happen in a vacuum; it mirrored the larger shift of twentieth-century thought (Grodin [1991] 1994,13).

Constructivism, at the most general level, is the view that knowledge is self-constructed and cannot be tested against a reality that exists independent of any individual's way of seeing it. Jere Comfrey describes it thus:

Put into simple terms, constructivism can be described as essentially a theory about the limits of human knowledge, a belief that all knowledge is necessarily a product of our own cognitive acts. We can have no direct or unmediated knowledge of any external or objective reality. We construct our understanding through our experiences, and the character of our experience is influenced profoundly by our cognitive lenses. To a constructivist, this circularity is both acceptable and unavoidable.

But the circularity Comfrey describes should not lead us to believe that our constructions are static or of equal merit.

We change our constructions. We do this by reflection. In so far as our constructions are verbal and include statements, we can reflect on their internal consistency or on their ability to account for our observations. For nonverbal versions, however, the character of our reflection changes. Donald Schon, for example, distinguishes between reflecting on action and reflecting-in-action. To take a simple example, when we have hit a tennis ball awry, we may stop after the shot and review our form--we pause and think.1 Alternatively, as we feel the wind pick up, we may attempt to compensate while we continue our stroke. Here we are thinking in the course of doing, which has the virtue that it can make a difference for this point, not the following one and

1 Here, we engage in a kind of reflection which Hannah Arendt has described as a stop-and-think, we step entirely outside of the action present in order to think about what we are doing. Arendt claims that this is the only mode of reflection available to us. This claim is contradicted by Donald Schon, who admits that it can be dangerous under some circumstances to think while doing, but also shows both how it is accomplished and how it is critical to exemplary practice in uncertain situations (Arendt 1971: Schon 1983.)
the issue that it may be partly or entirely tacit and therefore hard to make explicit (Schon 1987).

The constructivist sails a precarious path between rocks and sirens, arguing at one and the same time against the philosophical position that all “right” versions are reducible to one and only one version and also against the counter position that contrasting versions can be relativized. Nelson Goodman, for example, argues that relativism, the view that each version “is right under a given system--for a given science, a given artist, or a given perceiver or situation,” while not inconsistent with the acceptance of a multiplicity of “worlds”, is useless. It is the presumption of an “intertranslatability” among worlds that allows us to advance understanding and to establish what rigor means within pluralism ([1978] 1992, Chapter 1).

Hutchinson and Huberman found that the constructivist perspective, so prominent in the theoretical literature on mathematics and science education and the theoretical literature on knowledge use, had yet to penetrate into diffusion practice (1993, p.10). By this they meant that there continued to be many examples of researchers assuming that a careful statement of their findings was all that was required to produce an adequate understanding of the work in practitioners' minds.

In spite of the dearth of constructivist-based diffusion programs for empirical study, Hutchinson and Huberman held that there was little doubt as to how to approach the diffusion of constructivist pedagogy and materials. The research pointed unequivocally toward the importance of establishing "sustained interactivity," or “linkage:” a process that included "multiple exchanges between researchers and potential users of that research at different phases of the study" (p.12). What was perhaps surprising was that this strategy was identified as the most effective by researchers with diametrically opposed assumptions: not only those who assumed that practitioners were active construers of the incoming research, but also by those who assumed a more passive, reproductive receptor.
In this dissertation, I argue that Hutchinson and Huberman's recommendation is based on research into innovations that are relatively compatible with standard practice and relatively simple to understand. Where innovations are radical and/or complex, even moderately so, "linkage"—as they define it—is insufficient. Under these conditions, standard notions of linkage need to be begin with an interactive, concentrated effort aimed at producing effective descriptions of the innovations.

Hutchinson and Huberman, who simply reflect contemporary thinking on this subject, equate the quality of linkage primarily with the amount of contact between diffuser and user: more contact equals better linkage (see also, for example, Huberman 1990; Louis and Dentler 1988, or Beyer and Trice 1982.) The concept of linkage is essentially portrayed as a black box, or, to be less incendiary (because researchers have certainly tried to look inside this box), the multitude of positive effects coming from extended, personal interaction are so plentiful and so customizable that the gestalt of the phenomenon is far more important than any of its aspects.²

In this conception, "linkage" is a state but not a set of skills that can be developed. This conception therefore offers no recourse to the diffuser who has established regular, face-to-face communication with users and discovers through this interaction that his innovation is being regularly misused and, he suspects, regularly misunderstood. While the empirical literature does not provide many instances of this phenomenon, its existence can be inferred from the ample evidence that complex innovations are far more difficult to diffuse than those that are simple; that radical innovations are far more difficult to diffuse than those that are incremental (Leonard-Barton 1987; Rogers 1983; Schon 1971).

Of particular concern here is the content of innovation descriptions. In the academic literature (and in practice as well) the content of innovation descriptions is assumed to be

²The multitude of positive effects include: time for adopters to assimilate the new knowledge to their prior understandings; opportunity for diffusers to receive feedback on the research's import in the practitioner's setting or on what the practitioner understood from previous exchanges; creating the conditions for "attitude change or attitude strengthening" (or, said differently, extended contact predisposes practitioners to look favorably on the research product); and a means by which researchers may enter into the political sphere of the organizations to which practitioners belong. This point will be addressed in more detail in Chapter Three, which reviews the academic literature in more detail.
self-evident to the person who developed the innovation or has become expert in its use. This assertion can only be confirmed by looking at the theoretical assumptions these authors make. For example, Hutchinson and Huberman explain diffusion failure as the lack of "the will, competence or requisite time for interactivity." In other words, one's appetite for or even one's ability to create a direct relationship with practitioners can be wanting, but, one's ability to produce appropriate or usable descriptions is unquestioned.\(^3\) My research into the diffusion of radical practice innovations shows, however, that diffusers unintentionally give incomplete and, in some cases, misleading descriptions of their work. In particular, diffusers' descriptions are usually skewed in the following ways:

1. **Diffusers emphasize the novel and omit the routine aspects of their innovation.** Their work, therefore, may appear to be a more radical departure from traditional practice than in fact it is.

2. **Diffusers describe aspects of their work that are formulaic or technical, leaving out those aspects of their work that are difficult to make explicit because they either require a great deal of contextual background or because they are patterns of practice which are partly or wholly tacit.**

3. **Finally, and quite appropriately, diffusers describe their innovation in its original context. Yet aspects of the innovation that are critical in the original context may be less critical in other contexts. Thus, diffusers descriptions may “fix” aspects of the innovation that will prove mutable.**

Importantly, my research shows that, through a properly structured dialogue with (potential) users, diffusers can learn how to create more effective descriptions—a process that teaches both diffusers and users about the nature of the innovation.

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\(^3\) It is interesting to note that, while Hutchinson and Huberman identify “interactivity” as a skill in which one can be more or less competent, they do not suggest ways that diffusers can develop this skill. Instead, they hold that the lack of such skill among researchers is the reason that intermediaries are often hired to promote innovations, which has the negative effect of putting diffusion programs in the hands of people with less substantive knowledge of the innovation (1993.)
If diffusers give incomplete or inaccurate descriptions of their work as I have asserted, the following question arises: How have successful replications come about? One answer, and here we come full circle, is that the more compatible an innovation is with standard practice in the field, the more likely it is that a user from her own experience can correct for problematic distortions or fill in descriptive gaps (Carlile 1997, 43).

A second answer--and here we are probably close to what Hutchinson and Huberman would argue if confronted with data that showed issues with diffuser's descriptions--is that diffusers in regular contact with users, particularly during implementation phase, might detect omissions and errors in their original descriptions and correct them--an answer that offers little comfort to those diffusers whose descriptions have inadvertently inhibited the innovation's adoption.

In this dissertation, I will argue that the diffusion of innovations is best understood as a teaching/learning process. The paradigm I propose holds that:

1. The diffusion process requires two-way communication between diffuser and user, in which both parties learn what the innovation is—meaning that both parties arrive at a clear sense of its essence and of forms that are genuine alternatives to the original.

2. Effective implementation of these innovations usually requires both transforming the innovation and transforming aspects of the organization that is importing it.

3. The transformation of innovations, however, is not self-evident, instead requiring skilled judgment to determine which aspects of the innovations are essential and which are mutable.

4. Effective description of the innovation is also not self-evident, instead requiring dialogue with users to learn about deficiencies, skewings and potential transformations that may serve to adapt the innovation to a new organization.

5. Participating in such a two-way dialogue with users is likely to prompt changes in how the diffuser describes, understands, and uses the innovation.
In short, I see diffusion as a process of transformation. In this paradigm, no actor (diffuser, user) or thing (innovation, organization) is immune to the possibility of change.

Within this paradigm, effective description must do more than accurately represent the original innovation. It must enable diffusers to teach those aspects of their practice which are difficult for them to make explicit by including opportunities for practicing side-by-side, whether these be through simulated practice worlds or actual ones. It must also enable appropriate transformation of the innovation. I will argue that this can be best accomplished by structuring a dialogue between diffusers and (potential) users to lift up multiple descriptions of the practice. It is the process of comparing such descriptions that allows diffusers and users to build up an understanding both of the essence of the innovation and of ways in which transformations may preserve or damage this essence.

Overview of Remaining Chapters

Chapter Two presents what I call a “reflection case.” In it, I reflect back on my own practice as a staff member in the Mathematics Workshop Program (The Workshop Program). The Workshop Program tested a radical approach to the specialized instruction offered to minority undergraduates by most elite universities. This chapter describes the first six-years of diffusion work in which the earliest attempts were successful only with a select group of adopters: individuals who combined within themselves: comfort with the academic subject matter; a deep understanding of the intellectual isolation of most minority students on an elite campus (most were minority themselves); and strong connections to campus administration. It appeared that their background allowed them to appreciate multiple facets of the model and their positions helped them to overcome internal resistance to the radical features of this approach to academic support. The chapter then describes how, by trial and error, staff arrived at a new way to introduce people
to the ideas in the program, which was consistent with a constructivist approach to learning and was successful with a less select group of adopters.

This new approach to introducing people to the ideas in the program was acted out within the traditional approach to linkage, however. We continued to attempt to build a one-on-one relationship with every potential adopter. Our staff was small so the number of such relationships that were adequately maintained was also small. Thus, while we had gained some control over what we were able to convey, our diffusion efforts remained uneven.

The Reflection Case presented in Chapter Two gives the background, therefore, for the design of an action research program that would attempt to test both the approach to description that was fashioned in the Workshop Program and a means of creating effective linkage that would allow direct contact between diffusers and adopters but would increase the ratio of these contacts. The action research program was conducted by mounting a directed diffusion program on behalf of the Ford Foundation for the "program-related investment (PRI)," and is subject of chapters Five through Eight.

The Ford Foundation is one of two foundations credited with "inventing" the PRI and, in 1987, accounted for over half of the total capital invested nationally through this financial instrument. PRIs are usually direct loans to non-profits, though they range widely in complexity and in how they are managed. PRIs can be small, simple loans such as a no-interest loan to the Girl Scouts to print cookie boxes that will be repaid from the proceeds of the cookie sales. At the other end of this spectrum PRIs can be quite complex such as a loan to a consortium of legal services agencies to purchase and rehabilitate office space in downtown Manhattan. PRIs may be one-time investments, or, they may be organized into a regular lending program such as a loan fund, or, at one step removed from direct lending, they may be loans to an intermediary which might itself operate a loan fund. Less frequently, PRIs are made as equity investments in social-purpose businesses, or, as a guarantee to induce a bank (or other institution) to make a loan. Finally, there are some grants that are a mixture of grant and loan--known as
“recoverable grants”—that many, but not all, philanthropists count as a PRI. An example of this mixed grant/loan is a grant made to the distributor of an educational film, where the grant carries this condition: if the film is an unexpected financial success and brings in ‘x’ revenues, the grant must be repaid.

The program-related investment sits in an ambiguous space just outside the border of commercially viable investments. By law, the PRI must not be primarily for financial return, which in practice means that PRIs provide a lower return or present a higher risk then would be deemed prudent under commercial underwriting criteria. For the cluster of such investments that would further a foundation’s charitable mission, Congress opened a gap in the laws governing the fiduciary responsibility of charitable corporation board members. This ambiguous legal space allows foundations to fill a critical funding gap for non-profits by providing financing for low-margin projects that are unable to attract commercial capital. In other words, good PRI makers are usually in the business of distinguishing a loan request that is “unbankable”—but still viable—from a loan request that is simply inappropriate.

Between the presentation of the reflection case and the action research case are chapters that review the knowledge diffusion and utilization literature as well as the research protocol. Chapter Three opens by showing that there is support within this literature for all five points of the diffusion paradigm I propose above, save the issues I claim exist with description. It then focuses in on the concept of “linkage,” and Huberman’s “integrated model of research utilization” which suggests an alternative hypothesis for the increased success of the Workshop Program’s later diffusion efforts. Perhaps we simply created the conditions for best linkage practice within practitioner-to-practitioner diffusion?

Chapter Four describes the reasons for using an action research methodology in this study of the practice of diffusion. It raises the attendant issues of how to establish validity in studies of practice, which aim to develop normative theory. This chapter
concludes by describing the action research “intervention”: a series of working conferences for philanthropic foundation executives on program-related investing.

Chapter Five begins the action research case with a history of program-related investing at the Ford Foundation. This is an institutional history rather than a history of the technical developments of Ford’s PRI practice. The chapter describes how the original conception of the PRI was expressed institutionally—both in where the new, specialized division was housed and from where the funds for PRIs were drawn—and how these institutional arrangements change over the twenty-year period preceding this research.

This chapter also describes the changing regulatory environment for philanthropic foundations, in which the Ford Foundation was able to preserve a space for program-related investing. At the core, all philanthropic foundations are financial entities: their body or “corpus” are funds that, upon being entrusted to the foundation, were sheltered from certain taxes. The investment of a foundation’s corpus must meet certain, legal standards as must a foundation’s distributions—usually these are made in the form of grants that will support work in line with the foundation’s stated mission. Grants, with rare exceptions, are made from the income earned on the corpus investments. The PRI was a radical innovation in that it provided a means of directly harnessing the wealth of a foundation’s corpus for philanthropic aims: through the PRI the corpus could be directly invested in ventures such as housing developments for low-income people; businesses that would bring jobs to inner cities; or office space for nonprofit agencies. The final section addresses the central topic of this dissertation: Ford’s descriptions of its PRI practice. The chapter concludes with four PRI descriptions, made in four different contexts, that together comprise a “baseline” against which we will judge whether participation in a structured dialogue with adopters prompted any change in either Ford’s description of the PRI or in Ford’s use of the innovation.
Chapters Six through Eight are best viewed together. Each rests on the dialogue in the working conferences, but each offers a different view of what is learned in this dialogue.

Chapter Six gives a bird’s eye view of the first four conferences. In many ways, these conferences are separate events: they were held in disparate geographical regions; drew participants locally--apart from Ford staff--and attempted to map the PRI practice of that particular group. Yet, these conferences were also links in a single, conceptual chain. The MIT conference team entered each conference with a careful statement of best PRI practice that emerged from the previous conference (or, in the case of the first conference, from my interviews with Ford staff), which was then subjected to a critical inquiry by the conference participants. Each of the first four conferences surfaced one or more ideas that were later incorporated into this evolving definition of “best PRI practice.” The chapter traces this evolution.

Chapter Seven dives into the particulars of what was said in the working conferences on PRIs. I have indicated that diffusers’ descriptions are usually skewed or incomplete in three ways. This chapter shows how each of these three issues were addressed within the structured dialogue provided by the working conferences.

Chapter Eight returns to the diffuser and shows that Ford’s written descriptions of the PRI changed in ways consistent with the learning detailed in Chapter Seven. Second, it shows that the director of the Office of PRIs came to see Ford’s practice in a new light and, as a result, began a small experiment that he hoped would draw “recoverable instruments” out of the purview of the PRI office into the everyday practice of Ford grantmakers.

Chapter Nine concludes the dissertation by returning to the question posed at the conclusion of the literature review. Could the concept of linkage alone account for the data we gathered through this action research on diffusion? While the data in the reflection case is inconclusive with respect to this question, the data in the action research case is not. The descriptive issues were significant and clear cut. Once they were
addressed, the practice was adopted (or expanded) with the benefit of very little traditional linkage. This occurred in part, however, because the conferences clarified that program-related investments could be made either from a foundation’s corpus or its earnings—a defining feature as to whether the PRI was perceived as a radical or a more marginal change in philanthropic practices. And, more important, the conferences revealed that the vast majority of foundations, including Ford, had moved their practice away from the radical edge.
CHAPTER TWO:

HOW THE DESCRIPTION OF RADICAL PRACTICE INNOVATIONS CAN BE PROBLEMATIC AND WHAT A CONSTRUCTIONIST APPROACH TO DIFFUSION LOOKS LIKE

The Mathematics Workshop Program was founded on the belief that students could overcome gaps in their mathematical training in the context of— and not instead of—working on the most challenging material in the freshman calculus and science courses. This belief proved true, with three caveats. For this remediation to be successful, students first needed to adapt the distribution of their academic time: they had to be allowed to concentrate their efforts on their mathematics-intensive courses both by enrolling in three rather than the standard four courses in the first year and by attending ten hours per week of workshops. Secondly, students needed timely assistance with non-academic issues, such as delayed financial aid or housing accommodations, that otherwise might consume valuable hours. Thirdly, there was a threshold below which students’ mathematical preparation was so minimal that enrolling in these courses was not advisable. The best indicator of this threshold was whether students had passed several high-school mathematics courses, even though many of these courses were of poor caliber.4

Small study groups, or “workshops”, were the core of this intervention. Here is how Dr. Leon Henkin, a founder, described the Workshop Program:

... [students were organized into] small study groups that met regularly during each academic term [under the guidance of a workshop leader, who was usually a doctoral

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4 The University of California system at this time guaranteed admittance to students who had maintained a B average and taken a prescribed set of high school courses, including three years of mathematics. Students who did not meet the grade point average but performed above a certain level on SAT tests were also guaranteed admittance. Students who met neither of these standards were admitted at the entrance committee’s discretion.
student conducting research on mathematics or science education]. These groups were arranged to accompany regular University courses in mathematics, physics, chemistry, and computer science. They provided a forum within which students could work on homework problems, discuss results and critique one another's work, and ensured that students both devoted adequate time to school work and had an opportunity to meet and socialize with their peers. Moreover, the frequency of Workshop leaders' contact with students enabled the leaders to closely monitor students' academic progress and adjustment. Unlike university counselors or advisors who typically see students only when they are in trouble, the workshop leader offered timely advice on the spot and addressed problems in the making—such as housing arrangements or delayed financial aid—before they became crises (FIPSE 1984, 2).

Looking back, it may be difficult to imagine how radical—or, to some, how irresponsible—this experimental program was. The usual practice at this time was to ease minority students into campus life. Upon enrolling, minority students were assigned to a special advisor; counseled to avoid rigorous mathematics and science courses in their first year; and encouraged to partake of a range of supportive services such as classes in study skills, reviews of mathematical material presumably learned in high school, and tutorials (Fullilove and Treisman 1990).

This set of practices had enabled very few minority students at Berkeley to complete mathematics or science-related majors. In fact, this set of practices had enabled few minority students to pass beginning calculus: the average grade for all Black students taking first term calculus was D+, in every year between 1972 and 1978—a statistic that

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5 Workshops met four or five days a week, one or two of these sessions would be led by a "peer-leader," usually an upper-classmen who worked under the supervision of the regular leader.

6 "Minority" is a problematic term, particularly in California, where people of African-American, Hispanic, and Asian descent are in fact the majority of public school populations. I have used this term in spite of its issues.
does not account for those students dropping the course mid-term. For Hispanic students, for those years, the average grade was C- (Culler 1985.) Such poor performance in freshman calculus lent credence to the broadly held view that all minority students would benefit from remedial work during their first year at the University.

It is perhaps common sense to assume that poor performance was the result of poor preparation, but, in this case, there was not such a simple one-to-one correspondence. Some of the students were good students from academically poor schools but some were good students at academically rigorous schools. Preparation alone could not account for minority students’ poor performance in Berkeley’s mathematics classes.7 (For national data that supports a similar conclusion see Steele 1992.)

In 1977, Phillip (Uri) Treisman, then a doctoral student in mathematics, had a concrete reason to question the view that poor performance by minority students was solely the result of poor preparation. He taught in a program that prepared minority high school students for college-level work. The previous year, Treisman had personally taken a small, mathematically-talented cadre of these students through an honors version of freshman calculus. Yet, within a few months of beginning at Berkeley, all four of his former students were struggling in their calculus and science courses. It could be argued that the “pilot” of the Workshop Program came about when Treisman gathered these four students together and led them in a study group.

This was not the only antecedent of the Workshop Program, however. As it turned out, Treisman was simultaneously researching the study habits of groups of Black and first-generation Chinese undergraduates. Earlier, in 1975, while developing a training program for Mathematics Department teaching assistants, Treisman became aware of the high rate at

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7 The best data for this assertion was collected after the Workshop Program was well-established. This data revealed that, prior to the Workshop Program, among economically poor Black students, the best prepared (those in the top third of SAT scores) failed out earlier and in larger proportion than their peers with SAT scores in the middle third. The pattern among middle class Black students was reversed, similar to the pattern among majority students (Treisman 1985.) Among Hispanic students, the usefulness of SAT scores as a predictor of performance depended on gender: they were better predictors for Hispanic men than Hispanic women (Mathematics Workshop Program unpublished data).
which Black students were failing freshman calculus. "I had made it my practice to speak with T.A.s about the weak and strong students in their teaching sections, and the regularity with which Black students appeared in the former group and Chinese students within the latter struck me as an issue that should be addressed in the training sessions" (1985,4).

Personally skeptical of instructors' explanations for this phenomenon, which ranged from academic preparation to intellectual capacity, Treisman began an eighteen month ethnographic study, which he described in his doctoral thesis.

I accompanied these students [20 Black and 20 Chinese] to the library, their dormitory rooms, and their homes in the hope that I might see first hand how they went about learning, and doing, mathematics. I was particularly interested in learning about the use they made of their textbooks and classroom notes and about their approach to homework and review problems that, at least on their first try, they could not solve (p.5).

It is not possible in a compressed summary to do justice to this compassionate and at times disturbing account of Black and Chinese students' first years at the university. The conclusion, blandly put, was that Black students maintained a rigorous boundary between their social and intellectual lives. They almost always studied alone, and, when they got stuck, they had few effective strategies for moving beyond the impasse. The Chinese students, by comparison, quickly formed study groups. These groups "enabled their members not only to share mathematical knowledge but also to 'check out' their understanding of what was being required of them by their professors and, more generally, the University" (p.13).

Treisman discovered a tragic disjunction between how the Black students he studied viewed themselves and how the university "saw" them. These students were the serious, elite students of their high schools, but most of these schools had low academic standards. Most entered Berkeley determined to succeed, confident in their skills, and accomplished at
isolating themselves from their peers.\(^8\) Whereas university staff saw them as under-prepared for college-level work, they saw themselves as well-prepared. Treisman intuited that an honors program would appeal to these students, to the point that they would willingly do a great deal of supplemental work to participate in it, and designed the Workshop Program accordingly. Against common practice, he recruited minority students for an honors program that required ten hours of workshop participation and encouraged all participants to enroll in the most rigorous calculus and science sequences.\(^9\)

The pedagogy used in workshops was forged prior to Treisman’s ethnographic study and in a different context. During the 1970s, a loose group of professors together with their doctoral students (among whom was Treisman,) were doing research on the acquisition of mathematical language and the relationship between use of this language and conceptual understanding of mathematics.\(^{10}\) This research shared a constructivist epistemology and employed the armamentarium typical of such research: micro-analysis of video-taped exchanges between teacher and student. Treisman, who was an experienced teacher, found the Neo-Piagetian research into students’ representations of mathematical concepts compelling but mismatched to the demands of the classroom. He held that the press of day-to-day demands in a classroom meant that it was nearly impossible to investigate how a particular student construed a particular problem. (For other mathematics teachers who arrive at a similar conclusion see Cobb, Wood et. al.1990.) Over his years of teaching, Treisman had fashioned a group-learning approach to mathematical instruction where “worksheets,” were used both to convey material and to “drive interaction between the students” (Treisman 1992).

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\(^8\) In these schools, where few students went on to college, it was probably adaptive for the serious student to isolate himself or herself. For students attending schools with higher academic standards, which, in the Bay Area serve predominantly white and Asian students, isolation was also common, but here it grew of being different, of being one of the few Black students in a school.

\(^9\) This practice changed as the program grew in size and began to serve students interested in a more diverse set of majors. For example, both architecture and business had a calculus sequence prerequisite, but, did not require the most rigorous calculus sequence. By 1982, all Workshop students were tested and were placed into the most rigorous sequence for which they were prepared.

\(^{10}\) The professors included logician Dr. Leon Henkin and physicist Dr. Robert Kaplans.
In an interview with the author in 1992, Treisman described his approach to writing "worksheets." He begins by comparing his approach to that of fellow doctoral student/workshop leaders:

I was also interested in mathematical language, but, I was mostly interested in problems that would test the limits of understanding, and would reveal things that I knew were misconceptions that students have. ...And I was also more interested in putting in problems that I saw as embodying pieces of mathematical culture, full culture, the things that you don’t learn in school.

...[Also by 1977, I knew that the Chinese students I had [studied] had access to files of final exams, and I knew that we would have to provide access to the kinds of things that they and fraternity kids had.... If you look at the 1978, '79 worksheets, they [contain both problems I wrote to test students’ conceptual understanding and problems from old exams. Theater was important to keeping students’ going on tough worksheets, so my problems] were handwritten, but the problems from the old exams were Xeroxed onto the worksheets, for theatrical effect....

Treisman recruited his fellow doctoral students to lead the first workshops. Thus, the Workshop Program was established as an informal "lab school" where pioneering work in the use of learning groups for mathematics, chemistry, physics and computer science instruction was done, much of it documented in the doctoral dissertations of workshop leaders. In these groups, students were taught to work cooperatively, critique each others' work and test each others' mastery of the material.

The Workshop Program outcomes were dramatic. Workshop student performance was evaluated extensively between the program’s inception in 1978 and 1984. In every one of these years, the average grade of both Black and Hispanic Workshop students in calculus was B+. For the first time in campus history, minority students were outperforming their
majority peers in freshman mathematics classes. Moreover, these results remained steady while the Workshop Program doubled in size, serving near the end of the period, most minority students enrolled in targeted courses. In answer to critics who claimed that the Workshop Program was "creaming"—admitting only the best-prepared minority students—statistics showed the weakest prepared students accounted for the most substantial gains in performance. The Workshop appeared to have wiped out differences in initial academic preparation (Treisman 1985).

Perhaps the single most impressive statistic, however, comes from comparing graduation rates in mathematics-based majors. Among the first cadre of Black students (entering class of 1978-79), 65% either earned a degree or were in good standing at the end of four years. The comparable rate among the remaining total student body of Berkeley was 41% (Fullilove and Treisman 1990).

**Diffusion of the Mathematics Workshop Program**

The first external support for the Workshop Program was provided in 1980, after two successful pilot years, by the Fund For Improving Post-Secondary Education (FIPSE). FIPSE asked all applicants to plan for and attempt to diffuse their work, should it prove successful (FIPSE 1980a,10). Therefore, Treisman put some effort into making the results known and stimulated a good deal of interest in the program. Over a six year period, staff were to show the program to people from over sixty campuses and work more intimately with twenty-five who were serious about starting a program modeled on Berkeley’s. This became a natural laboratory in which to learn about diffusion.

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11 This was the Workshop Program’s highest enrollment. In that year, it was serving 188, about half the total number of minority students matriculating at Berkeley.
12 This six year period covers 1980-1986, the time when I was on staff with the Workshop Program. Dr. Rose Asera documented the number of diffusion sites and calculated the number of diffusion sites for this period at twenty-five when I interviewed her on September 13, 1992 in Berkeley, CA. Diffusion continued after I left the Program staff and, in fact, intensified because the program garnered significant support for diffusion from the Dana Foundation in 1987 and because Treisman became nationally known as a MacArthur Foundation “genius award” recipient.
The diffusion plan submitted with the 1980 grant request had two parts. First, staff proposed “to assist in replicating the Mathematics Workshop on another campus of the University of California,” this activity was later described as “offering technical assistance.” Secondly, staff proposed to “prepare and disseminate materials in order to share our experience widely” (FIPSE 1980b,14). What these “materials” might be was left vague until two years later, when diffusion had begun in earnest. They were:

- A statistical study of the program’s effect on Black and Chicano student performance at U.C. Berkeley,
- A manual describing the day-to-day operation of the program suitable for use by those seeking to replicate our work (FIPSE 1983,22).

In particular, the manual was to cover four areas: recruitment; creating community among minority students matriculating at a predominantly white campus; important administrative procedures—such as monitoring student attendance and advising; and teaching mathematics in groups, which focused primarily on the construction of worksheets, including a compendium of worksheets which could be used directly or used as models. Beyond this, staff intended describe the rationale for the program’s design and common problems for which adopters should be prepared (Culler 1987,11).

It is probably obvious that Treisman aimed to meet the spirit of FIPSE’s request to diffuse good results while expending a minimal amount of staff time. There was good reason for this. The administrative staff was small—excluding himself, there was the equivalent of two full-time staff—and he had proposed a substantial expansion of the Berkeley program between 1980 and 1983. Treisman also expected that diffusion would present some challenges, thus he confined his plans to partnering with one site and, if this was successful, he expected to add others slowly. This plan, however, did not account for the interest he would arouse through his numerous speaking engagements.

The strong demand for diffusion support placed program staff, which I joined in 1980, in an awkward position. If we responded to the calls and letters requesting information or support, it would cut the time we had to learn about diffusion short and
stretch a small staff quite thin. The alternative, however, was even less appealing. Whether support was offered or not, it was clear that some would attempt to mount a similar program. Staff worried that programs fashioned from an inadequate understanding of the model might put students at risk and might also damage the credibility of the model.

Thus, with little planning, we shifted from the initial diffusion plan—working in an intense partnership with one other campus—to working with many campuses. Our typical introduction to the model began with a two-day visit to the Berkeley campus, where people observed several workshops in session, reviewed our extensive data on minority student achievement, met with workshop leaders and a few students, attended a curriculum meeting, and spoke with administrative staff about their roles and responsibilities. Following this introduction, we offered technical assistance over the phone and, when possible, Treisman would travel to the campus. We provided a compendium of worksheets and samples of other documents such as our recruitment letter. Finally, we were convinced that the model could not be transferred, a whole cloth, to a new campus without being changed. We believed that the model would need to be adapted to a new campus, even substantially changed, and were quick to advise people asking about the model of this eventuality.

The partnerships we formed differed in intensity—from weekly contact at one end to sporadic contact at the other. We were overwhelmed with work, thus the amount of contact depended on the initiative of the adopting campus.

The results of our early efforts were mixed. There were a few programs that both captured the spirit of the original and replicated our results. The bulk, however, did not. Intensive contact with adopters did not ensure a higher quality program, but, with two notable exceptions, sporadic contact was associated with the lower quality programs.

More interesting, and in some ways more disturbing, was that the Workshop Program was being misunderstood in predictable ways. People interested in the model were primarily from two professions: administrators in academic support programs and mathematics professors. Based on their professional background, they tended to adopt a different, visible feature of our program.
Those from standard, remedially-oriented, academic support programs appeared to see our innovation as group rather than one-on-one tutoring. The shift in how tutoring was delivered was not accompanied by a shift away from the standard, remedial content. Nor were students encouraged to enroll in calculus and science sequences that prepared students for mathematically-intensive majors. It is not difficult to understand why at least the latter occurred. Academic support professionals were typically deeply concerned with the welfare of minority students on their campus, and, over the years, they had seen many flounder or fail in the mathematics and science classes—the dismal record of minority students in freshman calculus at Berkeley was but one example of a broader pattern. Surely the practices of enrolling new freshmen in these courses must have appeared unwise, if not actually irresponsible (Treisman, 1985).

Faculty, on the other hand, tended to see the workshop program as honors versions of the standard courses. A subset of this group grasped the pedagogical approach, but, faculty tended to ignore the supports that helped students handle the challenging academic material. For example, a key feature of the Workshop Program’s design was that responsibilities for academic content and monitoring students’ adjustment to the university were integrated into the workshop leader’s role rather than distributed to different individuals. This meant, for instance, that it was the workshop leader’s responsibility to call any student missing one workshop meeting unannounced and to go to the home of any student who missed two meetings in a row. Thus, Workshop leaders required training in both the pedagogical approach and the counseling duties, but the latter was frequently overlooked.

In sum, many people appeared to select a visible feature of the model which stripped the model of its potential to create a fundamental change in their usual practice. Research in perception (Nisbett and Ross 1980) and in the implementation of innovations (for example, Beyer and Trice 1982; Berman and McLaughlin 1979), lead one to anticipate that self-legitimizing distortion will occur. Yet, this distortion was predictable enough as to be both
troubling and inviting. Were it more random, it would seem more an issue of the individual adopter, but consistency suggested to me that we look to the broadcasters: ourselves.

Seeking constants in these people’s experience of our model led me to wonder if our descriptions might be at issue. We had quickly developed a standardized format and “spiel” for the two-day introductory visit. After so many years, neither my colleagues nor I felt we could reliably reconstruct our rationale for standardizing these visits. It may be that this was simply a response to a repetitive task. It is my conjecture, however, that standardization was one means by which we attempted to ensure a comprehensive introduction to the model. This conjecture rests on what I believe was a broadly held, yet tacit, set of assumptions among Workshop staff about the differing roles and responsibilities of the “diffuser” and the “adopter”—here I am consciously using the parlance of diffusion theorists. The implicit division of labor we held was this. We, the diffusers, possessed a deep understanding of the Workshop Program at Berkeley and our job was to describe the model as comprehensively as possible. The adopter, on the other hand, possessed a deep understanding of his campus and it was his job to adapt the model to his campus. This implicit model appears, pentimento-like, in Uri Treisman’s description of the Berkeley Workshop Program in a report to FIPSE on diffusion activities.

We began with a successful program on an elite research university campus that had demonstrated that it could reliably help minority students to excel in freshman mathematics and science courses. ... The project was based equally on a novel analysis of the problem of minority underachievement and the mobilization of an idiosyncratic set of resources special to the Berkeley campus” (emphasis added, FIPSE 1988, 2).

To Workshop staff, “adaptation” meant discovering the idiosyncratic resources of the importing campus and crafting a program that would harness these. The features of the program that devolved from the “novel analysis” were to remain constant site-to-site. It did
not occur to us that adopters would eliminate core features of the model, such as its non-remedial content, and think of this as "adapting" the model. To us, this was a "mutation" not an adaptation.

While what staff in general and Treisman in particular meant by adaptation may be vague, Treisman could be quite specific in relating what he hoped to avoid.

A group of deans from Southern California [visited the Workshop Program] and then sent some people to "study" our program, and studying it meant Xeroxing everything, and that was the first event in which we realized that something was wrong with our dissemination. It was so comically out of whack. ... We used to have a [recruitment] letter [that talked about how we sought to prepare students to earn a] Nobel prize and they were sending this out to community college students (Treisman 1992).

It may be useful at this point to ask whether I or other staff entertained alternatives to the possibility that our descriptions of the workshop program were ill formed or misleading. Did we, for example, look to intensifying the amount of contact we had with adopters? In at least this sense, I would argue that we did. At first, staff responded to the demand for diffusion support by trying to meet it. Disappointed by the results of this approach, we began, in Treisman's phrase, to "graciously discourage" the interest of some people, either because they did not have the necessary mathematical expertise or because they were acting alone, without departmental sanction. We had learned to focus our energies on the most likely sites, but we did not see more intensive contact as a complete solution. In part, this was because our one planned diffusion site, with which we were in steady contact, produced a disappointing program.

The mathematics professor who spearheaded the effort at the planned site was quite impressed with the Workshop pedagogy. He was convinced that this was a better way to teach calculus and, therefore, decided to position the program within the mathematics
department. (The Berkeley Workshop Program was a free-standing program of the Academic Senate.) This professor, different from many, accepted the importance of a counselor/advocate and was able to garner the time of professional staff member in addition to several workshop leaders. The power he saw in our teaching method created an ethical dilemma for him, however. While he was concerned with the performance of Black students in the department’s calculus courses, he did not feel it was appropriate to selectively offer minority students a better way to learn the mathematics. He therefore incorporated the materials and methods into the extra-lecture services of the Mathematics Department: he opened these sections to everyone, and gave no special attention to recruiting Black or Hispanic students. Though a small number of minority students attended these supplemental sections in the beginning, they quickly drifted away. By the second semester, there were no Black or Hispanic students remaining in the sections. The students who remained did well in the course, but we counted this effort a failure: this program in all likelihood increased the disparity between majority and minority students’ grades in that course sequence.\[13\]

**Studying Description**

With Treisman’s encouragement, I began an informal program of research. First, I interviewed the original workshop leaders and produced an oral history of the Workshop Program’s early years. Next, Treisman and I selected three people whose adaptation of the program we admired and tried to reconstruct their learning process. I then shared our reconstruction with one of the three to see whether it matched his sense of his own learning process. These conversations and the ensuing changes we made in our approach to diffusion pedagogy were the subject of an unpublished paper I wrote upon entering this doctoral program and will be quoted below.

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13 The details in this example are recreated from my memory alone. In my 1992 interview with Treisman, he answered my request to reconstruct this piece of Workshop history bluntly: "____ was a failure."
The informal oral history revealed a rather dramatic shift in Workshop Program personnel and the focus of staff’s attention about four years after inception (1982-83). In the early years, while workshops met five times a week, four of these were led by doctoral students and the remaining session was led by a “peer-teacher,” which was both an economy and a step in preparing students in the skills necessary to form and run their own study groups. By 1983, when diffusion had become a strong focus of staff effort, most of the doctoral student/workshop leaders had completed their theses and moved into faculty positions. Several doctoral students were replaced by former workshop participants, undergraduates who were nearing graduation. During this period, the workshop program was also undergoing a significant expansion—in these years it grew from 80 to 188 students. With the exit of the old guard, the locus of concern and discussion shifted from pedagogical issues such as how students were construing particular mathematical concepts to issues that came of the Workshop Program’s increasing size, such as how to maintain a strong sense of community among students. I concluded that, with the exit of the old guard, the composite of our program descriptions—that which an adopter might put together from the various conversations he or she might have with staff and students—under-emphasized the pedagogical underpinnings of our program’s success, which left the incorrect impression that students’ impressive academic performance rested mostly on developing an intellectual community on the campus.

Turning now to our reflections on those who had created programs that we admired, we discovered that they shared an unusual set of background characteristics. Two were former faculty members in elite universities: a mathematician who had recently been promoted to dean and a chemist, who had left her faculty position to follow her husband to his new position as a dean of a science college. Both of these people were also themselves minorities: the mathematician was Black, the chemist Hispanic. The third person was white, however, she was also the director of a Minority Engineering Program, and possessed an unusually strong mathematics background for someone in this position. (She had an M.A. in mathematics and had written two mathematics teaching texts.) Each
of these individuals combined within themselves: comfort with the academic subject matter; deep understanding of the intellectual isolation of most minority students among a predominantly white student body; and strong connections to campus administration. It appeared that their background allowed them to appreciate multiple facets of the model and their positions helped them to overcome internal resistance to the radical features of this approach to academic support.

Secondly, different from the predictability of Workshop Program “mutations,” each of these three programs took a strikingly different form. For examples, I describe two. The dean, who worked at an elite, eastern liberal-arts college, rejected our free-standing “honors program” and instead developed a “Science and Technology” course, for which he recruited heavily among the minority students. He claimed that our model could exist at Berkeley, where the student body was so large that an honors program dominated by minority students enjoyed a measure of anonymity. On his campus, however, such a program would be quite visible and, if successful, would bring forth strong complaints from the students who were not recruited. The chemist also rejected our free-standing model, but, she integrated the program into the traditional academic support unit on her campus--against our strong advice. We believed that our status as a program of the faculty provided critical protections in turf wars and allowed us to resist pressures that we water down our expectations of students and serve everyone. We learned from her success that the turf wars on our campus were particularly virulent--probably brought on by the way we protected the pilot: while the approach was still unproven, we used our extensive data on minority student performance to criticize the efficacy of other academic support programs on the campus.

Finally, these adopters were distinctive in their approach to learning about the model. The dean particularly so. Here I excerpt from my paper, written shortly after leaving the Workshop Program:
…each tour of Workshop Program included visits to workshops that were in session—a chance to see the program “in action.” Typically visitors were impressed with this part of the tour and commented about it during or after, mentioning that the room was noisy for a classroom, noting that the students seemed very animated, or asking questions about the background of a student who had caught their eye. The dean stands out in my mind because his response to this part of the tour departed significantly from typical responses. The dean led a personal investigation into precisely how the workshop leader encouraged students to work together. He asked many specific questions. Did the workshop leader treat shy students differently from talkative ones? Did the leader assign students to the groups? Did she rearrange the groups during the workshop? How did the workshop leader use a student who knew the material well? The dean seemed to place himself “in the workshop leader’s shoes” and then raise questions about the difficulties he imagined encountering.

Later that day, the dean asked us to alter the next day’s agenda: he wanted to co-teach a workshop rather than meet with more people to discuss the program. The dean wanted to experiment with the workshop leader’s theories for dealing with the difficulties he had raised (Culler 1987, 10).

I then reflect on this story to generalize how the adopters, who I called “sophisticated adopters,” were distinctive learners.

But not all sophisticated adopters asked to teach a workshop. What distinguished one type of adopter from another? It was, in fact, the lack of a pattern among the sophisticated adopters that distinguished them. They asked questions that ranged widely, were idiosyncratic in the aspects of the program they choose to investigate deeply (ibid.).
**Descriptions are Found to be Skewed or Radically Incomplete**

As I indicated in the previous chapter, diffusers unintentionally give incomplete and, in some cases, misleading descriptions of their work. Descriptions of radical practice innovations tend to be skewed in these three ways:

1. Diffusers emphasize the novel and omit the routine aspects of their innovation. Their work, therefore, may appear to be a more radical departure from traditional practice than in fact it is.

2. Diffusers describe aspects of their work that are formulaic or technical, leaving out those aspects of their work that are difficult to make explicit because they either require a great deal of contextual background or because they are patterns of practice which are partly or wholly tacit.

3. Finally, and quite appropriately, diffusers describe their innovation in its original context. Yet aspects of the innovation that are critical in the original context may be less critical in other contexts. Thus, diffusers descriptions may “fix” aspects of the innovation that will prove mutable.

Each of these points can be illustrated from our early attempts to diffuse the Workshop Program.

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14 The phrase “radically incomplete” is Jeanne Bamberger’s and it is used to acknowledge that, within a constructivist epistemology, our descriptions are always incomplete in at least this sense: whether I am a researcher or a practitioner, I arrive at my innovation through action—a seamless continuous experience; my description of this dynamic experience is necessarily fixed, however. Some of our descriptions are so partial as to be radically incomplete, in which case they inhibit understanding. Less obvious perhaps is that our descriptions are also necessarily skewed because we are selective in what we retain from our experience. Bamberger points out that our descriptions are partial in both senses of the word: they give us only parts and the are partial to some aspects of our experience over others (1996).

More generally, our descriptions of our actions or practice are constructions. They are, to use Donald Schon’s appropriate phrase, “always attempts to put into explicit, symbolic form a kind of intelligence that begins by being tacit and spontaneous. Our descriptions are conjectures that need to be tested against observations of the original” (1987, 25).
Diffusers Emphasize the Novel

First, our early descriptions emphasized the importance of helping minority students create an academically-focused intellectual community on campus—what Uri Treisman labels as “the novel analysis of the problem of minority underachievement.” In so doing, our descriptions under-emphasized the pedagogy and mathematical sophistication that had brought about this pedagogy. The interviews I had with the original workshop leaders educated me—a staff member for a year—to the intense and creative effort which early workshop leaders had expended developing the pedagogy and curricular materials. But, by the time I had joined the staff, this once innovative aspect had become, in the parlance of diffusion researchers, “routinized.” The pedagogy was now being passed to new workshop leaders through a special form of apprenticeship: they experienced it first as students in the program, then as co-leaders with an experienced teacher. It was so integrated into the program staff’s everyday practice that it had ceased being remarkable and, concomitantly, remarks on it had receded from staff’s description of their model.

I surmised that our descriptions were skewed to such a degree that we might unintentionally have given the impression that student’s impressive academic performance rested mostly on developing an intellectual community on the campus. This was most certainly not the case. The pedagogical approach was a critical aspect of the work, as this student describes in his interview with Rose Asera, an anthropologist who wrote an ethnographic study of the workshops.

I always got through on memorizing and plugging in. It was just mechanical: plug into a formula, crank it out. Here they teach you how to think, to learn why you do what you do, not just what to do. Somewhere around the end of my senior year in high school I started realizing the importance of knowing why something goes on. Now if I don’t know why, I don’t want to do it without understanding. I like that the people don’t just tell me the answer, and don’t just tell me what to do. They ask me, “What do you think you should do?” If I have it right, they encourage me; if I have it wrong, they explain it.
The workshop helps me spend more time on math than I might on my own. It especially helps to spend time on subjects that I wouldn’t otherwise. For the last midterm, my weakest area was min-max problems, but I couldn’t make myself go over them. I knew when I came to reviews [however], that we’d go over min-max in workshop (emphasis added, p.57).\textsuperscript{15}

\textit{Diffusers Omit Highly Contextualized or Tacit Knowledge}

Second, as the dean showed in his questions as to what workshop leaders actually did to encourage students to work jointly on the mathematics problems, our descriptions omitted discussion of key aspects of our practice. The omission is predictable because knowledge such as the dean sought has been gained and used within a particular practice setting. Knowledge-in-practice is both highly contextualized and supported by tacit understanding, which is difficult to make explicit. Thus, what the workshop leader \textit{actually did} to encourage students to work together is difficult to say in part because the workshop leader likely employed different strategies depending on who the students were and what she wanted from having them join into a group.

In addition to being bound to its context, this kind of knowledge is supported by tacit understandings that are difficult to make explicit. For example, the workshop leader is quite unlikely to be able to articulate the entire body of knowledge that she might be able to bring to bear on these decisions: her nuanced knowledge of that week’s worksheet; her understandings about her students’ particular mathematical strengths and weaknesses; her knowledge about their problem-solving aesthetics: which students are neat and systematic and which puzzle through problems on small scraps of paper. Tacit knowledge such as this is most accessible when it is actually being employed; it can be made conscious when the workshop leader notices that she has made an intervention to help students work in a group.

\textsuperscript{15} Asera gives other similar, if less complimentary, testimony. For example, one student states “I spend about 60 percent of the time feeling frustrated in workshop. The problems seem too hard. I NEVER finish them here. I mean to do them at home, but I rarely do. Sometimes I feel so stupid when its an easy problem and I can’t see it (p.58).
and reflects on why she made the decision to put these particular students together; or, why she asked them to continue, even though they were puzzled; or, why she decided to step in and help the group finish a particular problem. (Carlile 1997, Bamberger and Schon 1991, Schon 1983). This the dean knew intuitively and provided for in his request to work along side the workshop leader while trying out her theories.

Diffusers Describe Their Innovation In Its Original Context

Third, the original Workshop Program was housed outside of the University administration; it was a special project of the Faculty Senate. We believed that this provided critical protections to resist pressures to “water down” our expectations of students and serve everyone. It seemed plausible to staff that this critical independence could be preserved under the auspices of a department, and, therefore, the professors who attempted to implement a departmental version of the model drew no comment from Workshop staff. Such was not the case with the program that was implemented under the auspices of the administrative office responsible for minority support programs, however. Yet, it turned out that as long as these programs preserved the targeted recruitment, viable forms of the original model could be exist under the aspices of any of these bodies.

A final point which, however, draws on information that became apparent after the close of the period covered in the reflection case. While it is theoretically possible that there are an infinite number of such transformations, our experience in the Workshop Program’s diffusion was that the transformations clustered easily into a few types. The structures of the adopting institutions appeared to limit the kinds of transformations that would be viable and the numbers of these institutional forms were themselves limited—for example, large public research universities, small private liberal arts colleges, community colleges. Thus, there was a point where staff, who had begun diffusing a model which they knew only in the form it took in its original context, had the experience to place this original as one of a family
of such forms. Thus, there was a qualitative difference between the early part of the diffusion program—where we were actively learning about both the efficacy of our descriptions and the common transformations of the original—and the later part.16

**A Constructivist Approach to Diffusing the Workshop Program**

Over time, we switched our approach to diffusing the Workshop Program. In the beginning, our diffusion effort centered on conveying to potential adopters as comprehensive a description of the Berkeley program as we could muster, which, from a pedagogical point of view accepts the underlying premise of “direct instruction”: that one can simply “pass on information to a set of learners and expect that understanding will result (Comfrey 1990).” Our revised approach was based more on a constructivist approach to instruction. We selected certain features of the model that we had learned were critical to its success. Using our growing sense of how these features were regularly misconstrued, we worked to focus adopters on understanding: what these features were; why they were there; and how adopting these features would force a change in their current practice.

I conclude this section first with a list of how we structured learning opportunities for diffusers within our constructionist approach to diffusion, which is followed by a list of how we changed our approach to description.

**Learning Opportunities**

When initial conversations revealed a serious interest, rather than offering a tour of the Berkeley program, we offered to schedule a time for director Uri Treisman to visit the caller’s campus and assist in preparing some preliminary research. This research included conducting a review of minority students’ academic records and

16 Dorothy Leonard-Barton similarly distinguishes between the initial implementation of new production technologies and what transpires after this initial period of intense learning. She argues, in fact, that the initial phase of implementation is so dynamic that it is best understood as an extension of the process of creating the innovation. “The major thesis of the framework [she provides] is that initial implementation of a new technology is an extension of the invention process. That is, instead of the predictable realization of a preprogrammed plan, implementation is a dynamic process of mutual adaptation between the technology and its environment.” (1988b. 2).
interviewing minority students who had enrolled in these courses. In essence, we
offered guidance in re-creating the research that informed the original design.

Â We changed subsequent Workshop "tours" to interactive teaching sessions, where
visitors were asked to co-teach workshops, participate in curriculum planning
sessions, and observe workshop leaders' doing their monitoring functions.

Â We abandoned the manual, and, instead, consciously crafted a series of stories
about students to illustrate the reasons behind certain features of the Berkeley
model. We abandoned the manual because every boiler plate section we wrote
seemed destined to mislead adopters. We had come to the view that the
Workshop Program was successful because it was both administratively flexible
and unrelentlessly demanding of students’ time. (One visitor characterized our
program as a combination of “Piaget and Mussolini.”) In striving to write a
comprehensive resource, we created a static document that did a poor job of
conveying the key to our success: flexibility. In place of the manual, we
substituted several polished and carefully-crafted stories. These stories were
based on actual students and real events, but they might be composites or
otherwise fictionalized. The stories offered differing views of the program, views
that in some ways contradicted one another. Whereas the manual gave the
appearance of answering questions, the stories--which revealed gaps, overlaps and
contradictions--provoked them. The stories became a means of prodding potential
adopters into an active rather than passive learning mode.

In sum, adopters were asked to begin by looking more closely at their own
campus: How were minority students performing in their mathematics- and science-
based classes? Were there particular classes that acted to as a bottle neck, diminishing the
number of minority students eligible for mathematics and science-based majors? What
was the experience of minority students on their campus who had attempted or succeeded
in mathematics and science-based majors? What kind of resources--internal or external--
might be available for an Workshop-type program? By focusing potential adopters (and, importantly, potential transformers) of Workshop Program on their own campus initially, and, only after this on the model—which gave Workshop Program staff an opportunity to learn about the adopting campus very early in the diffusion conversations—Workshop Program staff were able to intertwine at the outset two critical tasks: 1) teaching adopters about the model and 2) working with adopters to redesign it to fit their context. These moves did not eliminate mutations, but the numbers of people capturing the complexity of our model increased enough that we were comfortable with our new methods.

*Constructionist Approach to Description*

Our approach to description changed in four, fundamental ways. They are as follows:

1. We learned from our early diffusion attempts that adopters were highly selective in what they retained from what they heard and saw of our model. This encouraged us to become selective in our descriptions as well. Whereas we had originally tried to provide a comprehensive introduction to the model, we narrowed our focus a few relevant essentials and thought about how best to teach these.

2. We recognized that there was a danger in crafting a standardized introduction to the model because, depending on the expertise of the potential adopter, certain essential features were more likely to be ignored. But, just as we had little confidence that a teacher, in the midst of the press of daily classroom demands, could understand how a particular student was construing a particular problem,

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17 This is a dicey issue, and we found that the amount of internal resources was often open to influence. We learned that a presentation made by Treisman to administrative leaders early on in the process was useful. In this
we had little confidence in our ability, in the midst of the press of a short visit, to understand exactly how the adopter was construing our model. Our experience made us alert to some usual misconceptions, as research in students’ constructions of mathematical concepts made teachers alert to some usual misconceptions. Using the pedagogical approach underlying the construction of worksheets, we tried to construct problems that would challenge people holding the usual misconstructions. For example, we posed questions about the performance and experience of minority students that challenged standard beliefs and could not be answered without adopter’s conducting some research on their own campus. While discussing the results, we could steer adopters toward seeing the complexity of the problem they were trying to address and link these to various facets of the model.

Â Our approach became more interactive: we helped people to design and conduct research on the performance and experience of their minority students in key courses and we encouraged them to “co-teach” workshops.18

Â We learned that adaptation was not self-evident and, as a practical matter, we learned that it was more difficult to influence an adopter once the new program was up and running. Usually, in order to implement their version of the model, several transactions had occurred within the college which then constrained some of what was possible. Moreover, the number of people committed to a problematic interpretation of the model was now likely increased, which increased the effort it would take to make a shift in how the model was implemented. As we attempted to become more active in helping people

way, our first description became re-integrated into the introduction to the model, with the difference that it was given to people who would not be directly involved in putting the model into action.

18 It must be admitted that this was an effective means of discouraging people who were not mathematically trained.
redesign the model to fit their strengths and context, we found that we needed to actively learn about those who would be in charge of the program and about the importing campus.

In sum, our diffusion pedagogy evolved into a transactional teaching process that was consistent with a constructivist pedagogy. Our approach to diffusion moved from comprehensive to selective; from “showing-combined-with-telling” to “doing-combined-with-telling;” from describing to educating. In retrospect, this was probably a natural transition; we brought our approach to teaching about the model in line with our approach to teaching mathematics.

**Linkage and Issues of Scale**

Our approach to providing technical assistance also changed in ways that brought our technical assistance in line with the practices associated with better-performing diffusion programs in the diffusion literature (Huberman 1990; Havelock 1990). Specifically, as new programs were mounted, we were more willing to advocate for the new effort with local administrators, usually to assist them in getting access to student performance data or start-up funds. We also more readily offered our help with the first student recruitment drive, including giving some of the initial presentations. Finally, we developed a means of monitoring the quality of replication efforts—a significant issue when you have no formal control over the work of people who are attempting to use your ideas. We set up with all replication sites an exchange of term-end grades for all workshop students.

These changes brought a measure of control to our diffusion efforts. We learned who to discourage and, among the others, where we were able to invest time and effort, quality programs regularly came about. On a practical level, however, we still had not solved our diffusion problem: we were still unable to meet the demand for our support and, as a result, new programs got an uneven amount of attention. It proved to be very difficult to address
this lack of staff capacity. During this period, the Berkeley project was not able to raise enough soft money to cover the true staff costs of dissemination and, had the money been available, it was not clear who we could have hired. The model was radical enough that, even when more money became available in 1987, it proved difficult to find people who were effective providers of technical assistance apart from who were also staffing the Berkeley program or one of its sister programs (Treisman 1992).
CHAPTER THREE

REVIEW OF THE “KNOWLEDGE DIFFUSION AND UTILIZATION” LITERATURE WITH RESPECT TO THE ROLE OF DESCRIPTION AND THE PRACTICE OF LINKAGE.

Overview

The knowledge diffusion and utilization literature (D&U) is vast. Everett Rogers' third review lists 3,000 articles pertaining mostly to technological innovations in ten academic traditions (1983,45). Nathan Glaser's second review, published the same year and casting a somewhat broader net, estimates the number of relevant articles at 20,000 (1983); Dunn et al., published a decade later, estimates the number at twice Glaser's (1994). It is not surprising, therefore, that reviews, such as those cited above, have been unusually important in the theoretical development of this field.

In the face of this great bulk, it is odd to note that there are relatively few empirical studies: in 1982, Beyer and Trice could find only 27 that revealed "systemic observation" of knowledge use in organizations. The vast bulk of studies employ a retrospective survey or interview design (Beyer and Trice 1982, 591; Dunn et. al. 1984, 2832) leading, in Huberman's phrase, to a "list [of] apparently generic characteristics shown to be consistently associated with higher rates of dissemination and utilization (1983, 505)." Much of the work, in other words, aims to refine a set of general characteristics that will help us predict who will adopt "x" innovation and when.

The other general characteristic of this literature that should be noted is that this literature is primarily concerned with the spread of innovations from a research setting to
a practice setting. The paradigmatic example of such a diffusion is the Agricultural Extension Service, where innovations such as hybrid corn, or specialized weed sprays, or crop rotation, developed at the land grant universities or commercial labs are spread to farmers, with the assistance of an intermediary: the local extension service agent. Though studies of “practice to practice” diffusion, such as we faced with The Mathematics Workshop and the program-related investment diffusion effort are represented in this literature (for example Hulme’s 1990 study of the diffusion of the Grameen Bank), these studies are not numerous and have not produced results that have prompted the better known theoreticians to treat them separately from the main stream of the work (Glaser 1983; Rogers 1983.)

_A Practice-Based Diffusion Paradigm Meets the Academic Literature_

The screen used to organize this review is the five point diffusion paradigm given in Chapter One, which was initially developed from my experience with the Mathematics Workshop diffusion. The literature lent support for all of the points in the paradigm, save that it was almost silent on the issue of description.

On two-way communication (or “linkage,”) the literature is unequivicable that this is essential for developing a strong relationship between researchers (diffusers) and practitioners (users) (summarized, for example, in Hutchinson and Huberman 1987; Huberman 1990). It is also widely accepted that the nature of the relationship between researcher and practitioners is key as to whether or not research findings get applied in a meaningful way.

On the transformation inherent in implementation, the literature presented a mixed picture. Some view this as primarily a process of change to the innovation; others view it primarily as organizational change. In their landmark study of educational innovations, Behrman and McLaughlin saw effective implementation as a process of “mutual adaptation” of innovation and organization. Behrman and McLaughlin saw the "adaptation" of educational innovations as usually having the negative effect of stripping
an innovation of its potential to change a teacher’s or administrator’s usual practice. They found however that this stripping was averted when senior managers engaged in "mutual adaptation"--when they changed aspects of the organization to allow the innovation to truly penetrate its regular routines and, of course, encouraged a thoughtful process of experimentation with the innovation. This theory was confirmed and further developed through research on production technologies in twelve very large companies. (Leonard-Barton 1988; see also Van De Venn and Rogers 1988; Van De Venn 1986; Schon 1971.)

There was agreement that change/adaptation grew not just from issues that arise during implementation, but also from the adopter’s mental frame. Rogers & Rice presume that a highly planned and rational process has led to an organizational decision to adopt an innovation--the general problems of the organizations are first defined and agreed upon and then a search conducted for an innovation that matches the problem. Adaptation, then, results from improving the match between problem definition and existing innovation (1980). Others saw adaptation as a function of a "generative learning process" (Buttolph, 1992).

On the diffuser’s learning, a few studies note that a two-way dialogue between diffusers and adopters often produces a conceptual shift in the diffuser’s understanding of his innovation was documented by (Huberman, 1990; Kato 1998)

On description of innovations this is what was found. The literature equates good description with simple and clear description (Huberman 1990; Rogers 1983). A wide range of researchers assume that such descriptions are a precondition for diffusion (Rogers and Rice 1980; Havelock 1990; Kato 1998). There are a few examples in the literature, however, where simple description is demonstratively NOT possible because the innovations are so complex: for example in the case of an artificial intelligence system developed for Digital salesforce. In this case, the metaphor of “diffusion” gives way, yielding to a process called “integrated development.” In integrated development the possibility of creating a bridge of understanding between researcher (diffuser) and
practitioner (adopter) is abandoned. Practitioners join research teams and together they develop innovations (Leonard-Barton, 1987).

There is a related issue that has been profitably mined: what attributes of innovations are correlated with more successful adoption? Where the theoretical model of diffusion was based on studies of simple, concrete technologies passing from hand-to-hand (and where the "adopter" is an individual not a member of an organization), favorable "attributes" can be clustered into two components: 1) the ability of the diffuser to provide data that the new device will offer a performance improvement, and, 2) the ability of the diffuser to provide clear instructions for use. (summarized in Rogers 1983.)

Certainly, the latter is a descriptive issue, and, as the innovations are of a vaguer identity—such as educational innovations like new methods for teaching reading; constructivist-based mathematics curriculum; and "whole school" improvement programs—the former becomes a descriptive issue as well. Thus, we hear about providing "craft-validated" knowledge and "inspirational thrust" and information about the innovations "adaptability." Where research is concerned with more complex technologies being put into operation within large organizations, a similar correlation is made: the "communicability" of a new technology—degree to which a technologies operating principles (know-how) and underlying scientific principles (know-why) can be communicated to people other than the developers—is directly related to the success of implementation efforts. And the prescription for improving implementation outcomes: full written documentation and formal training for new users (Leonard-Barton, Oct. 1988). The thrust of these findings is clear: If you want better diffusion outcomes, you need to be better at describing the innovation in ways that are "succinct, suggestive and easily understood"(Huberman, 1987).

Where the theoretical models is based on educational innovations, which have a vaguer identity, and where the dominant research methodology shifts away from retrospective studies and more toward process studies, a more nuanced picture of how adopters deal with information emerges. With no concrete technology to pass from hand
to hand, adopters can be observed interpreting (Louis and Dentler, 1988), learning (Hall and Loucks, 1978.) and, importantly, selecting some aspects of the innovations over others (Huberman 1983; Behrman and McLaughlin 1979.) Sometimes selection occurs in such pronounced ways that the end result is viewed as a "distortion," or "mutation" (Huberman, 1983; Nisbett and Ross, 1980). Researchers need to provide practitioners with descriptions that are "succinct, suggestive and easily understood" but it is inappropriate to assume that these descriptions will be readily understood. How will better understanding be achieved? Where innovations are simple enough that researchers are confident that clear and succinct descriptions can be crafted, there is a strong support for the strategy called "linkage:" contact, usually face-to-face, between diffusers and adopters that will allow back and forth (two-way) conversation and for a sufficient amount of time to allow for mastery of a new practice and for correcting shallow or faulty understandings.

Alternative Hypothesis:

Huberman states that the most effective form of linkage is that which begins before the onset of a study, continues during the conduct of the study, and then intensifies as the findings "are brought more forcefully into the practitioner universe"—what others call the "implementation phase" (Huberman 1990, 365). Contact prior to a study gives researchers an opportunity to shape their topics around areas of interest to practitioners. As time moves on and dialogue continues, researchers get a chance to see their emerging findings through practitioner's eyes, which at the very least helps them to "contextualize" their findings, and, sometimes to substantially reconceptualize them. These connections make for an ongoing conversation that intensifies during the initial implementation phase. In sum, such a process allows practitioners to share in both the framing and interpreting the study before attempting to make use of it. On the intended recipient's part, this partnership around the research allows time for learning, for correcting shallow or faulty understandings, and in terms of group theory, pre-disposes them to be supportive of the work when it is complete.
But, these conditions are unlikely to be met with practice innovations. Practitioners usually start by wanting to solve a particular problem within their work-sphere. If they succeed, they may try to make it available to others, but the primary intent of their work is to solve a problem with which they are faced. Practitioners could form groups of like-minded practitioners to follow the progress of their practice-experiments, but, because of time limitations and a desire not to share proprietary information, they are unlikely to.

Whereas The Mathematics Workshop staff saw their approach to description as the significant change in their diffusion strategy, the shifts we described in our approach to diffusion, particularly the time spent recreating the original research study, provide a means for this extended, pre-implementation conversation. A close reading of Huberman suggests this alternative hypothesis: perhaps the more important change was in degree of linkage. The Mathematics Workshop staff may simply have happened upon a means to created the conditions for good linkage in practice-to-practice diffusion.
CHAPTER FOUR

RESEARCH METHODS

The Ford Foundation Presents a Diffusion Problem

The Ford Foundation approached MIT in 1987 as part of a concerted effort to understand why, after nearly twenty years, relatively few foundations were making Program-Related Investments. Prior to approaching MIT, Ford had commissioned three studies on PRIs: one by Edward Skloot (1984) and two by Melinda Marble (1986, 1988). The Skloot study aimed to measure the level of PRI activity among other philanthropic foundations. The Marble studies aimed to identify whether charitable investing was occurring under a name different from PRI and what institutional barriers might exist to this form of “untraditional grantmaking.” By 1987, Ford was ready to support research on how these barriers might be overcome. As this formulation of the agenda suggests, Ford was convinced that the PRI was a useful philanthropic tool the potential of which was not well understood or exploited in the philanthropic community.

From the results of Marble’s interviews and from personal experience, deputy director of the Office of Program-Related Investments Jan Jaffe posed three hypotheses for the laggard adoption rate of the PRI. They were:

1) Program officers who had not used the PRI appeared to hold a rigid and, from Ford's perspective, overly narrow idea of the program areas which PRIs could advance. In novices’ minds, the PRI was usually associated with investing in low-income housing, which meant that program officers whose expertise lay in other areas--the arts, education or citizenship--assumed that they should not use the PRI.
2) Novices appeared to believe that only experienced investors could make PRIs. Jaffe, whose personal experience with this instrument was substantial, was of the opinion that a lack of investment expertise need not be a barrier to making PRIs since technical assistance was available either through external consulting firms or board members, who tend to have investment backgrounds.

3) Finally, in interviews with Marble, program officers said that they did not make PRIs because there was resistance to this concept among board members of their own foundations. 19

Jaffe presented a disjuncture between her understanding of the PRI and the typical program officer’s understanding of the PRI. She portrayed the disjuncture as an issue of perception, rather than, for example, an issue of expertise. This seemed a good opportunity to test whether the approach for helping potential adopters understand the Workshop Program would be of use in the dissemination of other radical innovations.

My experience led me to look first at Ford staff’s descriptions of their practice. Could these descriptions be misleading? Were their descriptions “usable?” I therefore asked Jaffe to answer the question: What is the PRI particularly good for? She explained that PRIs stretched foundation resources in two ways. A foundation could make a significantly larger loan than it would a grant. Ford, for example, routinely made PRIs for between $500,000 and one million dollars, whereas Ford’s average grant size was approximately $100,000. Even if foundations made loans of a similar size to their grants, the money was recycled and so the foundation could stretch its grant budget. Then, turning to the borrower’s side of this transaction, Jaffe said that a well-designed PRI imposed financial discipline on a nonprofit organization and, to the extent that nonprofits met this discipline, the PRI was both an inducement to and a tool for building the management capacity of these organizations. When I probed further, Jaffe spoke more
generally about how the vast bulk of foundation capital is vested in the foundation’s
corpus and, therefore, untapped for charitable purposes. The PRI offered a means to
harness a portion of this wealth. When I probed still further, she began to speak about
specific PRIs. My attention was drawn to the gap in these descriptions: the first was
general but also abstract; the other was concrete but so specific that it lacked
generalizability.

Based on this initial conversation, Donald Schon and I proposed an action research
“experiment” to test my approach to the pedagogy of diffusion and, in particular, my
hypothesis about how to construct better and more usable descriptions of practice.
Simultaneously, this research aimed to assist Ford in building a directed diffusion program
for the PRI. The goal and process of this diffusion program was stated in the author’s
March 9, 1998 letter to Jan Jaffe as follows:

to improve social investment practice by helping foundation staff learn from the
investment work they and others have done. In our conversations, you have
described a preliminary typology of evaluating the importance of different kinds
of social investments (See Chapter 5, section 5). From this typology, we draw a
preliminary notion of what might represent “best [program-related investment] practice—that is, work that is high quality in the view of people who do it
expertly. [We will design interactive] activities for refining (and probably
stretching) our understanding of best practice. [We will also design] activities in
which we will ask foundation staff to draw from an explicit notion of best practice
in articulating a frame for their investment work.

Why Action Research?

We proposed action research because we were persuaded that a traditional
empirical study was unlikely to yield information that would lead foundation staff (or
trustees) to change how they practiced. This kind of change usually involves some form

of what Lewin called “reeducation,” a term that Argyris et al define as “changing patterns of thinking and acting that are presently well established in individuals and groups.” Action research is well suited to reeducation in that it involves clients—in this case foundation staff—in “diagnosis and fact finding,” at least when these tasks are coupled with free choice to engage in new kinds of action (Argyris et al 1987, 4-9).

It is important to note, however, that theory which emerges from action research is normative theory. All studies address some version of the question: How ought I to act in this situation? This dissertation is a study of practice in two senses: it is a study of the practice of philanthropy and also a study of the practice of diffusion. Thus, it includes more than one version of this normative question. In the PRI study, we asked philanthropists to think with us on questions such as: “What constitutes best PRI practice?” The dissertation includes answers to that question as well as to the question: “How can we best create descriptions of radical practice innovations?”

**Establishing Appropriate Rigor in Studies of Practice**

What constitutes appropriate rigor in a study of practice which uses an action research methodology? Donald Schon addressed this question in one of his last books, *The Reflective Turn*, *Case Studies in and on Educational Practice*, and I rely on his argument below. Schon defines rigor in studies of practice as having two components: utility and validity.

**Utility**

In the core case on which this dissertation rests, utility will be judged by whether the diffusion program was useful to the philanthropists. Can we show that Ford staff changed their approach to description? Can we show that they used this approach in
other situations where they were attempting to diffuse an idea or practice? Can we show that good PRIs were made by new adopters?

It is obvious, particularly in the last question, that judgments of utility are made from within a practitioner’s own appreciative system, although our confidence in these judgments increases when it can be shown that they are shared by a community of practitioners. An example of the way such judgments are bounded can be found in the Workshop Program’s diffusion. In the end of Chapter Two, I describe our first diffusion site—the one diffusion partnership we had planned on in the first year—as a failure because, even though the program helped the bulk of its participants to achieve a good conceptual understanding of key calculus concepts, the program did not serve to decrease the disparity between majority and minority students’ grades in calculus. We traced this failure to the way recruitment was done rather than the way the teaching was done. One can easily imagine communities of practitioners who would judge this program differently—for example, practitioners primarily committed to improving the way universities teach calculus might judge this diffusion site a success.

The PRI case will reveal an interesting example of the way that judgments of utility are bounded by practitioners’ appreciative systems. Foundations typically bring together two distinct communities of practitioners: trustees, many of whom have traditional investment expertise, and staff. When trustees reviewed a proposed PRI, they often applied commercial investment standards and found the proposed PRI to be poor-quality investments. A core activity of the “PRI working conference,” which will be described below, was lifting up the many individual criteria for judging a PRI “good,” and, from this pool of criteria, crafting a shared set that fit the practice of the group philanthropists attending that working conference. Obviously, this exercise was useful for staff wanting to engage trustees in a dialogue about evaluating PRIs. It was also useful

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20 This section relies on pages 346-349 in Schon 1991. For a somewhat different approach to establishing appropriate rigor among studies seeking to inform both practice and theory see Argyris et. al. 1987. Chapters One and Two.

21 Many staff described PRI proposals as promoting a “culture clash” within their foundation.
for building a layer of description between the abstract and the concrete (my names for the two kinds of description given by Jaffe), or, in Clifford Geertz’s wonderful phrase, it was useful for building theory that “stays rather closer to the ground” (1973, 24). Finally, and the reason for this peak forward into the case, it was also useful establishing criteria by which new adopter’s investments might be judged.

Validity

What constitutes validity in studies of practice requires a more complex argument than what constitutes utility. We are essentially asking: How can the authors of studies of practice know what they claim to know? It is useful, however, to break this general claim into more specific ones, in Schon’s words, “about phenomena (what is going on here), about causality (what caused it), and about the generality of what is said to be going on and what is said to have caused it” (p343).

Schon holds that the first two questions can be appropriately answered from within a practitioners “frame” or appreciative system, but, where authors do not submit their frames to critical inquiry, their claims about generalizability are more tenuous. In The Reflective Turn, Schon concentrates on explicating an approach to assessing the validity of claims about phenomena and causality (seeking truth from within a frame) and reserves the later (seeking truth across frames) for a different book, which he expected to write after gathering different studies of practice: a set revealing a transformation in the researcher’s underlying frame.

Schon’s method for establishing validity in studies of practice is consistent with their medium for conveying information: narrative discourse. All students of practice employ narrative discourse: they tell stories. This is because it is through stories that we can “imitate” action--here Schon is employing Aristotle’s sense of the term where

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22 This second book remained only tantalizing sketched at his death.
narrative can be a simple mirror for action; it can tell what happened. Schon discerns four "categories of description" in good studies of practice and each category is in fact a different type of story: a manifest story; a causal story; a metastory—the researcher’s story of doing the research on practice--; and, a underlying story.

I will first illustrate these different modes of storytelling using my reflection on the Workshop Program’s diffusion. Then we will return to Schon’s argument that we can use these distinct categories of stories to establish validity within studies of practice.

*Manifest stories* capture examples of an individual’s or group’s practice. The beginnings of good manifest stories can be found in Treisman’s retelling of how he conducted his research on the study habits of Black and Chinese freshmen or how he developed worksheets. Were I attempting to write a good study of practice, rather than background to such a study, I would need to include some examples of observable data: text from his interviews, or, samples of final course exams from which he chose some problems and not others, or, samples of a worksheet in the making. Such examples allow the reader a means to check the degree to which the researcher’s explanations of what happened adequately capture at least a slice of observable evidence.

*Causal stories* lay out a temporal sequence of events in such a way as to give causal explanations of those events. The temporal organization of my reflection on the Workshop Program’s diffusion begins with our naive descriptions where our success at conveying the multiple ideas embedded in the original program is determined by the expertise and experience of our listeners. It ends by describing our seasoned approach, which I claim is successful with a less select group of adopters. I organize the story to show that the better diffusion result is caused by a shift in our underlying approach to description, which I variously describe as switching from “comprehensive” to “selective” or from “showing-combined-with-telling” to “doing-combined-with-telling” or, more generally, from describing to educating.

23 For a more complete treatment of the primacy of stories as a mode of reasoning about and in practice see Cheryl Mattingly 1991 dissertation or her subsequent book with Maureen Hayes Fleming (1994).
This reflection does not tell a metastory of carrying out research on practice, but it hints at one. Such a story could have been told about my interviews with Treisman, Asera and other colleagues where I sought their perspective on the Workshop Program’s early diffusion work. Though it is not included in Chapter Two, during our interviews Treisman was mostly interested in discussing how successful diffusion limits a creator’s room to transform the original, how it breeds “death by success.” For a variety of reasons, Treisman had abandoned the original, extra-departmental model and was now looking to use the underlying ideas to transform how mathematics departments approached the teaching of calculus and the recruitment of students in the major\(^{24}\). Yet, he was finding resistance to the new approach on many fronts: funders, adopters of the first model, and his staff. I could have told a metastory of my research in which I described the process of learning about the struggles of a mature diffusion program while trying to document the struggles of the infant. Alternatively, I could have told a metastory about the issues that arose in trying to reconstruct our shared history when my attempts to draw Treisman’s attention back to issues of description—a problem that was no longer central to his thinking—echoed the backward tugs he felt in his attempts to abandon a 14 year-old model that was increasingly mismatched to its context.

Drawing back the curtain to reveal a bit about the stories that my data and experience would allow me to tell but which I chose not to tell in Chapter Two gives a nice segue into defining underlying stories. The fact that I chose to tell some stories and

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\(^{24}\)Treisman’s shift was prompted by two, different trends. First, philanthropic support for university-based affirmative action programs was proving more difficult to raise in the late Eighties than it had been in the late-Seventies. Several philanthropists had frankly informed Treisman that this trend was likely to continue as there was a general perception that philanthropic dollars were being absorbed into calcified bureaucracies. Second, mathematics departments had experienced a large decline in majors. Upper division courses were difficult to fill and lower division courses—those required of engineers—were over-filled. Mathematics departments were becoming service providers to schools of engineering. Moreover, these departments had a number of aging, tenured faculty, who were no longer actively doing research. Treisman saw that it might be possible to interest mid-career tenured faculty in changing the standard lecture approach to teaching mathematics and recruiting minority students for mathematics majors—Treisman argued that there was a pool of mathematically-talented minority students who were disenchanted with engineering majors or had been excluded from them. This new approach allowed the work begun in the Workshop program to continue, but, in a way that would be covered by regular departmental budgets. For minority students, majoring in mathematics opened a number of job possibilities including, importantly, teaching mathematics at the elementary and secondary levels. During the late Eighties, teaching salaries in large school systems were increasing and there was a dearth of trained mathematics teachers.
not others reveals that I have privileged some categories of description over others; specifically, in Chapter Two I do not tell a metastory of carrying out my research on practice. I also give my attention to some aspects of the practice phenomena over others: I attend to issues of description but only fleetingly mention the constant evolution of the model program. For Schon, the underlying story is a means of “mak[ing] sense of such different strategies of selective attention.” He describes the underlying story in this way:

These are the fundamental messages or arguments [which] various authors seek to communicate through the telling of a manifest story. They have a generic, prototypical character, often linked, more or less explicitly, to the author’s favored theoretical perspective. An author tends to carry an underlying story around, embodying it now in one manifest story, now in another.

We storytellers may be unaware, or only partially aware, of the underlying stories that incorporate our deeper purposes. But we can construct an underlying story from the data of the manifest one, by asking why the manifest story includes some features and ignores others, why it begins and ends when it does, why it is couched in terms of certain categories and not others, what accounts for its thrust and direction. And, of course, any such construct may be found to be mistaken (p.346, second emphasis added.)

The method Schon advances for testing the validity of a proposed account of reality rests on Popper’s argument that the fundamental test of validity consists in a hypothesis’s competitive resistance to refutation. We cannot confirm a hypothesis directly, but, we can disconfirm it. Thus, a researcher must “juxtapose alternative plausible accounts of the phenomenon in question, and one must try to discriminate among these by means of a crucial experiment—that is, by making an observation consistent with only one of the contending hypotheses” (Popper, 1968 in Schon 1991.)

Schon carries Popper’s argument into the domain of the imagination. Appropriate rigor in studies of practice depends first on our ability to formulate alternatives to our
causal stories, which means that we must be able to “generate, compare and discriminate among multiple representations of practice phenomena” (p. 349). Secondly, it depends on our ability to test the competitive resistance to refutation of these alternatives. Here, we move into unmapped territory. Schon argues that our underlying stories “determine the kinds of observations that must be made in order to disconfirm an explanation derived from that story” (ibid.). But, whether or not these observations can be made depends on our research approach.

Turning now to the PRI case, I entered the research with two competing causal stories, one a product of my own reflection on the Workshop Program’s diffusion and the other derived from the diffusion literature. I hypothesized that the PRI would be better understood if diffusers made themselves vulnerable to the possibility that their descriptions of their own practice may be inadequate or misleading—or, more profoundly, that underlying assumptions they held about their practice may prove to be unique to their organizational type or history. And, having made themselves vulnerable in this way, their descriptions would likely improve if they invited other philanthropists to join them in a critical inquiry into both descriptions and assumptions. The diffusion literature offered the possibility that the success of the Workshop Program’s revised approach to diffusion rested not on the pedagogical shift, but, instead on having created the conditions for best linkage practice with an innovation that was birthed outside of a dedicated research setting. In the concluding chapter, I will present the case against one of these alternatives.

Design of PRI Dissemination Program—The Action Research “Intervention”

The corner stone of the PRI dissemination effort was a two and one-half day invitational working conference for ten to sixteen philanthropists; we called this

25 I am indebted to Bamberger and Schon (1991, 191) for the phrase “vulnerable to the possibility."
26 The data will show that my research method—action research—was consistent with producing relevant data about how practitioners construed the PRI. See, in particular, the conclusion of Chapter Six.
conference the Regional Working Conference on PRIs (Regional Conference). Invitations were issued jointly by the Ford Foundation and a local “regional association,” (one of a network of local, professional associations for grantmakers) to local philanthropists with substantial PRI experience or to local philanthropists who had distinguished themselves as “thinkers” about their practice and their field. Each conference was also attended by two staff from the Ford Foundation’s PRI division. Typically one-third of the philanthropists attending the conference had experience with PRIs— including Ford staff, who took the role of expert-participants—and two-thirds did not. The conference was led by Donald Schon and me from MIT and two or three consultants to us, who were themselves respected providers of technical assistance to PRI makers.

These conferences were not attempts to sell foundation staff on the idea of making PRIs. Instead, people were invited to come and think with us about the practice of making PRIs, its limitations, and, its potential for addressing some of the pressing problems facing foundations at that time. Prominent among these was a climbing number of grant requests: over the preceding five years, in response to the Reagan Administration’s diminution of federal support to social programs, all foundations had seen a significant increase in the number of grant requests. The philanthropic community did not have the resources to replace these public resources. Moreover, there was a long history of people within the field struggling with how to balance their support between new, unproven ideas and continuing support to effective non-profits (see, for example, Schroeder 1990). But, program officers were in close contact with those in the not-for-profit community and, therefore, they were privy to information which showed the consequences of the federal government’s shift in funding priorities. This information intensified their usual dilemma about how to allocate scarce resources. The PRI was of interest because, in recycling philanthropic capital, it might allow them to do more with the same amount of money.

In order to gain a broad picture of the practice of PRI-making, and to learn about how those who had not made PRIs viewed them, we asked each person attending the
working conference to write a short case: a reflection of their work with PRIs, or, if they had not made a PRI, a grant they had made that might have been a PRI and why they might or might not chose to make such a PRI. We emphasized that we were especially interested in examples of PRI failures and urged philanthropists to write about these, if they had had such an experience. Finally, we asked that philanthropists tell us about the pressing issues in their work and how, if at all, PRIs might play a role in ameliorating these. In essence, we were asking philanthropists to join us in researching their own practice.

We provided support for these self-studies. Each conference participant had a hour-long conversation with a member of MIT’s consultant team prior to writing a reflection. These conversations usually covered several possible examples and ways of framing the piece, but, they also often turned into a discussion about PRIs, where initial questions were posed and answers offered. Drafts of “reflections” were then passed back and forth at least once, but sometimes several times. Finally, to ensure a high quality product, MIT staff edited all the reflections for clarity.

The self-studies were the material of the first session, which usually lasted most of the first day. The second session was a day-long simulation of a PRI, where participants role played philanthropists, trustees and PRI seekers. The simulation opened with selecting the best PRI of four possible requests, moved into negotiations with potential borrowers, and ended with presenting the deal to the foundation’s board. The conference closed by returning to a set of deceptively simple questions: What is a PRI? and What is it good for? In a free-form discussion, we went back around these questions, which had underlay much of the discussion of the previous two days, but, we tried to approach them in a more “tough-minded” way by encouraging participants to question their own and each other’s assertions. 27

27 After several conferences there was a growing consensus on what constituted best diffusion practice and this last session shifted toward exploring technical issues through convening several technical round tables.
This design incorporated the diffusion pedagogy developed in the Mathematics Workshop Program. No standardized introduction to the PRI was given. Instead, philanthropists were asked to begin by writing about their own practice and their own organization. What was a PRI in their mind? What was their experience with this instrument? What role, if any, could PRIs--as they imagine them to be--play in ameliorating the pressing issues of their work and organization? This self-research began a coaching/consulting relationship with members of MIT’s team. The pieces that were produced become a means for everyone--MIT’s team and other philanthropists--to come to know something about each other’s ideas of the PRI, each other’s organizations, and the pressing issues within each of these organizations.

The day-long simulation moved participants from talking about their practice into a more active mode where they either demonstrated their approach or, perhaps, experimented with how they might approach the different phases of making a PRI. Moreover, the simulation provided a single, shared PRI experience for a group who rarely practiced together and who varied widely in their experience with PRIs.

This design differs from that which emerged in the Workshop Program in two, key ways. First, the design was oriented more sharply toward lifting up people’s understanding of and experience with the PRI coming into the conference. Second, it is less staff intensive. We will look at each of these points in more detail below.

The PRI presented a qualitatively different diffusion problem than the Workshop Program in that the PRI had been in existence for twenty years. Whereas the Workshop Program had freshly burst on the scene and we had the luxury of shaping people’s

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28 This was done to bring the potential adopter’s context into the dialogue between diffuser and adopter, but, it turned out that it was the most appreciated feature of the conferences. Professional dialogue between philanthropists tended to be carried out in two settings: professional meetings and as partners in specific projects. In the field of philanthropy, the coinage of status is reputation. Therefore, in professional settings, it was apparently difficult to learn from and about one another’s practice because there was a premium on putting forward the best face. Where philanthropists were partners, conversation tended to focus on the nuts and bolts of
introduction to the ideas, Ford began its directed diffusion program after many philanthropists had already formed views of the PRI. The majority, it appeared, had formed their views without any direct experience with the instrument. Thus, in my mind, it was important give philanthropists an opportunity to reveal the image of the PRI with which they entered the working conference. This approach was also appropriate because it allowed us to gather evidence pertinent to Jaffe’s hypothesis that the PRI was often misunderstood by people with no direct experience with the instrument.

Moreover, I placed a special emphasis on learning about failures because I sought to test Jaffe’s assumption that there was nothing unique about Ford--its size or specialist staff-- that would make the PRI useful to Ford but much less useful to other foundations. Perhaps the flat adoption rate for PRIs reflected its usefulness to other foundations? Perhaps during this 20 year period, a number of foundations had tried PRIs, had them fail, and then stopped using them. I assumed that Ford, a high status foundation and successful PRI maker, would not be privy to this information. It was unlikely that less prestigious foundations would initiate discussions about their PRI failures, since such failures may reflect badly on their staff’s competence. Neither the Skloot study nor the Marble studies had ruled this possibility out. Skloot based his study on the largest foundations, those most like the Ford Foundation. The Marble studies asked about a much broader category of practice “untraditional grantmaking,” opening the possibility...
that negative PRI experiences went unreported in favor of other, positive experiences that fit the broader category.

Turning now to the second, key difference: staff intensity. The Workshop Program followed one of the two common ways to organize a diffusion program. Its program staff and its diffusion staff were mostly synonymous. This arrangement had the significant advantage that it put the people with the most sophisticated understanding of the model in direct contact with those who were trying to learn about it. But, it had substantial disadvantages as well, as I described above. The staff could effectively serve only a small number of diffusion sites, and, they did so often at the expense of the mother program.29 A common alternative is to hire intermediaries, such as in the classic diffusion program of the Agriculture Department, the Agricultural Extension agent. This eases the staffing crunch of the first mode, but it puts people with a lower substantive mastery of the practice in charge of teaching it to others, which may not be an issue with marginal innovations but is likely to be an issue with radical innovations.

The design for the Ford Foundation was a conscious attempt to create and test an alternative to these two, common modes. I created at MIT a temporary organization, which was “porous” to PRI Division staff--by this I mean they flowed in and out of the organization. Specifically, Ford staff had two roles. They had final approval on the overall design and content of each conference and they acted as “expert-participants” within each conference, in some ways they were advocates for their image of best PRI practice and in some ways they were coaches for others by modeling how they analyzed PRI deals; answering direct questions; and, asking others to explain their approach. The time required for both roles could be carefully limited. Finally, as I explain below, the full diffusion program was organized as a series of conferences, so that as people began making PRIs or used them in new ways, there were opportunities to return to a rich coaching setting to look together at implementation issues and their significance for our
emerging definition of best PRI practice. Crafting a series of conferences meant that each time Ford staff were asked to be in direct contact with adopters, the ratio of their contact was more on the order of one-to-twelve than it was one-to-one.

MIT’s temporary organization absorbed all the management tasks of the diffusion effort including: maintaining contact with conference participants, organizing conferences, organizing one-on-one technical assistance between conferences, raising funds, supervising consultant staff, conducting the action research and evaluation. Probably the most interesting feature of the temporary organization’s work was how it managed one-on-one technical assistance--one of the more open-ended and time consuming aspects of a diffusion program. This was done by the consultant staff whose expertise was financial analysis but who had worked with foundations on analyzing PRI opportunities. These consultations had some of the issues associated with the intermediary form of organizing diffusion efforts, at times the consultants’ approaches were significantly different from how PRI Division staff would handle the same issue, but, the intermittent conferences meant that at least some of these differences were raised and discussed within the conferences.

An Overview of the Total Diffusion Program

The Working Conference described above was embedded in a staged research plan. The work of the first stage was arriving at a definition of best PRI practice in the perception of Ford Foundation staff. Beginning with Jaffe (our main research partner from the division), we produced a written version of her sense of “best PRI practice” (see Chapter 5, section 5). This was done over a three month period, in two telephone conversations and two letters written by the author dated February 22 and March 9. Some of the refinements she offered were prompted by personal reflection and some by

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29 In my consulting practice, this is a common problem—diffusion efforts stress and undermine the mother program—but the phenomenon has received little attention in the academic literature.
conversation with colleagues. Once she was comfortable with this description of her personal sense of best PRI practice, it was sent to all PRI staff for review. Each staff member was interviewed as to how their personal sense of best PRI practice fit or departed from the description they had read. To help staff members' root these discussions in practice, each person was asked to identify a least one particular investment, Ford's or another foundation's, which exemplified best PRI practice.

Finally, these examples were narrowed to three--one from Ford's portfolio, the second was made by a consortium of lenders in the Minneapolis area, and the third was made by a very small community foundation in Carlsbad, New Mexico--and a reconstructive case study was made of each (Culler 1989:30-54.)

The second stage, an initial working conference, brought people outside of Ford into the conversation. We sent the "best practice" cases to ten foundation executives in the southeast. Four of these executives had made PRIs: one had established lending programs at two foundations in which he had worked, a second had inherited one of these lending programs, and, the remaining two had each made a single, large PRI. The remaining six had not.

The southeast was chosen because this was the region of the country which showed the least PRI activity, and, because the foundations were least like Ford. The largest foundations are significantly smaller than Ford: the largest had 170 million in assets as compared with Ford's 6.4 billion. These foundations had small staffs, who, therefore, tended to be generalists rather than specialists. Finally, foundations in the Southeast tended to have geographical restrictions that discouraged them from working with foundations outside the southeast and which isolated them from their colleagues north of the Mason-Dixon line. The social isolation of this region made them the best

The source of the two non-Ford examples was the first Marble study. During my interviews, most Ford PRI staff in some way acknowledged their special status within the foundation community—both because of Ford's size and long experience with PRIs—and their desire to have any definition of best PRI practice be representative of the field as whole and not merely representative of Ford's practice. This desire clearly influenced the decision-making process which led to these three PRIs being chosen for further study. These cases were written after a site visit and interviews with all key participants. In addition, the files of any Ford Foundation examples that were dropped from the initial list were reviewed.

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candidate for submerged stories of PRI failures. If foundations in this regions had used PRIs successfully, we would have support for Ford's assumption that the PRI did not require either their wealth or special expertise.

The first conference did not reveal widespread failure of the PRI. It also confirmed that smaller, generalist foundations could use PRIs effectively, therefore, we opened into a third stage, a full diffusion program. The design of this program was as follows:

In year one, we planned two or three invitational, regional conferences, similar to southeastern working conference. The form of these conferences remained about the same, but the content changed as we incorporated what we learned in each conference into an evolving description of best PRI practice. In addition, we provided each participant with a grant for two hours of free technical assistance with one of the financial consulting firms working on the MIT team.\(^\text{31}\)

In year two, we proposed to develop a one-day conference on a particular issue area such as using PRIs in low-income housing. This conference would serve people who had attended the regional conference, were serious about using PRIs and had a specific interest in this program area. We would also invite program officers who had not worked with us previously, but who had extensive experience in this program area.

In year three, we proposed holding a "Problem-Solving" Conference for people who had attended any previous conference, who had subsequently made PRIs, and wanted to explore with us any difficulties they encountered.

This design was a funnel: it allowed us to work more intensely and continuously with those who choose to adopt the PRI.\(^\text{32}\)

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\(^{31}\) We limited the time to two hours because we wanted to ensure that finding assistance with analyzing a PRI would not be a barrier to considering a particular option. With the exception of some very small community foundations we expected that foundations could cover any consulting costs associated with PRIs, which can be counted against a foundation's required annual distribution or "pay out."

\(^{32}\) Obviously, universal adoption was not the goal of this dissemination effort. There are two reasons why we did not seek universal adoption. First, the PRI is a more restrictive instrument than the grant. The project must present a potential to generate capital. We could not expect that all foundations would be funding in areas that presented projects of this type. Second, and more important, the PRI is not risk free. There are examples of PRIs
Data Collection

The methodology employed here was a special form of a single case study, called by Yin an “embedded single case study” or one that is divided into more than one unit of analysis (Yin 1994, 41-44). The choice of subunits flows from the model I have presented. This model rests on a context-rich, two-way dialogue between diffusers and potential adopters. When the dialogue is of a certain character, I argue, it will prompt a two-sided discovery process. The adopter’s learning—which is the subject of much academic study—will be matched by the diffuser’s, who comes to understand better the nature of his innovation, how it can be effectively described and how it can be appropriately transformed. The centrality of dialogue to this paradigm suggests these three substudies: one of diffusers, one of adopters, and one of the dialogue between the two.

Before turning to the substudies, one general note on the data gathering protocol. In qualitative studies of a largely investigative nature, the usual data gathering instrument is a semi-structured interview (Miles and Huberman 1984; Huberman 1990). Interviews, however, have methodological limitations for capturing meanings, understandings or construings because respondents react to the phrasings and presumed biases of the interviewer (Hutchinson, 1995). The protocol for both the diffuser and adopter substudies combined reflective writing—where participants were given a structured set of questions and asked to write their responses—with transcriptions of conference dialogue and semi-structured follow up interviews. This protocol has the strength that the different modes of data gathering balance one another—for example, while written responses may reduce some distortion they have the limitation that the researcher cannot probe the respondent’s answer. It has the weakness that it is idiosyncratic and is not keyed to any extant data collection tool.

that have taxed the management capacity of nonprofit organizations to such a degree that their programmatic
The sub-study of diffusers traced, from the perspective of the diffuser, evolution in how the PRI was understood, described, and used. Background and baseline data was collected in three waves: 1) reviewing Ford Foundation PRI files and other background materials; 2) defining best PRI practice from the perspective of Ford Foundation staff; and 3) observing Ford Foundation staff describe the PRI to other philanthropists.

The internal materials review included reading files of all open PRIs and also ten years of annual "Discussion Papers" to trustees. PRI files contained: proposals from recipients; staff memos on the strengths, weaknesses and promise of each deal; and staff assessments after the investment was placed. The Discussion Papers gave background for key policy issues facing the PRI division in each represented year. Discussion Papers were carefully prepared, often summarizing a significant piece of research into the PRI division’s practice. Topics included: Whether and under what circumstances equity grants strengthened the PRI recipient's chance of success? How effective are existing intermediaries at small-business financing, which is a high-risk lending activity? Assessing the relative viability of different community development ventures such as shopping centers, industrial sites and small-business incubators. This background information was used to shape interview questions and to allow the interviewer to be conversant in the examples that pepper any practitioner's reflection on his practice.

Producing an initial description of best PRI practice from the perspective of Ford Foundation staff was an iterative process that covered a six month period starting March 1998. As described above, it began between Jaffe and me. In our initial conversation, she offered a first cut at such a definition. This was refined through a series of three letters which I wrote, each iteration incorporating changes we had discussed over the telephone. The final version of this letter was given to all staff in the Office of Program-Related Investing and I interviewed each staff member as to how their understanding of best PRI practice hewed to or departed from what they had read. Out of these conversations, three examples of PRIs that embodied the staff’s cumulative sense of best PRI practice were
chosen and I wrote a reconstructive case-study of each. In two of the three (Neighborhood Services of America, Inc. and The Minnesota Nonprofit Assistance Fund), I was able to interview at least three parties to the deal and review written materials. The third study relied on the perspective of the executive director of the Carlsbad Community Foundation.

To learn about how Ford Foundation staff described the PRI to other philanthropists, I observed Jaffe lead two informational sessions on PRIs at the national professional meeting in the early spring of 1998 and read her file of previous endeavors of this type: a speech Jaffe had made to her local regional grantmakers association and the teaching materials and background memos designing an internal, professional-development session on program-related investing for the Ford Foundation’s international, grantmaking staff.

Finally, to trace the PRI staff’s evolving understanding of the PRI, the protocol combined reflective writing--executed at close of Regional Conferences by Jaffe and some of the other PRI staff attending conferences--and semi-structured interviews at the close of the research period with the three Ford staff who had attended the most conferences: deputy director Jan Jaffe, director Tom Miller and program officer Ellen Brown.  

The sub-study of (potential) adopters followed the forty-six participants in the first four regional conferences for a period of eighteen months. Clustering them into groups of "novices" and "initiates", I chart change in their conceptual understanding of the PRI and/or their instrumental use of the PRI. The baseline against which we measure change is drawn from their own, written pieces submitted prior to attending a PRI working conference. Semi-structured individual interviews were then attempted with all forty-six at approximately 8 and then again at 18 months following the conference (n=40). These interviews were conducted by Melinda Marble, a former program officer who was

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33 Ellen Brown took over as MIT’s lead contact when Jaffe was promoted to director of planning and staff development for the Foundation near the end of the research period.
known for her interviews of foundation staff about their untraditional approaches to providing support to charitable activities.

Complimenting these interviews are five mini case-studies. Four are retrospective studies. These were made of all participants in the fourth Regional Conference who reported a significant step toward adopting the PRI in their first follow up interview. The amount of data available depended on how far their efforts progressed. Where a PRI was made, interviews were conducted executive directors, board members, grantees, and, external consultants. Where the PRI was explored internally but dropped, interviews were more limited but always included the executive director and the external consultant.

The five mini case studies are rounded out with one process study. A foundation invited to the fourth Regional Conference but unable to attend, did attend a similar conference the following year. The retrospective studies were mid-process by then, and, when it was clear that the executive director of this foundation was moving quickly to adopt the PRI, I asked to be allowed to observe his process as it unfolded. I was invited to listen in on telephone consultations with external consultants and board members, to visit the foundation prior to a board training and interview staff and board members, and to conduct periodic telephone interviews thereafter.

The adopter data, while confirming many concepts in the implementation literature, offered little of theoretical interest, however. This substudy is therefore not included in the case. A summary of the adopter data has been attached in the Appendix to show that participating in the working conferences did have an effect. It shows both the proportion of philanthropists who enter the conference with no PRI experience (novices), who subsequently make a concrete step toward bringing the PRI into their foundation and also the proportion of philanthropists entering the conferences with some PRI experience (initiates), who subsequently expand their use of the PRI.

The substudy of the dialogue between the diffusers and adopters draws its data entirely from conference transcripts.
CHAPTER FIVE

HISTORICAL BACKGROUND FOR PRI CASE

Introduction

Were history a movie and we able to freeze its action on demand, the frame capturing the instance of a "go" decision to undertake an innovation would reveal a confluence of three streams: an idea; an opportunity--concrete or projected; and an organization whose decisionmakers had formed the judgment that the risks they perceived from innovating were tolerable and, in fact, more attractive than remaining at status quo. These three components--ideas, opportunities and organizations--offer perspectives through which to view innovation and through which we will look at The Ford Foundation’s development and use of the PRI.

The Ford Foundation’s Development And Use of the PRI, 1968-1979

The Idea

The idea of using investment instruments to support charitable work in the United States has a history of emergence, disappearance, and re-invention. The first American social investment was probably made in 1790 by Benjamin Franklin, who bequeathed seed capital for two revolving loan funds that provided assistance to young married artisans starting in business. In the early 1800s, a group of wealthy investors, limiting returns on their housing investments to 5 percent and working under the slogan "Philanthropy at Five," sought to show capitalists that decent tenements could be built. Later, in the early 1900s, the Russell Sage Foundation convinced a group of philanthropists to invest in a new "planned" community for middle-income home buyers. And in the same period, John D. Rockefeller, Jr. financed a Harlem apartment cooperative
that became home to many writers and other figures in the Harlem Renaissance. The Great Depression of 1929 apparently stifled experimentation with charitably-motivated investing, however. Thus, in 1968, when former deputy vice president of the Ford Foundation Louis Winnick hit upon the idea of making charitable loans, he was aware of no precedent (Winnick 1988).

In 1968, Winnick reviewed a grant request from a group seeking to train Black and Hispanic youths in carpentry skills by having them rehabilitate tenement buildings. Winnick, a reformer in the mold of Charles Abrams, had a strong interest in economics. Characteristically, his first question was "Whose going to own the building?"--wondering, he said, if it would be the youths or the nonprofit? Winnick noted that the project was going to create an asset that itself could generate revenue, a point that he wanted to bring to the attention of the grant seekers. Ultimately, Winnick supported the grant, but, characterizing himself as "stingy," he wondered if Ford could have made a loan instead (ibid.). This idea opened for him the broader possibility that Ford might initiate a low-interest or "soft" loan program for asset-producing ventures. Winnick was attracted to the idea primarily because of its potential for stretching philanthropic resources.

The late Sixties was a period when Ford and other foundation staff were keenly aware of their limited resources in comparison to the prominent social issues of their day (Ford Foundation Archives 1968). The Johnson Administration's Great Society programs, modeled on Ford's "Gray Areas Programs," were newly minted and reflected a growing consensus that poverty was a national problem (Magat 1979, 121). Moreover, there was a broad perception that America's cities were in crisis, and, for many, their viability was in serious doubt. According to historian Diane Ravitch, "The perception of

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34 This history of charitable investing was reported to me by Louis Winnick of the Ford Foundation in our May 1988 interview. A similar list can be found in Investing for Social Gain: Reflections on Two Decades of Program-Related Investments, Ford Foundation Report, 1991. A slightly different history, one that begins with Rabbi Maimonides in the 12th Century, is reported by John Simon in Foundation News, May/June 1981.

35 In a later interview, Winnick is quoted as saying that, from the outset, he was explicitly interested in making loans to minority-owned businesses (Ford Foundation Report 1991, 5). In our interview, however, his original intent was represented as making the most efficient use of philanthropic resources. Other evidence, described below, leads me to believe that McGeorge Bundy, President of the Ford Foundation originally suggested the business slant for Ford's PRIs.
the 'urban crisis,' discussed intensively in the popular press and in policy-making circles [in the early 1960's], stemmed specifically from the changing racial composition of the cities. The perception that cities were in crises was augmented by the deadly riot that broke out in the Watts section of Los Angeles in the summer of 1965, followed the next summer by riots in 43 largely Black inner-city communities; and in subsequent summers by progressively more violent riots. In 1968, the year Winnick began pursuing the possibility of a soft loan program, New York City was a focal point of national attention. That spring there was both a protracted sit-in on Columbia's campus and riots touched off by Martin Luther King's assassination (Ravitch 1983).

Staff of other foundations, operating in the same milieu, were experiencing similar pressures on their even more limited grant resources. The Taconic Foundation, a small, innovative foundation which had shown a willingness to step off the "well-worn, socially approved paths" with controversial work such as supporting militant Black organizations in voter registration drives in the South, had received a number of requests from groups seeking to start business ventures in urban ghettos (Bremner 1960 1988, 186). These groups had been unable to attract traditional sources of capital. In response, Taconic's president John Simon, a Yale law professor, began researching a means by which philanthropic foundations might invest a portion of their assets in such business ventures (Urrows & Urrows 1981). Simon's research, which he began in 1967, was motivated by both practical and technical concerns. On the practical side, the capital needs in the inner-city and rural areas where the poor lived were substantial and Taconic's grant resources (at approximately 2 percent of the Ford Foundation's (Magat 1979)) were certainly small

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36Ravitch supports this with the following statistics. "The black population in almost every major American city grew markedly during the 1950's and early 1960's. Between 1950 and 1966, the black population in the central cities nearly doubled, from 6.5 million to 12.1 million, growing from 43 to 56 percent of the nation's blacks. During the same period of time, the number of whites living in the central cities remained the same, but by 1966 had dropped from 34 to only 27 percent of all whites. With the black urban population rising sharply, residential segregation also rose. The newly arrived blacks settled in the poorest neighborhoods of each city... The rural migrants arrived with poor education and few skills at a time with the number of jobs for unskilled and semiskilled workers was rapidly diminishing (U.S. Dept of Labor 1967 in Ravitch 1983, 147)"
in comparison. To make a noticeable dent, Taconic would need to reach beyond its earnings; tapping the foundation's principal provided one means of so doing.

On the technical side, there were two issues. First, grants to for-profit business enterprises could be illegal. This was a more significant impediment in the late Sixties then it is today because the intervening tier of not-for-profit organizations--community development corporations and the national intermediaries serving them--was in its infancy, without either the coverage or the sophistication they have now. Second, at that time foundation trustees or managers' investment decisions were governed by the "prudent man rule," which, according to PRI legal specialist Jean Ericson, appeared to set prevailing investment practice as the standard, but in fact held trustees to behaving "more conservatively than the average investor" because "the rule focused attention on preserving the nominal value of principal and the avoidance of 'speculation'" (Council on Foundations 1991, 29).37 Thus, for a foundation to invest in projects that furthered a social goal, such as bringing employment to an urban ghetto, it needed a means to legally lay aside the stricture of maximum return on investments while arguing the merits of charitably-motivated, high-risk investments.

Ford's president McGeorge Bundy was aware of Simon's research into the legality of making charitably-motivated investments. Thus, when Winnick approached Bundy about the possibility of Ford establishing a soft loan fund, Bundy brought the two men together.

In interviews, both men stopped short of saying they collaborated in developing what is now known as the Program-Related Investment (PRI), but they apparently learned from one another, and later that year two "social" investment pools were formed (Urrows and Urrows 1981; Winnick 1988). Taconic formed the Cooperative Assistance Fund (CAF) which pooled dollars from a consortium of foundations (including Ford) in a

37Jean Ericson gives this definition of the prudent-man rule: "exercising the judgment and care, under the circumstances then prevailing, which men of prudence, discretion and intelligence who are seeking a reasonable income and preservation of their capital would use in the management of their own affairs." (Council on Foundations 1991, 29.)
$4.9 million fund whose scope was limited to business ventures in low-income and minority communities. Ford segregated $10 million of its assets—approximately 0.3 percent of Ford’s total assets, then valued at approximately $3.3 billion—for PRIs and gave the "set aside" a broad mission (Ford Foundation Report 1991, 7).38

In an internal position paper for Ford trustees, the mission of a proposed PRI program at the Foundation was explained in this way:

"The Foundation does not have nearly enough cash to meet all the demands on its agenda, so the program-related investments should be a way to stretch limited funds, as well as attract the funds of others to good projects. PRIs will arm the Foundation with a range of options for achieving its objectives—the outright grant at one end, something a shade less than a regular market investment at the other, and in between such devices as guarantees, low-return stock and bond purchases, and even interest-free loans." (Ford Foundation Archives 1968, 2).

The Ford Foundation thus brought forth a novel interpretation of the idea of charitable investment: other philanthropic investors had specialized in a particular sector (historically this was housing and, contemporarily, CAF limited its fund to business lending), but Ford sought to use the full range of this instrument. Ford’s novel interpretation manifested itself in the breadth of its initial investments. In the first two years, loans were made to ventures ranging from "cattle feeding, fruitcake baking, and steel joist manufacturing to fast-food franchising, publishing, public transportation and catfish raising" (Ford Foundation Report 1991, 7).

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38 According to Richard Magat, the average budget for Ford’s grants in the 1960s was $249 million; thus the set aside was equal to about 0.3 percent of total assets but about 4 percent of the grant budget.
The Opportunity--Gaps and Mechanisms

Legal Gaps

The opportunity for using this innovation had both a programmatic and legal aspect. Programmatically, against the turbulent backdrop of the Sixties, Ford staff were seeking new or expanded ways to bring forward philanthropic resources. In the impassioned phrase of Ford staff member and later student of this innovation, "The urgency and magnitude of the problems confronting America [in 1968]--poverty, racial tensions, and preservation of the environment--demanded that the Foundation, any foundation, stretch its assets to reach for real solutions" (Ford Foundation Report 1991, 6). Decisionmakers at Ford perceived a "performance gap," which, in the sense that Zaltman, Duncan and Holbek use this term, unfreezes elements in the organization and provides both an impetus to innovate and a softening of some of the social-psychological forces that usually check proponents of change (Zaltman et al, 1973).

The legal opportunity was of a different type and requires some additional background. In 1968, foundations were still governed by the Revenue Act of 1950.39 Under this act, a foundation was required to spend, either through grants or expenses, its investment income. "Investment income" was primarily dividends on stock holdings or interest on cash accounts and excluded capital gains or new gifts to the foundation. While required to spend "income," perpetuities were "bound by charter not to invade their capital" (Andrews 1970,126). And, as we have said above, the "prudent man" standard held trustees to a conservative standard for investment of "capital" or "principal."40

The "prudent man" rule was itself an issue for foundation fiduciaries, quite apart from the issues that concerned Simon and, later, Ford staff. Modern investment management called for diversifying an investment portfolio with regards to types of

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39 This legislation would soon by supplanted by the Tax Reform Act of 1969.

40 Trade articles of the period held that a 5 percent return was a reasonable return on investment [Andrews 1970,128].
holdings and to expected return. Thus, a manager practicing to the prevailing standard
would tolerate greater risk with a portion of the portfolio, seeking an "up side," a hedge
against depletion of the portfolio through inflation. Yet, as recently as 1965, the
Treasury Department had advocated restricting foundations to "commercial paper and
government-backed loans." As Jean Ericson wryly notes in her history of this period, the
prudent man rule "gave little comfort to the trustee or manager seeking to depart from
accepted norms" (Counsel on Foundations 1991, 29).

Foundation managers could derive "comfort," however, in the face of strict,
conservative investment standards and public distrust, from the fact that the 1950 Act
was almost without sanctions. In extreme cases, tax-exempt status could be removed, but
even here proving "private benefit" or "noncharitable purpose" was, in the view of the
Treasury Department, difficult within the existing law (ibid., 30). If trustees acted in
good faith and invested--even imprudently--with no personal motive, they faced no
personal or organizational liabilities from the IRS. Thus, opportunity lay not in specific
allowances within the law, but rather in the assurance that, should trustees take the
foundation in a direction that was later found to be problematic, there was no provision
for lasting legal or financial repercussions.

**Accounting Mechanisms**

The PRI program at Ford was initiated by creating a "set aside" of the
Foundation's corpus, with a ceiling of $10 million. The "set aside" functioned as follows.
PRI staff were authorized to commit up to $10 million in loans, equity investments or
guarantees. As these commitments were distributed, they were viewed internally as debt,
a portion of the total allowable debt of $10 million. When principal returned, it was
credited to the set aside (reducing the debt) and was available for new PRI commitments.
Any earnings on PRIs were returned to the corpus and those that met the definition of
earnings under the current law were distributed as grants.
This was not the only structure available to the Ford Foundation, as later became clear. In Louis Winnick's account of how and why he was inspired to contemplate a soft loan program at Ford, there is no inevitable logic leading toward drawing the funds from the Foundation's corpus. The group seeking to purchase the tenement was eligible for a grant. A soft loan program from earnings would satisfy Winnick's "stingy" impulse--what some may call a desire for efficiency in using charitable resources--because recycling grant dollars would stretch those resources. And, as there was a weak precedent for charitably-motivated investment of assets, so there was at least a weak precedent for lending earnings: some foundations were then operating small student-loan programs from earnings.

Where the structure Ford chose appeared to hold an advantage was for those foundations seeking to make investments in for-profit businesses--at least, that was the case made at the time by John Simon41. Such investments were attractive to Ford staff promoting the PRI innovation internally, as evidenced by the fact that, in the first two years, more than half of the PRIs went to minority-owned, for-profit business ventures (Ford Foundation Archives 1985). Moreover, as we shall see below, staff were attracted to the "symbolic importance" of using foundation assets and hoped that Ford would "serve as a model for other public and private organizations interested in devoting a portion of their assets to public purposes” (ibid., 2).

The Organization--A Foundation in the Midst of Strategic Change

The Ford Foundation in 1968 was an organization poised to take unprecedented risks, according to Richard Magat, then Director of Ford's office for Reports, and author of the most intimate book of the Ford Foundation of this period. Prior to this, the Foundation had been beset by both public and private controversy, in reaction to which it

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41 Some argue that Simon has overstated the problems with making grants to for-profits (transcript PRI conference #1).
had adopted a "cautious" approach to its grantmaking. We begin with a brief description of the Foundation's early years and then move into the period of transformation that directly precedes Ford's decision to adopt the PRI.

Public controversy came in the form of congressional investigations into the dealings of philanthropic foundations generally, and the Ford Foundation in particular. There were several congressional investigations between 1948 and 1965. In the late 1940s and again in the 1960s, these investigations focused on alleged tax abuses such as using foundations to safeguard dynastic control of business enterprises and forms of self-dealing.\(^{42}\)

The Ford Foundation was of particular concern to the 1948 congressional investigation conducted by Senator Charles W. Tobey of New Hampshire because, in that single year, Ford grew from a small family foundation into the country's largest foundation. This growth came through Edsel Ford's gift of 1.5 million shares of class "A" non-voting Ford Motor Company stock. When, in the following year, the senior Henry Ford's estate was settled, the Ford Foundation owned 88 percent of the Ford Motor Company's stock and its assets comprised one-third of the combined assets of all philanthropic foundations. The issue that arises from gifting non-voting stock is this: giving non-voting stock to a tax-free entity may reduce estate taxes to the point where heirs can continue to hold voting stock control. Such was the object of Edsel Ford's gift according to Berrien Eaton, who writes in the *Virginia Law Quarterly* of December 1949, "Without the very simple Ford Foundation--created by a document running only three double-spaced typewritten pages--the Fords' would clearly have lost control of the company" (quoted in Macdonald 1956, 42). The Ford Foundation held the 88 percent of Ford Motor Company's stock until 1956, when it began a divesting, a process that would be completed over the next eighteen years.

In between committees looking into the financial dealings of foundations, Congress initiated two probes aimed at ferreting out, in the parlance of the period, "un-American activities". These probes, ironically, took advantage of the increased public reporting that was demanded in 1948. The first investigation, chaired by Representative Eugene Cox of Georgia, was directed at the larger foundations and concluded without indictments, real or rhetorical. According the historian Robert Bremner, "The committee reported that in a few instances foundations had made grants to individuals or associations subsequently 'cited or criticized' by congressional committees, but that the general record of foundations was good" ([1960] 1988, 166). This committee had followed a decent process, gathering information through self-report and allowing both public hearings and rebuttal.

By contrast, the second investigation, chaired by Representative B. Carroll Reece of Tennessee, was a farce. In it, hand-picked witnesses gave their conspiratorial view that foundations aimed to foist socialism on the American people, and then foundations were given no public forum for rebuttal. The final report proposed, among other things, a federal law "to remove trustees who made grants to 'subversive' organizations" (Magat 1979, 32). To further publicize the committee's viewpoint, its general counsel published a rambling, propaganda-filled account of the proceedings. While several large foundations were pilloried in this book, the Ford Foundation was alone in receiving whole chapters devoted to its work (Wormser 1958).

The Reece Committee was sharply criticized in the press. Moreover, two members of the committee issued a minority report condemning the proceedings. Nevertheless, these investigations prompted the Ford Motor Company to take "a strong interest" in the affairs of the Foundation. And, according to Magat, "Especially before the public sale of the company's stock, and thereafter to a lesser extent until we disposed
of all our shares, some of our work drew strong objections from certain parts of the company" (1979,.32).

Under McGeorge Bundy, a popular member of the Kennedy Administration who was hired as foundation president in 1966, the Ford Foundation broke with its past caution. By this time, the Foundation had dealt with its image problem. It had distanced itself from the motor company through three public offerings of company stock. And, in a strategic public relations move, rather than adding to the foundation's corpus, all the windfall was granted in series of popular programs: 260 million to raise faculty salaries at private, liberal arts colleges; 200 million to nonprofit hospitals for improving community services; and 90 million in endowment grants to private medical schools (ibid.). Moreover, in 1962, Ford had launched the Gray Areas program, a popular program that had become a working model for the federal government's Great Society programs.

Bundy reorganized domestic funding priorities to focus on programs for disadvantaged minorities. The change was decisive. In 1960, 2.5 percent of Ford's grants were related to minority rights compared to 38.5 percent in 1968; close to $70 million was redirected in this strategic change (ibid.,154). Among the more controversial of these new efforts were grants for registering minority voters, direct support to legal groups such as the NAACP to support civil rights litigation and support for what Magat calls "the New York City school decentralization maelstrom" (p.29). The PRI played a supporting role to the strategic changes undertaken by the Foundation in 1967 and 1968, and, in keeping with this role, the vast majority of PRIs in the first three years went to funding minority entrepreneurs in starting new businesses.

These were problematic loans, however. Of the thirteen loans to for-profit businesses completed in the first three years, eight were lost entirely and the largest recovery on the remaining five was 49 percent. Loans to minority entrepreneurs

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43 Magat adds, however, that Henry Ford II, who was both Chairman of Ford Motor Company and a Foundation trustee, "steadfastly refused from permitting his obligations to the company interfere with his responsibilities as a Foundation trustee" (1979,32).
accounted not only for sizable loss in the early years, but, also for 36 percent of the program's losses over a total twenty-year period. Later, specialized intermediaries would have greater success with similar, but more targeted lending strategies—even though loans to inexperienced entrepreneurs locating businesses in areas where the poor live always carry a high risk of default. Ford spread its loans over such a wide array of different types of businesses that staff could not develop the expertise that would help them limit their losses. By 1973, it appeared that those with hopes for the PRI as an instrument to support Ford's new domestic agenda were sobered. New allocations to the set aside dropped off, as did loan commitments, and loans directly to for-profit businesses were halted. Over the first three years, loan commitments of close to $28 million were made—almost $10 million per year. In 1973, that level dropped to $5 million, and continued at near that level throughout Bundy's remaining tenure as president (Ford Foundation Archives 1985).44

The Foundation’s next president, Franklin Thomas, re-invigorated and re-organized the PRI program. The changes he made are the last step in our history of Ford's PRI program. But, before continuing this history, we digress and describe the regulatory context under which PRIs are made and upon which the Ford Foundation exerted important influence.

The Regulatory Context for Foundation Investing: the Tax Reform Act of 1969 and PRIs

The regulatory context under which the Ford Foundation began its experiments with PRIs changed significantly with the passage of The Tax Reform Act of 1969. This was a highly complex measure, forty-six pages of which were devoted to entirely to

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44Grant budgets also tightened during this same period. Portfolios were growing slowly as companies coped with high inflation and foundations were feeling the pinch of a new excise tax.
philanthropic foundations. This Act altered the fundamental notion that a charitable corporation was a perpetuity whose principal was inviolate. Congress in fact considered twenty-five- and forty-year restrictions on the life of foundations but, in the end, settled on more moderate measures. Key among these were the following. Foundations were required to distribute a "minimum investment return" of 6 percent of the market value of investment assets. Additionally, foundations were required to pay an excise tax or "audit-fee tax" of 4 percent of net income, which was defined as "interest, dividends, rents, royalties, net capital gains from the sale of income producing property, less deductions attributable to earning such income" (Weithorn 1970, 86).

These and many of the Act's other provisions were onerous to foundation staff. The accepted standard for a good return on investments at the time was 5 percent, thus, implicit in the 6 percent distribution requirement and the additional excise taxes on earnings was an insult to the integrity of the foundation's principal. Moreover, many in the field found an insult of a different kind in the Act's provisions. The President-emeritus of the Foundation Center spoke for many when he wrote "Passage of the Tax Reform Act of 1969, with its severe provisions affecting foundations, make it clear that many legislators, and a substantial part of the public, believe that nearly all foundations were established for the financial advantage of donors and many of them engage in dubious practices, with little regard for public interest" (Andrews 1970, 125).

In addition to the distribution requirement and excise taxes, the act imposed heavy sanctions on foundations, and, in some cases their managers, for rule violations. For our purposes, the most important categories of sanctions were two. First, foundations that were found to have distributed less than the "minimum investment return" by the end of the year were subject to a 15 percent excise tax on the amount not distributed. If not corrected within a specified period, the foundation was exposed to an additional 100

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46 Later, the minimum distribution requirement was revised downward to 5 percent and the excise tax on earnings was reduced to 2 percent.
percent tax. Second, foundations that were found to have made "speculative investments that jeopardized the accomplishment of tax exempt purposes" (initially under the "prudent man" standard but ultimately under the more flexible standard of "ordinary business care and prudence") could be taxed 5 percent of the amount improperly invested. Moreover, foundation managers who knowingly participated in such investments were also exposed to a similar 5 percent tax up to a maximum of $5,000. Program-related investments were specifically exempted from the definition of jeopardizing investments. But, if on review investments were found to fail to meet all requirements for a PRI, foundation managers would face both personal and organizational liabilities.

Congress added specific language to the act exempting PRIs from the "prudence" standard in response to McGeorge Bundy testimony, where he called attention to Ford's program-related investing and sought clarification as to whether the Act meant to permit foundations to continue investments of this type. A Senate amendment to the House Bill offered this statutory language:

A committee amendment, however, makes it clear that a program-related investment--such as low-interest or interest-free loans to needy students, high-risk investments in low-income housing, and loans to small businesses where commercial sources of funds are unavailable--is not to be considered as an investment that might jeopardize the foundation's carrying out of its exempt purposes (since the investment is classified as a charitable expenditure.) To qualify as a program-related investment, the investment must be primarily for charitable purposes and not have as one of its significant purposes that of deriving a profit for the foundation (quoted in Council on Foundations 1991, 31).

However "clear" congress intended to be about its support of investing for charitable purposes, the statutory language was vague and its examples narrow. At the
request of foundation representatives, congress clarified that PRIs could be used to meet the 6 percent distribution requirement. Rather than a pre-set notion of "charitable purpose" foundations won a more individualized standard where the regulations would look to "the relationship between a proposed investment and the particular foundation's charitable activities" (ibid., 32). And also, at foundation request ten common scenarios in which foundation's might make PRIs were included as examples of exempt activities.

In spite of these additional clarifications, PRI legal expert Jean Ericson finds that "Foundation trustees and staff considering the use of program-related investments, as well as managers of foundations with existing PRI programs, may still express uncertainty as to the legitimacy of the PRI investment vehicle. ...Continued uncertainty as to the permissibility of charitable investments may not be surprising in view of the historic lack of clarity in the law governing investment management generally by charitable corporations" (ibid., 29). Moreover, the growth of intermediaries, which aggregate funding from various sources in order to operate loan funds, and other changes in the social investment landscape were not anticipated by congress and find no mirror in the scenarios added to the regulations.

**Ford's PRI Program Evolves, 1979-1987**

In 1979, Franklin Thomas became president of the Ford Foundation. He brought with him direct experience with overseeing loan funds and a special appreciation for the role that PRIs could play in supporting community organizations. Thomas had in fact been a PRI recipient (though not from the Ford Foundation) in his previous role as president and chief executive officer of Bedford-Stuyvesant Restoration Corporation, a community development corporation.

Under Thomas, the PRI program was greatly expanded and reorganized. Together with the trustees, he set a target annual disbursement of $15 million. New staff were hired and, continuing a trend that had begun under Bundy, they tended to be professional
loan officers or have held other investment-related jobs—though most had led careers that took them between the public or nonprofit sector and the for-profit sector.

Originally, the PRI was conceived of as an investment activity, and, accordingly, the PRI division was situated outside of the program division. Under Thomas, the PRI was brought into the program division, and, initially, the set aside was eliminated in favor of an annual allocation. Under the new approach, the Foundation's corpus continued to be the funding source for PRIs but both principal and earnings repayments would be reabsorbed into the corpus. Structurally and functionally, the PRI was becoming more grant-like.

After a year under the new system, PRI staff asked that the set aside be re-established with the higher ceiling of $50 million. In their discussion paper to trustees for the 1985 annual meeting, staff gave this explanation of their request, made earlier in 1982. Staff argued that the new appropriation system, which did not establish a ceiling, raised a policy issue with an important operating consequence. Without a ceiling, availabilities were determined solely by the size of the appropriation, and, there was no built-in incentive to recover old PRIs or stay within the established guideline of a five-year overall maturity for the PRI portfolio. On the other hand, a ceiling applied to an earmarked portion of the corpus would discipline PRI activities by tying the level of availabilities to staff's decisions on the terms for new PRIs, monitoring paybacks on existing PRIs, and modifications of troubled PRIs"(Ford Foundation Archives, pgs.3-4).

The implied discipline of the ceiling was perhaps exaggerated as the ceiling was regularly raised. It was 75 million in 1983, 100 million in 1986, and 130 million in 1990.
Steady increases in the ceiling allowed a more vigorous activity than was supportable by the return on loans which carried low-interest and matured, on average, in ten years.\footnote{There are, at least theoretically, capitalizations that would be self-sustaining. In 1989, PRI staff estimated that a set-aside of 175 million would have sufficient return for the PRI division to both cover annual losses of 15\% and make new annual commitments of 15 million (Ford Foundation Archives 1989).}

A more enduring aspect of Thomas's reorganization was an administrative decision to use PRIs to meet the foundation's pay-out requirement. With this change, the Foundation began treating its PRI program as a part of its grant budget, but few outside of the Foundation's administration knew this. Explanations of how this was accomplished are necessarily convoluted. Technically, PRI funds were still drawn from assets and they were carried on the foundation's books as a special class of assets. But, each year the treasurer's office would ask the office of PRIs to project its activity for two years hence. Grant distributions were then managed such that, with the PRI activity, the Foundation met its minimum distribution requirement. Technically, Ford's PRIs were part of its assets but functionally they were part of its grant budget. In the words of the Foundation's treasurer, "By including the PRI component in our projections for meeting pay-out, we are eating into what otherwise would have been grants. If we did not have a PRI program, we would have to make more grants" (Baxter 1994, 127).

**Descriptions of The PRI, Mid-1980's**

We turn now to looking at how Ford staff described the PRI for purposes of diffusion. The first such description is drawn from the Ford Foundation's annual report for 1989. A philanthropic foundation's annual report is intended for a broad readership; it describes the foundation's mission and how that mission was interpreted in program support during the previous year. It also includes audited financial statements and, typically, instructions about how to apply for grants. Each year, the Ford Foundation's annual report includes a brief description of what PRIs are and a detailed account of that year's PRI activity. The 1989 annual report describes the PRI in this way:
"Foundations usually pursue their philanthropic goals either by managing programs themselves or by making grants to enable other institutions to establish and operate programs. In 1968 the Ford Foundation added a third mechanism called program-related investments (PRIs), which are usually loans, but may also be loan guarantees or equity investments. They are earmarked for investment in enterprises that advance the Foundation's program interests.

*The distinguishing feature of PRIs is that they are drawn from a portion of the Foundation's capital assets rather than from its earnings.* They therefore enlarge the Foundation's philanthropic resources. Since 1968, funds allocated for PRIs by the Foundation's trustees have increased from $10 million to $130 million." (Ford Foundation Annual Report for 1989:pg. 144, emphasis added)

In this description, we learn that the Ford Foundation has been making PRIs for slightly more than twenty years. PRIs are described as comprised of common financial instruments: “loans, loan guarantees or equity investments.” We do not learn anything about these financial instruments that would indicate that they differ in any way from their commercial counterparts. The broad range of such possible investments is limited, however, to the set of enterprises that “advance the Foundation’s program interests.” Finally, the “distinguishing feature” of PRIs is where they originate: the Foundation’s “capital assets” (corpus) and not its “earnings.”

A second and less removed venue for describing PRIs is provided by regional associations of professional grantmakers (RAGs). These associations offer professional development training. Ford Foundation PRI staff received a constant stream of requests to lead sessions about PRIs or sit on panels devoted to this topic. Jaffe estimated that, over a seven year period, she participated in twenty such panels and turned down close to this number. In fact, this constant stream of requests was important to Jaffe’s decision
To approach MIT about research on overcoming institutional barriers to making PRIs, as she explains in the excerpt from our interview below.

...everybody in PRI is always asked to give a speech either to the Counsel on Foundations or regional associations or sometimes within a foundation about program-related investments. And, when I first came to Ford, there were only a few program officers [in PRI] and so I tended to do a lot of that speaking, as did Barry Gaberman. And over time I started to notice that there was always enormous turnout for these--tremendous interest. But, as we got into them, people’s eyes would sort of glaze over. And, I never really noticed that anybody did anything as a result of making the speeches. After a couple of years, I started to refuse to do them. And then I felt like a bad colleague. ... I started thinking “Why was this so hard?” And I finally started thinking that it wasn’t that people weren’t buying but that we weren’t selling the right set of information. I noticed that people [were] ask[ing] the wrong set of questions. We would give these talks and then people would ask what the interest rate was and “What was the term?” Things that really are the least relevant aspects of making the PRI (1990).

In 1986 she began accepting speaking engagements again, but took a different approach to describing the PRI. Here is how Jaffe described the PRI in a 1986 talk to the New York Regional Association of Grantmakers:48

The hypothesis I want to test with you is that with PRI, THE MEDIUM IS NOT THE MESSAGE. Rather, program-related is the message that turns out to be the profound aspect of making these investments work and perhaps making them prevail. WARNING: understanding how to make an investment is

48 The quotes are drawn directly from Jaffe’s notes for her talk and not a transcript; the notes were not formed into complete sentences.
important--but it can be learned--or bought--and it has many of the same qualities as good grantmaking. …

PRI was designed by program people for program purposes--to expand assets within the foundation that are available for charitable purposes. To address community development needs that were not being underwritten by traditional credit sources. Hopefully to do things that would INDUCE lenders to take these credits in the future.

Next Jaffe recounted the evolution of PRI practice within the Ford Foundation. She begins by describing its early, dispersed lending, isolation from the “program side” of the foundation and concomitant losses.

Jaffe ends this talk by trying to be explicit, using her terms, “about what ways that PRI and grantmaking overlap.” She relies on the housing sector for her example. Neighborhood Housing Services was a well-established program that aimed to draw banks into lending in deteriorating neighborhoods. It included a “high-risk” loan pool, which was funded by foundation grants. Ford made a PRI to bring a secondary market (Neighborhood Services of America) for NHS loans into being. The PRI was a guarantee to back an insurance company in its first purchase of a packet of NHS loans. Subsequent purchases were made without benefit of the guarantee. Jaffe concludes by making a case for the importance of bringing grantmakers and PRI makers into dialogue with one another.

…When the opportunities for cross-fertilization are most meaningful--before all the money is allocated. [It is important to] meet early on with grant program officers, who are deep thinkers on their topics, [and] begin to describe places that PRI and grantmaking overlap. There are important symbiotic relationships to be developed among banks faced with deregulation, [Community Reinvestment Act] CRA actions, community loan funds and the low income housing credit. A PRI to one project--no matter how many units it might contain--will not catalyze the
relationship. If a foundation is to be involved in the low income housing debate, a combination of grant and loan funds must be channeled towards that effort—in a sense as part of one package (NYRAG speech, 1986.)

Jaffe’s intent here is perhaps vague, but it becomes clearer in light of her description of best PRI practice, our third description. In succession, Jaffe uses the terms “overlap” and then “symbiotic relationships.” I do not read her as saying that grants and loans can be used for the same projects, which would be one possible reading of “where PRI and grantmaking overlap.” Instead, her NHSA example suggests that she sees separate but complimentary roles for grants and loans: each has a distinct use. When used together, philanthropists can go beyond the limited results of a single housing complex and, instead, “affect the low income housing debate.” And, the best practice of philanthropy is to combine its various available tools to bring about systemic change.

Next we turn to the third description: answers to my question as to what constituted “best” PRI practice, a question I put initially to Jaffe and, later, each member of Ford’s PRI staff. Jaffe’s notion of best PRI practice rested on the bringing about of a fundamental rather than local change. She sees financial markets as the stage on which program-related investments act.49

At the low end of this continuum is the “unique deal.” Such a deal may be well crafted; it may have required ingenuity to put the financing together. But, it sits at the low end of this continuum because its effects tend to be limited to a single client and a single investment situation.

In the middle of this continuum are deals that you have called “packages.” “Packaging” implies a strategy for deal-making that can be standardized, repeated

49 The quote was written by me and approved by Jaffe as an adequate rendition of her definition of best PRI practice. This particular version was shared with all PRI staff.
and used by other foundations. Packages are important because they allow experience in one situation to be captured for more general use; they can be more efficient than unique deals in their use of skills and resources and they can affect a wider client base. Packages also have drawbacks. They can blind an investor to the unique features of a particular deal; worse, if used inappropriately, packages may be less efficient than a unique deal.

At the high end of this continuum is an investment that changes the *viability* of a class of socially useful investments. In trying to bring more definition to this last class, you used the [phrase] “changing the way a market place reacts to a type of investment.” Seeking greater specificity in your description, you turned to an example: Ford’s work with Neighborhood Housing Services of America (NHSA). In this case, Ford guaranteed Neighborhood Housing Services mortgages to a prospective purchaser. Ford’s guarantee encouraged the first large purchase of these mortgages and, since then, other purchasers have been willing to buy without such a guarantee (Kalyn Culler Cohen, letter to Jaffe March 9, 1987).

Jaffe’s definition of best PRI practice is similar to a commonly expressed idea in the writings of philanthropists, particularly those involved in setting up the professional foundations in the early half of this century. These writings often distinguished modern philanthropy from charity, where the former sought to “solve” social problems and the latter sought to ameliorate the suffering of individuals. Yet, in the context of PRIs, where very few foundations make PRIs regularly, this standard struck PRI staff as somewhat controversial. Was it more at the cutting edge of Ford’s practice than of the field? There was broad agreement among PRI staff, however, on the example that Jaffe choose: the guarantee to NHSA was widely viewed as among the best of Ford’s investments. Still, no
staff member was willing to endorse Jaffe’s extrapolation from this case to a set of criteria by which all investments could be judged.

As I have indicated, several of the PRI staff interviewed as to how their notion of best PRI practice hewed to or departed from Jaffe’s demurred because they feared that Jaffe’s definition would apply more to the Ford Foundation’s practice than to the field. For example, one program-investment officer said:

> Any initiative that comes under the rubric of the Ford Foundation--that builds in expectations/ resentments. Either people discount it because “the kinds of questions Ford has the luxury to think about are not the things that we have the luxury to think about… (Biggers, 1988)

Another makes a similar point:

> Looking for a systemic change comes from our culture here at the Ford Foundation. We are a national foundation. We, basically, have a culture that values systemic change (Arrick, 1988).

Most PRI staff came to our interview with a cluster of examples, which they may not have been willing to call examples of “best practice” but were certainly willing to characterize as “better-performing” projects. Yet, with one exception, the program investment officers I interviewed were able to offer only vague alternatives to Jaffe’s formulation of a theory of best practice. The exception was program investment officer Ellen Arrick, who stood out for her ability to clearly articulate her thinking beyond the concrete examples. I quote at some length from her April 1988 interview.

> Program-investment officer Ellen Arrick had prepared for our interview by trying to think of investments that met the NHSA standard Jaffe had put forward. She, however, “didn’t come up with any that I could point to as successful.” She then
described an example that might be close: Ford’s investment in the Jersey City YWCA. This “Y” wanted to build low-income housing on some of its property so it co-ventured with a for-profit developer-general partner. Part of the financing came from a syndicated pool of for-profit investors: the investors received a tax deduction and the project was able to lower its debt service since it had some money that came in without an interest burden. But, the investors did not buy their equity all at once, so the Ford Foundation PRI (at an interest well below standard construction loans) was used to cover construction costs and was repaid as the investors paid in. “Now it seems that everyone is doing it.” But, she reflected, this was not a “change in the market but a change in the way social investors invest in housing.” Moreover, she argued that it was harder to think about “this ‘market’ question outside of the field of housing because the ‘markets’ in the others are ill-defined.”

Arrick then offered an example from her portfolio which she felt could build a bridge between Jaffe’s formulation which she saw as tied to housing deals to “changing how a system works,” which might encompass deals in less market-like sectors. She gave the example of the Rensselaerville Institute, which was attempting to help small rural communities rebuild deteriorated water and waste-water systems that were out of compliance with the federal Clean Water Act. The people in these communities would be hard pressed to pay the increases in water rates that typically accompany such renovations, but, many had the necessary skills to rebuild the systems. Thus, the Rensselaerville Institute was promoting a self-help solution. Self-help, however, was “perceived as high risk so the communities [couldn’t] get conventional financing or float bonds.” The Ford Foundation’s loan helped capitalize a fund from which these communities could borrow to underwrite expenses such as the purchase of materials.

The Rensselaerville Institute’s objective was to help rural communities become more self-sufficient and to change how the state agencies approached these communities: to move them from an “enforcement agenda to being more helpful and proactive.” In recommending the PRI, Arrick harbored the hope that “it can build a track record and
attract conventional financing for this kind of project.” But, Arrick stressed, the focus on conventional financiers “was a distant goal” compared to the focus on the government entities.

Still, Arrick wondered if even this broadened definition of “changing systems” was too limiting. She turned to a different example: a recent PRI had been used to establish a loan fund providing capital to community health centers. This loan fund corrected an “irrationality” in federal regulations. The federal government would pay the debt service for renovations made to clinics but would not provide the capital up front. Arrick then reflected on this example. She wasn’t suggesting that “correcting irrationality” should supplant Jaffe’s criteria of “changing the viability of a class of investments.” Instead, she offered this: “some things are susceptible to change and some aren’t. We need to get better at knowing where to push and where to adapt.

In closing, I turn to a different approach to diffusion used by the PRI staff for the purpose of internal training. In 1984, Foundation staff, particularly those in Ford’s various international offices, observed a shift in their grantmaking toward support for non-agricultural employment and income generation projects (EIG.) In response, a five-day staff retreat was planned (in Dhaka, Bangladesh), where, in addition to reviewing the state-of-the art of income generation programs and learning from the cumulative experience of the disparate field offices, grantmakers sought to increase their skills in assessing the business-side of these ventures. PRI staff planned a full-day training for this purpose and developed several teaching cases--some generic and some based on actual projects. Jan Jaffe played a key role in the design of the training and its materials.

The training materials were designed as aides in teaching certain common skills such as how to put a business plan together, how to read financial statements and how to analyze cash flow projections. But, more importantly, they were written to induct program officers into the assessment process used within the PRI division, a hybrid practice which is not “soft” in its analysis but which is disciplined by its philanthropic context. These materials posed and attempted to address questions such as: “What kinds
of knowledge can one expect to gain from the speculative exercise of business planning? How is collateral likely to be used within the context of philanthropic venturing? How to use consultants?” And, more generally, “How to match financial needs [of projects] with Foundation resources;[and] how should financial assistance be structured to both increase the likelihood of financial success and achievement of programmatic objectives” (Ford Foundation Archives March 12, 1984).

It is interesting to note that PRI staff were not teaching how to make PRIs, they were teaching their assessment process: “Whether the projects are funded with a grant, a recoverable grant, a PRI in the form of a loan, equity or guarantee is less important than whether the revenue and cost assumptions are realistic and they are well structured from a business and/or financial standpoint” (Ford Foundation Archives February 27, 1984). In fact, the PRI division had concluded that international lending entailed additional layers of complexity—both legal and cultural—and was researching (for its own use) the possibility of using recoverable grants in the international context (Ericson, 1988.)

The goal that PRI staff set for themselves in the training—that participants would learn how to structure financial assistance “to increase the likelihood of financial success and achievement of programmatic objectives”—expressed a different notion of “best” or “ideal” practice than was framed by Jaffe above. Yet, as we shall see, this was close to one aspect of the definition of best PRI practice that emerged from the conferences. Jaffe’s original formulation of best PRI practice assumed this layer of effective practice, I believe. As the next chapter will show, once Ford staff enter into dialogue with adopters, it became obvious that it was important to articulate the dimension of “how” in addition to the dimension of “why.”

From the perspective of the PRI diffusion program, the most interesting thing about these training materials is that they were never mentioned. I discovered their existence at the close of my research in a concluding interview with Jaffe. In this

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50 I was saddened by this late discovery as I found these to be an elegant set of training materials and felt they would have been a real asset to our diffusion effort.
interview, I asked Jaffe to assess her initial hypotheses for the laggard adoption of the PRI in light of what she had learned through participating in the working conferences. She saw the NYRAG speech as pivotal, since it expressed her analysis that description was at issue and showed her new approach to describing PRIs to grantmakers. In response to my request for notes from other speeches, which might allow me to compare her earlier and later descriptions, she looked and found none on file. In that moment, an after-thought really, she remembered the Employment and Income Generation training. She, nor anyone in the New York office had kept any of the materials, but, the Ford Foundation archivists located a copy of the workbook and memos on the training’s design in Ford’s Dhaka, Bangladesh office.

The fact that these materials were considered irrelevant offers clues as to how Jaffe and the PRI staff framed diffusion. These materials were developed for training and for internal staff at that. These materials were not intended to convey “how to make PRIs,” instead they sought to convey skills of assessing businesses and structuring investments, skills that could be adapted for a grantmaker’s use. These materials were outside of their mental frame for directed diffusion efforts. Yet, because I hold the view that diffusion is best conceived as a teaching/learning process, the directed diffusion program for the PRI which I designed had much more in common with the PRI Office’s training for its grantmaking colleagues than it had with the panels on PRIs held at professional meetings for the express purpose of diffusing the PRI.

**Conclusion**

As we know from Ford’s treasurer, quoted above, the Ford Foundation counted its PRIs toward the pay-out requirement, even while drawing them from a set-aside of the corpus. The fact that PRIs were counted toward pay-out, however, was certainly not obvious from the various descriptions of the PRI made for purposes of diffusion. On the contrary, the Annual Report stresses that the PRIs drawn from the corpus expand the resources available for philanthropic support. Similar to our descriptions of the
Workshop Program, which emphasized the ways we built academic community among minority students at the expense of how we actually taught the mathematics, Ford’s descriptions emphasized the novel aspects of its practice. This emphasis projected an image of the practice that was more radical than, in fact, it was. How these descriptions changed as Ford staff entered into an extended, two-way dialogue with other philanthropists (and potential adopters) is the topic of the following chapter.
CHAPTER SIX

DEFINING THE PRI IN TERMS OF HOW IT IS COMMONLY USED

This chapter takes a bird’s eye view of the first four conferences. In many ways, these conferences were separate events: they were held in disparate geographical regions; drew participants locally--apart from Ford staff--and attempted to map the PRI practice of that particular group. Yet, these conferences were also links in a single, conceptual chain. The MIT conference team entered each conference with a careful statement of best PRI practice that emerged from the previous conference (or, in the case of the first conference, from my interviews with Ford staff), which was then subjected to a critical inquiry by the conference participants. Each of the first four conferences surfaced one or more ideas that were later incorporated into this evolving description of “best PRI practice.” The chapter traces this evolution.

The description with which we are concerned has several components, but, each version begins with a deceptively simple question: What is a PRI? To give a sense of the magnitude of shift in the description of best PRI practice over the course of four conferences, I begin with answers to this question at three points in time: prior to the conferences, the end of the first conference, and at the beginning of the fourth conference.

From the PRI descriptions given in Chapter Five, we can distill an answer to the question: What is a PRI? Into something like this:

The PRI is an investment--usually loans, but also loan guarantees or equity investments--that advance the foundation’s program interests. In fact, similar to a grant, the fit to a foundation’s program interests is THE most important consideration in making a PRI. The feature which most distinguishes a PRI from a
grant is that PRIs are drawn from a portion of the foundation’s capital rather than from its earnings.

At their best, PRIs can deepen a foundation’s ability to address a social problem because it provides the foundation with two, complimentary tools, with different capabilities. A related, but distinct point, is that by virtue of being investments, PRIs have the potential of changing the viability of class of philanthropically meaningful investments. In this sense, the PRI can be an exceptionally efficient use of philanthropic resources.

The next step in the evolution of this definition comes from Donald Schon, the speaker in this next quote. He has culled from his notes of the previous two days work, quotes and images that pertain to defining the PRI. He opens by proposing a definition (which he invites people to “attack”). In this definition, he underlines the recent discovery that Ford uses its PRIs to meet the annual payout requierment and therefore is functionally (though not technically) taking its PRIs out of its grants budget.

For the purposes of getting the discussion started, I’m going to make a proposal to you: The PRI is a loan to a non-profit for “program-related” purposes which comes either out of corpus or out of income --though we discovered yesterday, out of corpus is a null set: there’s nothing in that category—

And, there’s the other things that we’ve said are NOT PRIs, but, they are other terms that are clearly in the ball park of PRIs. One of them is the term “social investment.” We’ve also used the term “recoverable grant” A “recoverable grant --vs- a PRI” or, a “expenditure-responsibility grant”... .
Schon concludes his “definition”—which is sliding away from defining the PRI and into describing a philanthropic practice that includes PRIs—with two images. Each image was crafted by a participant in an effort to convey what they were having difficulty stating expositorially. Schon states:

Two images have come up that I was able to cull out of the notes. One was Sue’s. Which was that here’s a “spectrum” which on the left you’ve got grants and on the right commercial loans and then there is this intermediate space where PRIs sit along with certain other tools.

Now, George had a similar kind of image, but not quite, which is this. Here are grants with our “grant constraints”, like for example we are limited to $100,000. And here are bank loans with their constraints like they are asking for collateral. And here is a space or gap in between where PRIs can be used to bridge the gap.

Thus, between definition one and two, there is an obvious shift in where the funds are said to originate. In definition one, PRIs originate in the foundation’s corpus. Whereas, in definition two, PRIs may be drawn either from the corpus or income—though, surprisingly, this group’s practice resides exclusively in the later. These two definitions share the assumption that PRIs and grants are distinct tools—they each have a unique, yet complimentary, niche. The definitions differ, however, as to what this niche is. Finally, Schon points out that the conversation has lifted up several “things”, these things are grants not PRIs, but, they accomplish PRI-like outcomes.

After this definition emerged from its “attacks,” it was determined that, in fact, there was not a unique niche for the PRI. In every instance where one could use a PRI, one could also use one of the “things.”

By the fourth conference, the MIT/FORD team had crafted this “broad functional definition of the PRI.” It is phrased in two, different ways.
[PRI’s are] a method of providing support to an organization [that’s] consistent with program goals and involves the potential return of capital within an established time frame. [Or], a lot simpler: A PRI is a recyclable grant.\footnote{Melinda Marble prefaced this definition by saying that this is not the only possible definition of the PRI. It fact, this definition clearly didn’t capture all the ways in which PRIs could (or have) been made. It did, however, conform to how most foundation’s participating in the conference—including all of those with substantial portfolios—used the tool.}

The gap between the first and the third definition is significant. Such a gap provokes the question: What has led the diffuser to see the PRI in a new way? While such conceptual shifts can be almost instantaneous—an “ah ha” experience—such was not the case here. This new conceptualization of the PRI was, comparatively, glacial and evolutionary. It came about through the accretion of the many examples of PRIs brought into the conferences by participants and the rested significantly on the well-documented, yet counter-intuitive argument presented by a participant in the second conference that the net gains from drawing PRIs from the corpus verses drawing PRIs from the earnings were relatively small. In the remainder of this chapter, I detail the ideas emerging in the first four conferences that aided in the reconceptualization of the PRI.

Two critical shifts to emerge in the first conference have already been discussed: that Ford counted its PRIs toward its payout requierment and that there was no unique niche for the PRI. I will not discuss these further except to say that the later should not be taken to mean that there were no differences between using PRIs and using grants. Instead, the import is that the merits of using a PRI do not rest on its providing an exclusive entry into some endeavors. Participants, however, saw that the PRI had distinct advantages over a specialized form of grant. A significant advantage was one of clarifying the transaction: the PRI, which brought with it the connotations of loans, and, investments, signaled that the borrower was entering a relationship with the foundation that was different from a grantee’s.
The relational aspect of PRIs is the final point that I draw from the first conference. This was a key learning mostly for the financial consultants on the facilitation team. Apparently, they viewed the PRI as an instrument whose structure was entirely determined by financial concerns. The consultants were strongly impressed, therefore, by what they named “the importance of context” in structuring PRIs. At the close of each conference, I commissioned a memo to explore any open questions raised in the conference. The importance of context in deciding whether and how to make a PRI was the topic of a December 22, 1989 memo written by John Weiser and addressed to the participants in the following working conference.52

...As one might expect, “creditworthiness” and “potential programmatic impact” were important to conference participants in deciding whether and how to make a PRI. I and other conference staff did not expect, but learned that “the context” of the request was just as important. ...an illustration of [what I mean by ] context occurred when the conference participants discussed whether to make a PRI to a battered women’s shelter. Creditworthiness and program impact were considered, but the most critical information (from the attender’s point of view) was the fact that the foundation wanted very much to support the shelter, but also was concerned that the shelter would be too controversial an organization to “get close to.” A low-interest loan was favored over a grant because it allowed support while maintaining a “business-like distance.” Again, a piece of context--in this case the public relationship between the two organizations--was critical in deciding whether to make the PRI.

As these examples show, PRIs frequently acted as a window through which to view the practice of philanthropy. As Don Schon succinctly summed, “I think it turns out,

52 These memos were one means of informing participants in up-coming working conferences of the salient questions that emerged in the previous conference. Thus, the discussion moved forward across the conferences.
most of us feel this conference has been about [patterns of practice]—that is, not only about PRIs but about: How do we do our work?"

The second conference, more than any other, was the impetus for reconceptualizing the PRI as a specialized form of a grant. The momentum came primarily from the well-researched and clear argumentation of Paul Lingenfelter of the MacArthur Foundation. More than a year prior to the conference, Lingenfelter had build a model to compare the effects on a foundation’s assets of an “all grant strategy” against various combinations of “PRI/grant strategies.” He had, in fact, modeled eleven such strategies and written a well-reasoned, if technical, paper which concluded that:

The term program-related investments (PRIs) sometimes misleads people to think of PRIs only as a below market investment option, something any prudent trustee would avoid altogether, or tolerate only in small doses. PRIs, however, are more properly thought of as highly leveraged grants, grants whose benefits greatly exceed the philanthropic resources they require (1997,441).

Lingenfelter concluded that, if PRIs were made from within the annual distribution requirement, over a twenty-year period, they increased both the foundation’s permanent assets and the dollars available for charitable distributions significantly. For example, if 10% of the annual distributions in year one were made as PRIs, over twenty years the pool would grow between 9.4% and 14%. (Precisely how much depended on the assumptions about PRI yield and loss rate.)

More surprising, however, was that if PRIs were made in “excess of the payout requirement” (a different way to say “from the foundation corpus”), the erosion of the corpus was much smaller than a simple comparison of PRI returns to regular investment returns would suggest. This was because PRIs carry a substantial advantage over regular
investments. Annual distributions are calculated as a percentage of the foundation's assets—the lion's share of which is its investments. PRIs, however, are excluded from this calculation. Thus, to calculate the actual PRI returns, one needs to add an additional 5% to the interest rate charged borrowers. The net effect is this. Suppose the usual return on standard investments is 10% (which it was in the late 1980's) and the usual return on PRIs is 3%. Add 5% (in annual distribution charge which is not applied) to the PRI's return of 3% and the PRI's return is 8%. Thus, the actual gap is 2% (10%–8% = 2%).

Lingenfelter's paper, which had been submitted to an elite journal in his field had been rejected, offered the primary basis for the radical revision of the PRI's definition: The PRI is a recyclable grant. The paper found a receptive audience among working conference participants when it was included as part of the conference materials.

Whereas the idea of a foundation as a player in a web of relationships was a key organizing idea for the first conference, the issue of debt was an organizing idea for the second conference. This idea was expressed in a concern that PRIs contained within them a siren's song for foundations. Sophisticated program-related investors, of which there were several in this conference, were concerned that effective nonprofits were too highly "leveraged" with PRIs and, in need of "permanant capital" (i.e. grants.)

It was also expressed in the conclusion by participants that foundations had institutionalized certain conceptions of debt. This was most obvious among a cluster of foundations from southeastern Michigan, which had done no experimentation with PRIs. They noted that most of these foundations had been organized in the late 1930's, a period when debt was viewed as the antithesis of a charitable action. Conference participants felt that the patterns of giving established in the post-Depression era had continued in some ways into the present and dampened any experimentation with the PRI. We took this idea forward into the next conference first by commissioning a paper on risk and by building a unit on analyzing the risk of a particular deal into the debrief of the PRI simulation.

53 It was added to the packet of materials mailed to each participant and finally published in a technical manual compiled by the current director of the MIT Project on Social Investing Dr. Christie Baxter.
The third conference was dominated by the question: What is the true opportunity cost of a PRI Program? This question was driven by a program officer and finance officer pair from a single foundation. The program officer was an expert in the area of low-income housing development and saw the PRI (which could be much larger than a grant) as critical to scaling a housing program to where its impact would be noticeable. The foundation's administration was on record as being completely closed to PRIs (Marble 1989), but the program officer had apparently recently won a hearing on the idea. The finance officer made it clear during the conference that he saw PRIs as dangerous on two fronts. They were not "A" quality investments and the opportunity cost of taking funds out of the corpus could be significant. This pair's keen interest in this question meant that they asked it at a variety of points; and eventually were able convince participants that it was an interesting question which should be engaged more completely. At the close of the conference, MIT was asked to commission a paper on the costs of making PRIs relative to the cost of making grants. (A different question from that which Lingenfelter addressed.)

The paper showed that the costs varied significantly depending on the program strategy. For example, the Ford Foundation was a trail blazer in both its grant making and program-related investing. It saw its role as that of "an incubator," thus in its PRI program, "Ford often provided seed capital for new types of social ventures" (Brody and Miller [1996]1997,). By comparison, the MacArthur Foundation tended to try "to maximize [its] impact by supporting successful organizations to expand or replicate their programs," which, in the area of PRIs translated into "providing expansion capital for established intermediaries" (ibid.). The incubator strategy was more expensive--for both grants and PRIs--than the "expansion capital" strategy. PRIs certainly had more subsidiary costs than most grants--legal fees, consulting fees--but, since PRIs tended to be larger than grants, the overhead for distribution costs was not that different. The paper concluded that strategy was a more important determinant than the type of instrument.

The fourth conference was attended exclusively by small, community foundations. Many of the actual examples of PRIs brought forward in participant's reflections involved...
recoverable grants. This conference underscored the flexibility and usefulness of the recoverable grant, particularly for foundations that were thinly staffed. It also took up and discussed structural questions that had been raised but dropped in each previous conference around the legal issues that arise when the foundation’s assets are governed by trust law rather than corporate law.

By the close of fourth conference, there was a noticeable slacking in the quantity of change. The first four iterations of the conference had produced significant changes in how the PRI was described and also in the conference format; these both gelled in the fourth conference. This is not to say however, that conference discussions became standardized; each continued to reflect the interests and experiences of that particular group. It is to say that the foci were more often ideas that had surfaced before and did not produce a noticeable shift in the conception of the PRI.

Ford had actively participated in each of the changes the conference team made in the PRI definition or conference format—changes we made in an effort to represent the most common approach to PRI practice. Was Ford embracing this description as an adequate view of its particular practice? I put that question to PRI director Thomas Miller in this way:

Int: In our last interview, you talked about [Franklin] Thomas’ hope to promote ‘balance sheet philanthropy’. As I have learned, this is not as simple as it might appear. In the Southeast [conference], you said that Ford’s PRI program is used to meet Ford’s payout [requirement]. In Chicago, Paul Lingenfelter made the argument that—and I think I have this right—that even if the foundation is making PRIs beyond payout and losing approximately 20%, assuming that it’s charging 3%, over time the foundation will reduce its assets insignificantly because of the interest [coming in] and the reduced payout requirement. Enlighten me. How important is it to take the PRIs metaphorically from the asset side of the Foundation’s balance sheet?
TM: I think it is real important. Back to this frontier that needs to be assaulted within the Ford Foundation: I did my own set of computer runs, in fact before Paul did his... The question is: what is the Foundation’s basic policy about management of its resources? There are two extremes. One is you maximize the growth of the foundation’s resources while not violating the payout rule. No matter what the stock market does you payout just what the IRS says you have to. On the other side of this equation is “let’s get as much out there as we can without endangering the foundation.” Perhaps the best definition of that is “let’s make sure we keep the corpus whole in real dollars so that we don’t devaluate through inflation. If you have 5% inflation and 5% payout, you have to earn 10% every year on your money to break even. You’re getting your money out, but you are not decapitalizing through inflation. There is a middle ground there, but, the latter is what I think the policy ought to be. The foundation’s goal should not be either to increase its corpus at the expense of putting money out there nor should it be to decapitalize itself by putting too much money out there. The basic rationale here being that there will be a long term need for foundations and their resources. The need is not greater now than it will be later…Therefore, what you say is that we want to keep this institution at the level it is forever. Everything else we pay out. If that is your philosophy then the PRI program needs to be made to a large extent in excess of payout. My calculation showed something like if 75% of your PRIs were made in excess of payout, you could do that without economic injury to the foundation at all.

Int: With what kind of loss rate?

TM: I think I was assuming 15% loss rate and I was assuming, I think, 2% interest earnings on the PRIs. There are lots of different assumptions in there.
The point is you could, to a large extent, make PRIs in excess of payout and get a lot more money out there without violating the fundamental principle that I’ve just nominated. It is not necessarily the Foundation’s philosophy nor have I heard it expressed explicitly, but it is at the very heart of what the Foundation thinks of itself. Therefore, it is a very hard question to ask and pay attention to. It takes a certain kind of air time to ask that sort of question. Honest and truly, it was at the top of my list at the point I decided to escape to Africa.\footnote{At the time of this interview, Miller had accepted a transfer to a field office in Africa where he would primarily be making grants.}

In other words, Miller did not see himself as having abandoned Ford’s “expanding philanthropic resources” frame; in fact, he saw this frame as central to the “foundation’s thinking.” But, as we shall see in Chapter Eight, moving between these two representations of his practice allowed him to notice that Ford was not in fact practicing across the full spectrum of recoverable investments: it was making very minimal use of the recoverable grant.

CHAPTER SEVEN

LEARNING ABOUT DESCRIPTIONS THROUGH DIALOGUE

Introduction

I have stated that diffuser’s descriptions tend to be skewed or incomplete in these three ways:
1. Diffusers emphasize the novel and omit the routine aspects of their innovation. Their work, therefore, may appear to be a more radical departure from traditional practice than in fact it is.

2. Diffusers describe aspects of their work that are formulaic or technical, leaving out those aspects of their work that are difficult to make explicit because they either require a great deal of contextual background or because they are patterns of practice which are partly or wholly tacit.

3. Finally, and quite appropriately, diffusers describe their innovation in its original context. Yet aspects of the innovation that are critical in the original context may be less critical in other contexts. Thus, diffusers descriptions may “fix” aspects of the innovation that will prove mutable.

In earlier chapters, I have showed how our initial descriptions of the Mathematics Workshop Program had the issues listed above. I have also shown that Ford’s descriptions were also skewed or incomplete in these ways. To recap:

1. Ford emphasized that it’s funds for PRIs were drawn from a set aside of the corpus and omitted a subsequent decision to count the PRIs toward the annual pay out requirement.

2. Jaffe describes her discomfort when novices signaled that they perceived the complex analysis at the core of effective program-related investing as “setting the interest rate.” She tried to address this by emphasizing the importance of beginning the analysis of a PRI where one would begin the analysis of a grant: with the program. But, both her descriptions and other’s descriptions omitted a detailed discussion of their innovative practice’s core, which requires, for example, becoming skillful at a far more complex analysis of income-producing projects. Expert program-related investors are competent at analyzing deals from the perspective of both financial return and “social” and structuring deals to protect both. What this analysis or deal
design actually consists of cannot be made explicit in general; it can only be made explicit in relation to a particular deal, in a particular context.

3. Finally, I have shown that Jaffe’s, initial description of best PRI practice expressed to some degree her membership in the community of large, national funders. For funders from small, rural Southern foundations operating within a web of long-standing relationships, best PRI practice certainly included making fundamental changes in “social markets”—in a local version of the NHSA case, one of the participants had made a PRI that was credited with starting the first secondary loan market for student loans in the State of Mississippi—but, it also needed to address the more relational aspect of local philanthropy.

The purpose of this chapter is to show how, within the structure of the working conferences, these issues were discovered and addressed. Since Chapter Six has already covered points one and three, this chapter deals only with how the working conferences provided opportunites for the diffusers to convey the specifics of how they go about the work of program-related investing.

Making Knowledge-In-Practice Explicit

I turn now to a transcript that is taken from the second regional conference, which drew foundations from the Chicago area and Southeastern Michigan. This excerpt is from the middle of the day-long simulation. In the first part of the day, conference participants, in their role as the executive director and treasurer of the fictional Winthrop Foundation, have reviewed two requests for PRIs, one from an art movie house and the second from a African-American studio museum. Working in small groups, they have analyzed these two requests and chosen to move forward with the museum, a construction loan. The museum has been accepted into a government program that will cover the bulk of the construction costs, but only after the construction is certifiably complete. The museum
has committed itself to mounting a capital campaign for $440,000 that will fill the gap between the government’s contribution and the estimated construction costs.

The facilitator asked each group to brainstorm as many ways to structure the deal as possible. As this excerpt opens, the facilitator, in the spirit of ensuring a complete spectrum of structures, has asked whether anyone considered using a guarantee. A guarantee is a pledge of credit by the foundation as security on a loan that is a bank or other commercial lender make to a non-profit. The pledge of credit can be a pledge, where no money changes hands unless the non-profit defaults and the pledge is called. In some cases, the foundation may actually place the total amount of the guarantee in an escrow account.

Guarantees are important to discuss because novices and experts often see them quite differently. Novices tend to see a guarantee as quite attractive because they believe that a deal which includes a bank is safer, since the bank is experienced at evaluating loans. Moreover, guarantees are attractive to novices because they offer the possibility of making something happen without dispersing any funds. Experts, on the other hand, tend to see guarantees as risky.

In this transcript, three categories of speakers are identified. The “experts,” Ford staff but also practitioners with substantial experience such as Paul Lingenfelter who was local to Chicago, are in italic. The “facilitator” is labeled as such; and the other voices are in normal print.

Transcript

I was going to add one more sidebar to that, something T. and I talked about, whether or not assets can be used -- we all have cash flow when grants aren't paid, and so you keep X amount of cash in the bank all the time so your checks can draw on it. How many or has anybody used that kind of money -- let's say it's two million dollars that's always sitting there as a guarantee? It doesn't move anywhere, but the bank knows that when it makes a loan to the museum, that there is this two million dollars sitting in the foundation's checkbook that it could draw against if they didn't meet their obligation.
Guarantees have upsides and downsides. The upside is if everything works it doesn't cost you anything. The downside is that if it fails, you pay through the nose for something that's failed. That's not very attractive.

(Having succintly described what he perceives to be the issues with guarantees, the expert follows by describing the narrow circumstances under which he would consider making a guarantee.)

FACILITATOR: Right, so it leaves a very bitter taste if it fails. …

We never made a guarantee. I've been thinking about it now for a project. I almost think the best time to make a guarantee is only when you're absolutely sure that the banks are being absolutely irrational about taking the risk of the loan and it won't happen any other way. The other thing about a guarantee is if you try to meet a distribution requirement, if that's an issue to you, your guarantee is worthless.

When you say the banks are being absolutely rational...

Irrational about not accepting the risk of the loan.

So you're thinking the risk is something they should accept and (unclear)

Right.

But it's really a bankable loan.

(A few more people state that they are puzzled about why a foundation would make a guarantee. The facilitator, one of the financial consultants on the MIT team, answers.)

FACILITATOR: You could lower the interest cost of the loan to the organization without any actual cost to yourself because [the guarantee makes] it a more secured loan.

(Here the conversation shifts from an ‘expert-participant” diad to a more open dialogue with several participants describing relevant aspects of their experience.)

Right, so, instead of having 18 percent they might get a 13 percent or a whatever the numbers might be, but they may be able to significantly lower the cost during the construction period because it's not just the studio museum and it's not just an abandoned building, but it's also the Winthrop Foundation was in this. [Unclear] Just to share our experience as what you're talking about here is that we had two similar situations. In one case it was a capital campaign. We guaranteed the
amount and the amount was lent from a commercial bank to the organization that was doing the capital campaign, so they had the use of the money and we guaranteed the loan from the bank in case that was not forthcoming. In another situation, we guaranteed the principal and interest repayment on a bond ten years out and that guarantee allowed them to sell the bond to an insurance company.

(Here the questioner is a participant who has not made a PRI, but who is financially trained. He reveals through his questions that there are ways to limit the Foundation's exposure, which may not have been considered.)

In each case was there a cap, then, on your guarantee?

No.

So that would be the other point of danger

That was recognized going in, yes, and you could project that with the interest and the bond repayments.

That one you could.

Well, in the guarantee on the campaign you knew the amount of the loan there, too.

I think the danger in this deal, though, might be that if you guaranteed the construction loan in the way you posed, it's a blank check. Because construction overruns are undetermined. And you know there will be. I mean, that's the only think I think for me that would be a given -- there will be overruns. ...

(One of the Ford PRI staff piggy backs on this discussion of the "dangers of guarantees" to make a more general point that working with banks is not a replacement for the foundation doing its own due diligence on a loan--a teaching point that was made in almost every conference.)

*One of the drawbacks of guarantees and against the customer's relationship with the bank is whether you're sort of removing some of the pressure on the bank to do due diligence. One experience we've had is that with guarantees is we get junior loan officers on a deal who often aren't very sophisticated about monitoring construction projects and the contract gets out ahead of them before they realize it and the guarantee kind of serves as some comfort to the bank because they don't really have to go through any kind of due diligence. So I think it's a tricky --again, a close relationship with the bank you know exactly who the loan officer is and that*
might work, but there may be other situations where you really basically think they're going to guarantee, it may be you're better off just lending it yourself ...

What could have been learned from this exchange on guarantees? There is the succinct summary of why guarantees are risky—“if it fails, you pay through the nose for something that’s failed— and when one expert would use one—“to correct a bank’s irrationality”.

Within this exchange, we also see the hybrid nature of program-related investing. The expert holds that the effect of a failure is to lose money, but, the risk is paying for something that is a programmatic failure. This stands in comparison to the different standard that can be perceived in the philanthropist’s questions about capping the guarantee. The philanthropist, who has never made a PRI but is experienced in matters of finance reveals the standard he would apply: well-structured deals limit the foundation’s financial exposure.

There is a cautionary tale about how guarantees can induce banks to be sloppy—so think twice about using a guarantee because you assume the bank will substitute for your own careful analysis. But, this should not be taken as a blanket reason against guaranteeing loans. Instead, the philanthropist needs to be facile at seeing the uniqueness of each context.
CHAPTER EIGHT

WHAT THE DIFFUSER MAKES OF PARTICIPATING WORKING CONFERENCE DIALOGUE

Introduction

This chapter returns the attention of the dissertation to the diffuser and asks: What did Ford make of its participation in the working conference dialogues? The paradigm I proposed at the outset of this thesis, held that:

1. Effective description of the innovation is not self-evident, instead requiring dialogue with users to learn about deficiencies, skewings and potential transformations that may serve to adapt the innovation to a new organization.

2. Participating in such a two-way dialogue with users is likely to prompt changes in how the diffuser describes, understands, and uses the innovation.

Thus, we will ask whether Ford changed either its descriptions or its practice.

Descriptions

Prior to the PRI working conferences, in annual reports and in a brochure explaining PRIs, this phrase consistently appeared:
The distinguishing feature of PRIs is that they are drawn from a portion of the Foundation's capital assets rather than from its earnings. *They therefore enlarge the Foundation's philanthropic resources.*

In 1991, following the PRI conferences, Jaffe was the principal author of a small monograph about Ford's development and use of the PRI. This monograph opens with the above paragraph, except that where the PRI was once described as "enlarging" the Foundation's philanthropic resources, it is now described as "stretching" these assets--a term that was commonly used in the working conferences to describe what happens when loans are returned or dollars recycled in other ways: "The grant budget is stretched". This initial description is followed by a significant clarification in a later section on accounting procedures.

"Traditionally, foundations have pursued their philanthropic goals by either engaging directly in program activities or by making grants to enable other institutions to establish and operate programs. In 1968 the Ford Foundation added a third mechanism--the PRI--to its philanthropic tool kit. The PRI has become a valued addition to the practice of philanthropy by providing more resources and more flexibility to address Foundation goals. By making loans from the Foundation's corpus, a new philanthropic resource is created to stretch assets available to support charitable activities..." (pg. 3)

But later in the document, this clarification is made:

PRIs are funded out of a $130 million set-aside from the Foundation's corpus. ...Although PRIs are made out of the Foundation's asset base, any losses are funded out of grant-making budgets. When a PRI is made, a loss reserve equal to 15 percent of the PRI is funded from the Foundation's grant budget. Financial
performance of loans is reviewed quarterly by staff. If the condition of the PRI deteriorates, the reserve level is raised and the necessary funds are transferred from grant budgets to the loss reserve fund. Therefore, when actual losses occur, the amount needed to recognize the loss has already been added to this fund.

Repayments of principal on PRIs are credited to the set-aside and become available for new PRIs, but earnings are returned to the Foundation's corpus. *PRI disbursements are counted as qualifying distributions toward satisfying the payout requirement that the Internal Revenue Service imposes on private foundations.* PRI repayments increase the payout requirement just as would an unused grant returned to the Foundation. (Ford Foundation 1991, 10-11, my emphasis)

The monograph also succeeds in making some of the nuances of the practice available for the reader. The middle section profiles nine Ford PRIs. Each profile narrative gives some background on the borrowing organization, the reason it sought financing, and a summary of Ford’s investment, including the term, interest rate, repayment schedule, whether security was attached, and the schedule for Ford’s dispersing the funds. Once the structure of the deal has been presented, the profile lists the “deal points.” This list provides synopsis of Ford’s analysis of the risks in the deal and how the structure attempts to mitigate these.

**Changes in Practice**

Learning about the ways in which our descriptions of our practice miss or distort what we do is an important consequence of entering into dialogue with diffusers. But, the paradigm I have proposed holds that when a true two-way communication is achieved, the diffuser’s learning can be of a more fundamental nature. The diffuser can, I argue, come to understand his innovation differently, which is likely to prompt changes in how
he uses the innovation. The more fundamental shift occurred in this case. The focus of this shift is the recoverable grant. The person who experiences the shift is Director of the Office of PRI Tom Miller.

Miller’s conceptual shift occurred during the fourth conference, which was attended exclusively by executive directors of small community foundations. Staffing was tight for these foundations, which typically had between two and five staff members. Conversation in a group session had turned to using recoverable grants. Tom Miller joined in by musing out loud how Ford might use recoverable grants: there were some cases where loans had been considered in developing countries where recoverable grants might have been (more) appropriate. At the end of that session, indicating a fundamental shift in his thinking, he added:

‘You know, I think this is an important idea and that we should change the name of the project from the PRI project to something that said this is a project on recoverable strategies’ (Miller interview 1/3/92)

Next, Miller requested and was given a million dollar increase in his grant budget, which he would use for recoverable grants. He described this new effort in this way:

[The recoverable grants] would be budgeted and accounted for as grants but would be looked at as PRIs in terms of the due diligence and in terms of the monitoring. So the PRI office will be making--has already made one and is going to make several more recoverable grants in situations where the risk and uncertainty makes it inappropriate to use a loan. That’s obviously a very subjective sort of judgment, but it’s a valid concept, I think. They also tend to be smaller than the PRIs we’re making now. That’s a whole other discussion. But, anyway, this idea
of recoverability is now the idea. Whether its a PRI or a recoverable grant or a
guarantee or some other use of foundation assets, I think the key notion is
recoverability. Whether you call it a PRI or something else doesn’t matter. That’s
an important point of evolution within the way this foundation thinks about the PRI
program and its own toolbox so to speak. ...

What Susan [Berrisford, then a vice-president of the Foundation] said to the
trustees is that the Foundation should be experimenting more with recoverable
grants. We’ve never had an explicit experiment with that particular tool. We’re
not saying that this is going to be a window in PRI forever. We’re just saying PRI
is the right place to conduct this experiment. We might find two years from now
that we have an entirely different approach to using recoverable grants throughout
the foundation and that PRI is not necessarily involved in every one of them or
even most of them. So what we’re doing now is conducting an experiment that we
hope will be of use to the rest of the foundation around this notion of
recoverability (1/3/99 p19-20.)

What had changed? As is common with conceptual shifts, the change was not
easily translated into words. In response to my direct question, however, Miller
attempted to describe his insight.

INT: I left the working conference on PRIs for community foundations and the
last interview with the impression that the community foundation conference
stimulated you to think about making recoverable grants. Did it or had you been
thinking about [making] them before the conference?

TM: An important event in terms of that thinking.

55 This was said directly to me rather than to the whole group
INT: So you would say that it crystallized [in] the meeting, even though you had been thinking about it some before?

TM: Yes. What that meeting did [was] it really crystallized it. Not so much because other foundations were doing it, but it just all became clear at that point for unknown reasons.

Metaphorically stepping into the “continuum” frame we had built together (where drawing from the asset base was inconsequential), allowed Miller to see Ford’s work in a new light. If he accepted the notion that there was a continuum from grants to investments from assets, then Ford’s practice had a gap in one place along that continuum: recoverable grants. Moreover, the conference had highlighted a transitional quality to recoverable grants—they were explicitly a “grant” and possibly a loan—which made them a comfortable first step into the territory of “recoverable strategies.” Miller saw in this an opportunity to move “recoverable strategies” out of the domain of the PRI office an into the whole foundation.

This possibility was realized when Susan Berrisford became president, but, likely differently than Miller had imagined. When Berrisford reorganized the Foundation, she eliminated the Office for PRIs. Program-investment officers remaining with the Foundation were integrated into the regular staff, where they might support projects with grants or the range of recoverable instruments.
CHAPTER NINE

ATTENDING TO DESCRIPTION: THE FOUNDATION FOR EFFECTIVE LINKAGE

I undertook the action research “experiment” described in this dissertation to test the approach to the pedagogy of diffusion that I and other staff members of that Mathematics Workshop Program had fashioned in the course of our attempts to diffuse that program. Within this broad goal, I specifically aimed to test my hypothesis that diffusers descriptions were skewed in these ways:

1. Diffusers emphasized the novel and omitted the routine aspects of their innovation. Their work, therefore, may have appeared to be a more radical departure from traditional practice than in fact it was.

2. Diffusers described aspects of their work that were formulaic or technical, leaving out those aspects that were difficult to make explicit because they either required a great deal of contextual background or because they were patterns of practice which are partly or wholly tacit.

3. Finally, and quite appropriately, diffusers described their innovation in its original context. Yet aspects of the innovation that were critical in the original context may be less critical in other contexts. Thus, diffusers descriptions may have “fixed” aspects of the innovation that would prove mutable.

And, most important, to test my assertion that by participating in a properly structured dialogue with (potential) users, diffusers would learn how to create more effective
descriptions--a process that would teach both diffusers and users about the nature of the innovation.

The diffusion literature offered the alternative hypothesis that the success of the Workshop Program’s revised approach to diffusion rested not on the pedagogical shift, but, instead, on having created the conditions for best linkage practice in practitioner-to-practitioner diffusion. Where linkage is defined as “multiple exchanges between researchers and potential users of that research at different phases of the study” (Huberman, 1990).

The action research case offers ample evidence that diffuser’s initial descriptions were skewed and incomplete in the ways that I listed. Moreover, it shows that the diffuser came to understand the essence of his innovation differently: Ford’s literature no longer portrays drawing funds from assets as an essential part of the practice. Thomas Miller, director of the Office of PRIs, reported that he came to reconceptualize his practice as the use of “recoverable strategies,” a conception which led him to propose and begin an experiment in making recoverable grants.

But, the concept of linkage allows for the possibility that descriptions may have the issues I describe and that linkage will still prevail.

The diffusion that came of the PRI working conferences—shown in Appendix A—however, happened almost exclusively without benefit of linkage, if one defines linkage as Huberman does.

Why was this amount of contact sufficient? It was because the descriptive issues in this case were so strong that once they were addressed the PRI was revealed to be a far less radical innovation than it had initially appeared. Recoverable investments could be drawn from income, so, from the perspective of the foundation, the financial risk was minimal—a default simply produced a grant. Moreover, recoverable strategies could be employed at different levels of sophistication and scale. Adopters in this research program began loan funds which varied in size from $5,000 (a fund that made cash flow loans to artists awaiting the distribution from an awarded government grant) to $6 million.
(an investment in a national housing intermediary.) These qualities—minimal organizational change, low risk, scalability—are all associated with innovations that have higher rates of diffusion.

The strong findings in support of my assertion that diffusers may inadvertently give incomplete and, in some cases, misleading descriptions of their work mean that little was learned in this study about the linkage. There is some evidence that weighs in against the effectiveness of doing linkage through a series of conferences, however.

The original diffusion plan was designed with the idea that people would need more than a two and one-half day conference to learn to use PRIs. We proposed working with a core of about thirty foundations over a three year period—offering first a regional conference; then, a year later, reconvening to study together how PRIs were being used within a particular program area such as low-income housing development; and concluding with a “problem-solving” conference, where the subject of the conference would be difficulties encountered in trying to use PRIs for the first time.

Three “issue” conferences were held, but, foundation executives treated them as an opportunity to train other staff in the use of recoverable grantmaking strategies. Without the same staff members returning, the conversation could not build on what had gone before, so the conferences became a version of the regional conferences—organized around program area rather than geographic region. The last tier in the original design, the “problem-solving conference,” was canceled because foundation staff resisted bringing examples of problematic ventures into such a public forum. Instead, an informal affinity group was established at the national meeting, where deals could be discussed “off the record”.

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56 After the research period ended, the Ford Foundation did create a version of the problem solving conference, but this was more in the spirit of a medical “Grand Round”. Ford presented a complicated, multi-investor case that had been extremely costly to negotiate. Ford invited philanthropists and intermediaries to review the case with the hope of building a shared understanding of the negative consequences of placing many, different demands on a single intermediary that is putting together a multi party deal.
Thus, given the current evidence, we need to view concentrated efforts aimed at building effective descriptions as a foundation for effective linkage, where the innovation is perceived as a radical, practice innovation.

Such efforts require more than simply gathering diffusers and adopters together in a room. This dialogue must be structured in ways that can help diffuser’s learn about their descriptions. The main components of such a structure are:

1. Potential adopters must be explicit about the ideas they bring into the dialogue, particularly: what they imagine the innovation to be and how they would use it. These are best if they are written so they can be read by others prior to entering into an interactive dialogue;
2. Diffusers, too, need bring a careful description of their innovation and how they actually use it. These descriptions ought to concentrate on those features of their practice that they consider to be essential and buttress these with practice stories. To learn about the efficacy of these descriptions means that diffusers must make themselves vulnerable to the possibility that their descriptions of their own practice may be inadequate or misleading—or, more profoundly, that underlying assumptions they held about their practice may prove to be unique to their organizational type or history.
3. Finally, there need to be low-risk opportunities to practice side-by-side, be these real or simulated.

The data I have gathered speaks to the process for designing and executing these components, but this has been reserved for a separate piece.
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