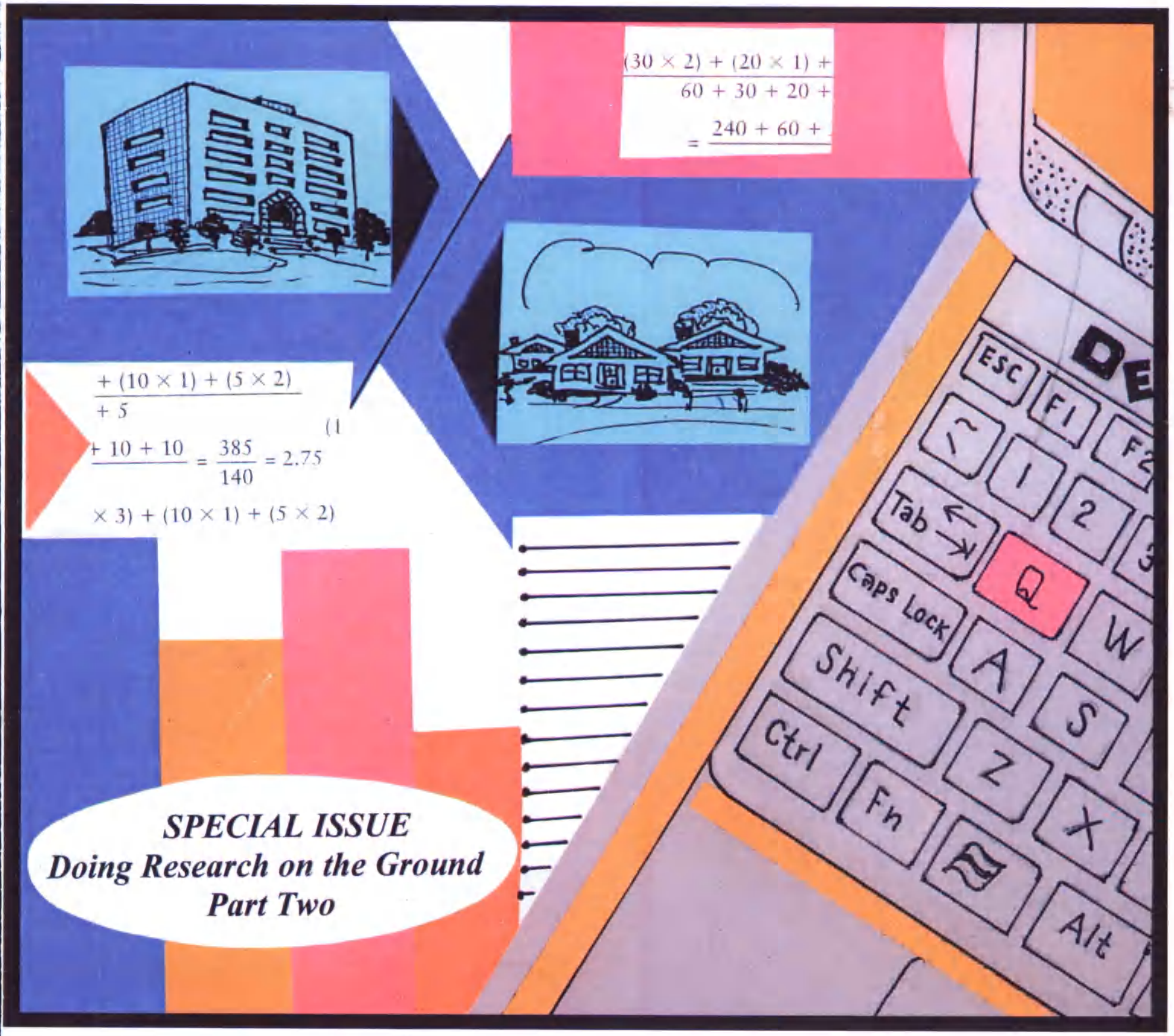
REFLECTIONS

NARRATIVES of PROFESSIONAL HELPING



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The Rewards, Frustrations, Challenges, Trials and Tribulations of Doing Social Work Research "On the Ground"

Part II
Issues of Courage, Collaboration, and Politics

Edited by

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REFLECTIONS
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Letter from the Editor

Jillian Jimenez, Ph.D.

This is the second issue on "Doing Research on the Ground," guest edited by Bill Meezan and Karen Staller. All the articles in these issues grappled with some of the most important and least discussed dilemmas in social science research. These challenges are markedly increased when the research is conducted by social workers who must consider the ethical implications of human research.

The authors in the first issue published in Fall, 2005, struggled with the complex nature of their relationships with the persons whose lives they searched and opinions they solicited. In this current issue the authors acknowledge, sometimes ruefully, the difficult environments in which they conducted their research. How do academic researchers manage the collaboration process with community partners? What happens when the results of the research do not meet the expectations of these partners? These are the previously unwritten stories behind the research that fills our academic publications. Similarly, writing with research partners who have considerably more institutional and professional power carries its own set of painful negotiations, ones that the unequal nature of the relationship can render exquisitely sensitive. All these back stories that accompany doing research on the ground are shared by the authors in these compelling narratives that contextualize their research.

The idea for the issues came from an outstanding researcher and great friend of ***Reflections***, Bill Meezan, who envisioned a way of looking at the research process that was imaginative and unique. I want to thank him and his co editor, Karen Staller, for taking a considerable amount of their personal and professional time to create these issues. ***Reflections*** has been greatly enhanced by their courageous work and by that of these authors.

Introduction to the Journal: My Journey with Reflections

Karen M. Staller, Ph.D.

In the first special issue of *Reflections* devoted to "Doing Research on the Ground," Bill Meezan wrote an introduction documenting his personal journey with this journal (Meezan, 2005). In many ways my own relationship with *Reflections* could not be more different than Bill's. Nonetheless, this journal shares an equally important place in my heart. I feel indebted to it. So, like Bill, I would like to take this opportunity to pay grateful tribute to those who created it, not only because their doing so provided a unique and welcome space for all of us to share personal experiences, but also because—on a personal level—it helped shape my scholarship and has influenced my career in the academy.

In the 1980s I worked at Under 21/New York, a shelter for runaway and homeless street youth in New York City, better known by its corporate name Covenant House. I first discovered the shelter as a recently divorced, second-year law student. At that time in my life I was craving personal freedom; having come from a nurturing, supportive, middle-class family in upstate New York I had never ventured far from security before and it was time to do so. Like Bill's experience in Lithuania, Covenant House altered my life in profound and seemingly inexplicable ways.

Covenant House was located on a desolate and lonely street. To get there from the "civilized" part of the city, you had to exit the seedy rear-entrance of the Port Authority Bus terminal, cross 9th Avenue, pass the spot where the Lincoln Tunnel deposits New Jersey commuter-traffic into New York City, and keep going until you reached the isolated edge of 10th Avenue. From this new and unfamiliar home base I was introduced to some of the youth subcultures that flourish in the city, but that most respectable folks merely pass by without notice. They included the young denizens who lived in the Port Authority Bus Terminal and Grand Central and Penn Stations; the kids who slept on the piles of road salt stored off the west side piers; the gay youth who hustled on the East Side "loop," and the transvestite kids who negotiated deals in the pre-dawn hours on the cobblestone streets of the meat packing district. During this period, I was tutored by a remarkable group of mostly resilient teenagers who had managed to survive (many with no family support whatsoever), moving in and out of the various private and public institutions that were supposed to care for them. Their finely honed survival skills stood in stark contrast with my own fledgling steps toward independence.

At some point, during these years, and for reasons that baffled me at the time, my parents gave me a Mac "classic," my very first computer. Unlike today's sleek and trendy models, this early version was a small, clunky, gray box with a pint-sized screen. I spent several months staring at the ugly little thing that cluttered up my studio apartment wondering what in the world I was supposed to do with it. I eventually turned it on.

Late one evening, basking in the computer screen's blue glow and the fluorescent light of my 55-gallon fish tank, with its comforting gurgling as background noise, I began to write. In the beginning, I believed I was writing the tales of kids who had particularly moved or inspired me. But as the stories accumulated I discovered that these writings were not so much about the kids as they were about me. I grew to relish the "ping" of my little Mac classic coming to life, which signaled each evening's solitary excursion into my reflections. With each new entry, I found myself facing the puzzle of why a particular youth had so captivated me. I was driven to re-visit our exchanges until the cadence of the child's voice I heard echoing in my head matched the rhythm of the words captured on the screen. So while Bill found a welcome place to process his provocative lived experiences in a conversation at Paul and Sonia Abels' dinner table, I retreated into this internal space and the process of writing to make sense of my mind.

Mostly the youth's stories, and my reactions to them, stayed private, although I occasionally shared them with a few friends. One of those was Stuart Kirk, an academic mentor and friend at the Columbia University School of Social Work where I had become a doctoral student. Like great mentors everywhere, Stuart is a cheerleader who nurtures creativity – no matter how quirky – whenever he sees it. One day Stuart said (as best as I can recall), "Hey I just got a call for papers for this brand new journal called *Reflections* – you really should send some of your Covenant House stories there." It was all the encouragement this doctoral student needed.

I sent off a cluster of three stories. Although it seemed miraculous that anyone would see fit to publish these personal essays, I got a "revise and resubmit" letter from *Reflections'* editor Sonia Abels (of whom Bill wrote in the last issue). So my very first published work appeared on the pages of this journal in its fledgling years submitted at a time when I was still very much in my own formative years (Staller, 1997).

In hindsight what is most remarkable is that it was this experience with *Reflections* that set me firmly on a scholarly path from which I have rarely strayed. I find myself using narrative and democratic approaches in *all* of my scholarly writing, and I argue passionately (and I hope persuasively) for their validity. I have experimented, like other post-modern researchers, with voice and style, but have permanently substituted first person for the "scientific" third person. I honor individual stories and occurrences that help unite us because they invariably speak to shared human experiences. I seek out the unique and worry little about "generalizing" despite my original research training. I don't know if this is a path I would have even known was available had I not found a successful home for my very early narratives in *Reflections*.

I am ever so grateful to Bill Meezan for his invitation to join him in co-editing these two wonderful special issues of *Reflections*. It was a gift, and has been a unique treat from beginning to end. Yet I can't help feeling that some greater force of destiny played a part in weaving our paths together on this project at this time, and afforded us the opportunity to discuss the rich and provocative narratives submitted for these special issues. We are both sitting at important new career junctures; Bill as a recently appointed dean and me just a stone's throw away from submitting tenure documents. Bill's first narrative appears on the following pages, my first one started me down a scholarly road. Bill admitted that it took a small act of courage for him to write in narrative style, I find it takes an act of courage to stay committed to it and not to submit to the dominant, quantitative, multivariate culture that is currently the altar being worshiped by most knowledge-builders in our profession. Nonetheless, for the moment, Bill and I have met at a crossroad while working on this project and I have learned a great deal as a result of it.

In 1997 I opened my *Reflections* essays by repeating the instructions I had received from a Covenant House staffer who provided directions to the shelter before my job interview. At the time he had told me that if it *felt* like I was heading in the wrong direction that I should not worry because it probably meant that I was heading the right way (Staller, 1997). Who knew that would be such a useful metaphor to resurrect again today?

A number of researchers who tell their stories on the following pages have stayed very committed to their work even when others might have advised them they were heading in the "wrong direction." It is heartening to read their tales and learn from their determination. The fact that we have this forum to do so is tribute to *Reflection's* creators, who saw the rich possibilities of providing a permanent space for this kind of voice.

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Introduction to Special Issue II
Courage, Collaboration, and the Politics of Research on the Ground

Karen M. Staller and William Meezan

The narratives in this second issue—like those in the first—deliver powerful lessons, both individually and collectively. The first special issue about "Doing Research on the Ground" spoke primarily to tensions in the practitioner-researcher roles; this one speaks predominantly about courage, collaboration, and the politics of doing research in real world settings.

Although courage is not a word often associated with social science research, it appears to be a common theme in most of the narratives that follow. Of course courage comes in a variety of forms. Its most obvious incarnation may be when one is faced with physical risk. But courage can also take the form of confronting one's own fears, or staying committed to a path with unwavering devotion in spite of pressures to do otherwise. It can involve maintaining one's convictions about how research should be conducted when other approaches might be easier to follow, or sticking by one's findings when stakeholders don't like them or the political consequences that come with them. It can be about confronting the internal dynamics of a team, or tenaciously standing by a set of collaborators, or staying in a research relationship when things just aren't going very well, or leaving such a relationship despite the consequences of doing so. Or it can be about finding ways to negotiate one's environment just to stay true to oneself.

Although common threads link some of these narratives in exciting and unexpected ways, they also fall relatively comfortably into small family groupings, and are therefore presented in this volume in that way. In the first quartet, the issue of *facing dangers and confronting fears in the field* is most immediately evident. We pair the narratives into two subsets: dangerous environments and unfamiliar environments. In the opening piece Newman (along with his research team) studies HIV risk and prevention with men who have sex with men in Chennai, India. It is an environment hostile to both the men who participated in the study and the researchers conducting it. Newman wryly observes, "It should therefore come as no surprise" that conducting such research "is a contentious sociopolitical undertaking." Lidchi also confronts physical danger doing her research in Columbia, South America on internally displaced families. "Persisting with the research required thinking realistically of the risks involved in the project and overcoming fear," she writes. She considers the necessary and unnecessary risks that must be taken when working in unstable and violent areas of the world, and relays the thoughtful, personal lessons she learned on her research journey.

In the second pair of narratives in this first section researchers note that it often takes conscious acts of courage to venture into unknown territories. Farkus opens her narrative with her personal mantra, "I have to get comfortable with being uncomfortable." In her thoughtful account of conducting research with women in jail, her recognition that she must confront her own discomfort in this unfamiliar environment, despite the fact that she is physically safe and that the "jail's primary product" is "security," is powerful. Thompson and her colleagues provide a rich and multi-faceted narrative on the "quagmires of conducting clinical research" that certainly goes well beyond just confronting the unknown. However, in one particularly engaging section of this narrative these researchers report on research that requires "entering communities that may be uncomfortable at best, and dangerous at worst." Thompson and her team wisely struggle with sorting out the difference between perceived dangers attributable to "internalized stereotypes" and situations that are "realistically unsafe," and offer some creative suggestions on how to respond to such situations.

The next family of narratives deal with the politics associated with producing and reporting research in a section we call *"Perfect" Science and "Relevant" Results (or the Politics of Producing and Reporting in Community-based Collaborations)*. This trio of papers opens with Weaver's narrative, in which he notes: "The cliché is that no one should be exposed to what goes into the production of sausages or legislation; the same can be said for the production of research articles." He then proceeds to expose us to just that, and in the process provides us with some particularly provocative ideas about the politics of reporting research results. Hostetter and Folaron, who were charged with the responsibility of doing "perfect research" by the state child welfare director, knew full well that "perfect research" was an impossible dream, and doing research under this expectation caused them to lead "research lives of quiet desperation." Nonetheless their research was enormously successful, as the state acted on their recommendations and made significant changes in policy and practice, in spite of the fact the results were the product of "less than perfect" research. Meezan and McBeath faced a very different, and almost contradictory, problem. Working as participatory evaluators, and fully embraced by the agencies with which they worked, they used the "perfect" design — a controlled field experiment — to answer research questions that all agreed were critically important to the way services were delivered. When the findings were not to their partners' liking, they found themselves shunned by those who had embraced both the study and the researchers; organizational learning and the development of best practice were jettisoned in the name of self-interest and organizational maintenance.

The final family of four narratives attends to *power, collaboration and "cold war"* (to borrow from O'Connor and Netting). The first pair of accounts in this last section deals primarily with power dynamics *within* research teams; the second pair deals with institutional and external influences on doing research on the ground. Of particular note in these final four narratives is the situational position of their authors and how it impacts the stories that are told. When O'Connor and Netting seek to make sense of their 20 years of experience conducting funded, collaborative research, they do so from their well-established place in the academy and from relatively equal positions. They thus have the distance and security to attempt to "normalize the experience in all its incredible peaks and valleys." The other voices in the collection of narratives are primarily those of untenured assistant professors and doctoral students, many of whom are working with collaborators who are more senior, and all of whom are in vulnerable positions within their institutions. These more vulnerable researchers do not yet have the distance to "normalize" their experiences, and thus offer their reports from the immediacy of their current situations. They also report on how they negotiated (sometimes successfully, sometimes not) the unequal power relationships in which they found themselves, and how these power relationships impacted their responses to the situations in which they found themselves.

In the first narrative of the final set, Staller, Buch and Birdsall use the narrative's construction to invite the reader to share the various perspectives of their team and to consider how these perspectives are influenced by issues of age, gender, status, university position, and power. Through this narrative, the reader gets a glimpse about how such issues impact both the internal dynamics of a collaborative research team doing community-based research and the way in which the members of the community relate to this diverse group. In the narrative by Jackson, Cameron and Staller, a doctoral student and an untenured junior faculty member who is the principle investigator of a research project struggle over almost everything about the research. So fierce are their disagreements about what occurred during the time that they worked together that an outsider friend had to be called in to moderate the telling of their story and respond to the editor's suggestions for revisions . Their troubled dynamics and the continuing tensions in their relationship are evident throughout the entire narrative starting with its two titles.

In the final pair of narratives in the volume, O'Connor and Netting reflect with great wisdom and insight on lessons learned about power dynamics from their rich collaborative experiences doing funded research over several decades. They warn us, "There are substantial power dynamics at play in team based research, dynamics one should never ignore or flee from. Recognizing and managing the power dynamics that accrue due to gender, discipline, university status, research design and methods will always be needed in team-based research." They further advise, "Team work can be difficult, tedious, and time consuming," but cheerfully admit "It can also be a whale of a good time." In the final narrative of the issue Crampton illustrates the point that doing research in real world settings, in a participatory and collaborative way, can be "tedious" and "time consuming," but argues that this carries particular risks for the untenured assistant professor. His tenacious commitment to doing research that is more "relevant than elegant," and the way he has found to do such research as an untenured assistant professor in a competitive, research-intensive university environment, makes for a compelling and honest narrative.

While these three themes – facing dangers and fears, the politics of research, and issues of power and collaboration – were useful in grouping the narratives for the purpose of their presentation in this volume, there were other ideas woven in and between the entire collection of contributions that the reader will confront. We consider just three of them here: flexibility and creativity in the field; the politics of production and publication; and institutional pressures.

Flexibility and creativity in the field

Thompson and her team note, "This research is not for the faint of heart, the unmotivated or inflexible, as so much can happen within the unstructured boundaries within which this work is conducted." While community-based research endeavors can sometimes lead to "dead ends," as Weaver discovered, more often researchers demonstrate a fierce commitment to keeping their projects alive. The writers of these narratives share war stories about the unpredictable nature of doing research "on the ground". They demonstrate that community-based, real-world researchers must be determined, flexible and creative in order to carry out their mission. Jackson and Cameron changed their fundamental research question mid-stream with the sudden, unexpected announcement that the school in which they had planned to house their study was shutting down. Thompson's team resorted to "bright pink" colored recruitment sheets in an effort to bolster low study enrollments rates. Hostetter and Folaron, "unaware of the pop-up nature of the case managers' email system," had to do damage control among angry case managers to continue their work. Most poignantly Newman reminds us that even more global events can impact this work when he writes "the *tsunami* brought home the realization that as much as I do my ethical best to protect our research staff, and myself, from foreseen trouble, there is much that is simply out of my control." In short, small and large crises, comic and tragic incidences, and bitter and sweet occurrences, are common in real-world research and necessitate the need for researchers who can respond flexibly and creatively in the moment.

Politics of production and publication

Another provocative theme, addressed with brutal honesty in a number of the narratives, has to do with the politics involved in producing publishable scientific knowledge. In an example from their experience, O'Connor and Netting report an attempt to explain that a project was in serious trouble to a program officer. The response was, "I know it will be hard, but I expect you [looking at us] to work with everyone here to figure out how to make this right. We're going to declare victory regardless of the results!" It was only then that they "realized that the program officer's reputation was on the line and that he had convinced a board of trustees to fund this multi-site, multi-million dollar project. It was not about us; it was not about the site; it was about the program officer's reputation. Our role was to figure out a way to redesign the project and cobble together in the interest of saving face, not doing good science."

Arguably even more troubling is the self-censorship observed by Weaver. "The strongest lesson for me was in the way we could not proceed even to publicly discuss our initial findings until we had conceptually re-framed these findings in a manner consistent with the values of the social work profession." Like cobbling together a research design based on saving face rather than good science described by O'Connor and Netting, Weaver reports, "Our re-framing was driven not by considerations of validity, but by the requirement for our research to fit with the prevailing intellectual and value paradigms of our profession." And the unwillingness to reframe or recast results, while it can lead to professional publications for the researchers, can also break bonds of friendship and years of trust, and close the door to future collaborations within the community. Meezan and McBeath note that when powerful people at the agencies with which they worked figured out that the study findings were being used to undo the experiment in funding from which they profited, and that the researchers were not going to change their results to meet the needs of the agencies, their agency partners shunned them. People stopped coming to meetings to hear about any further findings — "They just wanted us to go away."

If, as discussed above, flexibility and creativity are a hallmark of community-based research, honestly in reporting its disorderliness when it occurs can lead to additional problems. Crampton writes, "My impression was that article reviewers don't like things they don't recognize and many of them think community-based work is messy and foreign." Weaver echoes this sentiment, "Nonetheless, the finished product of the research process does obscure the messy reality of the process." The politics of publication take a different form in the collaborative work described by O'Connor and Netting in which researchers wanted to horde their empirical data for refereed journal articles which "counted" in their university settings, and were willing to write only data-free descriptive chapters for the agreed upon edited book.

Of course, the net result of the encroachment of politics into the doing and reporting of science is deeply significant because it impacts the "knowledge" that becomes available for consumption. Although writing of his own work, Weaver captures an idea that has much broader significance when he writes, "It is a sobering thought that the conclusions of social science research (perhaps even basic research) are always nested in the value paradigms of their professions, if not explicitly than through the kind of self-censoring in which we engage in this project."

Institutional pressures: Promotion and tenure

Finally a topic addressed directly in Crampton's work, but which is a recurring theme in a surprising number of the narratives in this collection, is an acknowledgment that most of the authors live their research lives situated in a university environment, which have their own institutional rules about membership (tenure). They are thus subject to consequences — either good and bad — depending on their institutions' responses. Doing research in the real world can be valued and the vicissitudes of this type of work can be understood; or it can be trivialized and its difficulties ignored.

Many writers in this volume address the lurking relationship between their research decisions and the tenure process. There are tales of successful and of failed tenure attempts. The untenured faculty represented in these narratives struggle with staying true to what they believe is good research while keeping a watchful eye on their "tenureablity". Others, securely tenured, look back at the decisions they made early in their research careers and note the relationship of those decisions to their tenure status. This raises important issues for the academy as an institution and about its impact on the shape and flavor of science. What are the incentives and disincentives built into the tenure process for doing research "on the ground"? What implications do these factors have for the kind of work that actually gets done? And how do these factors shape what we think we know – what we allow to be called "knowledge"?

Hostetter and Folaron write, "as we planned the methodology, we soon became aware of feeling pressured and torn in several directions. Our academic institution strongly emphasized the need for faculty to acquire grant income…Also, we both needed more publications for tenure and promotion." Weaver tells a tale of a career path that necessarily followed funding trails. Crampton, who sits on the other side of the tenure divide, reports advice from "one well meaning colleague" who suggested he stop attending time consuming community-based meetings but admitted that "the real barrier to isolating myself like that is I would feel lost. The reason I do this work is to change practice on the ground. Take away those long frustrating meetings in which we try to improve a big social service bureaucracy, and I would forget why I do what I do."

It is an important reminder. We must not forget *why* we do what we do. For some of us, *how* we do what we do keeps us honest and true to ourselves. But as these narratives illustrate, staying true to ourselves, in light of political and institutional pressures, often involves unrecognized acts of personal courage.

Challenges On The Ground: Conducting HIV/AIDS Research With Men Who Have Sex With Men in Chennai, India

Peter A. Newman, Ph.D., University of Toronto

This narrative describes the author's experiences designing and implementing collaborative research projects focused on HIV/AIDS prevention and care with men who have sex with men (MSM) in Chennai, India. Various personal, logistical, methodological, and ethical challenges in launching and conducting research with a highly stigmatized and vulnerable population in the developing world are addressed, as well as negotiation of these challenges. In particular, field research with vulnerable communities need not dispense with rigor, while at the same time maintaining feasibility and flexibility and the utmost protection of participants and research staff. Research is presented as a tool to support advocacy, health, and human rights.

On September 2, 2004, the Delhi High Court dismissed a legal challenge to Section 377 of the Indian Penal Code. Plaintiffs filed the case in June 2001 after police arrested four gay and lesbian rights workers…for conspiring to commit "unnatural sexual acts" and possessing "obscene material" which was reportedly safer-sex educational materials construed as pornography. (U.S. Department of State, 2005).

India is home to the largest number of persons living with HIV/AIDS in the world—over 5 million (NACO, 2005; UNAIDS/WHO, 2004) and counting. India is also one of five countries in which the U.S. government has deemed HIV/AIDS a national security threat (NIE, 2000; Singer, 2002). In this day and age, speaking in the language of security and terror often marshals more public concern than a discourse of human rights and suffering.

Albeit officially dubbed a "heterosexual epidemic"—and indeed millions of heterosexual men and women are affected—HIV/AIDS is also ravaging men who have sex with men (MSM) in India (Chakrapani et al., 2002). Two recent studies in India document high risk behaviors among MSM (Dandona et al., 2005) and elevated risk for HIV (Go et al., 2004). Many of these men are very poor. Access to state-of-the art HIV/AIDS medications is as unlikely as winning the lottery, so they get sick and die very quickly. And they often die without psychosocial support, having to hide both their illness and their same-sex behavior for fear of social repercussions so severe that they supersede even their fear of death, loss of family, employment, friends, community, and life as they know it. They also are often victims of harassment and violence. Yet, of the over 600 government HIV serosurveillance sites—locales in which the National AIDS Control Organization (NACO) conducts HIV testing to estimate the impact of the epidemic around the vast landscape of India—only three were designated for data collection among MSM (NACO, 2005). And so, not surprisingly, HIV/AIDS in India remains a "heterosexual epidemic."

One may not commonly think of behavioral epidemiology or sexually transmitted disease (STD) screening as a sociopolitical endeavor—much less the realm of one trained in social work. Surveys and in-depth interviews are not commonly perceived as tools for social change. Nevertheless, the need for culturally competent interventions that address contexts of risk has been well documented in HIV-prevention research, for example, among South Asian (Ratti, Bakeman & Peterson, 2000), Black/African American (Peterson, Bakeman, Blackshear, & Stokes, 2003), Latino (Diaz, 1998), and White (Seibt et al., 1995), MSM in North America, as well as one study among MSM in India (Asthana & Oostvogels, 2001). Targeted investigations such as these are needed to provide an empirical basis for culturally tailored interventions and to advocate for their being funded. Epidemiological and social behavioral research can be powerful tools to support the design and implementation of targeted interventions.

Epidemiological and sociobehavioral data, or lack thereof—as in the omission of MSM from government HIV/AIDS statistics—may construct one reality; but, it may also be used to provide sites for contestation. It should therefore come as no surprise that conducting HIV risk and prevention research among MSM in India is a contentious sociopolitical undertaking. We are met with challenges at every step—which continue to surpass anything I had envisioned; thankfully, so do the dedication and tenacity of my colleagues and our research staff.

India

India, as a developing country, presents challenges for anyone raised in the countries of the developed North. After five years and a dozen sojourns across swaths of the vast Indian terrain, I have become accustomed to obstacles presented by intermittent natural disasters and a tenuous infrastructure. We, in the North, take for granted the electricity, heat, and hot water almost invisibly pumped into our homes much less abundant, clean food, drinkable water, indoor plumbing and a fairly reliable public transportation and highway system. As one who has cursed out the New York MTA for 20-minute subway delays and re-routings of the E train on the F line, decried two-plus hour delays on the congested freeways of Los Angeles and the streetcars of San Francisco with the constant detachment of the overhead power lines, I never dreamed that what I thought to be the 3-hour-late Friday noon train to take me on the 18-hour journey from North Bengal back to Calcutta was in fact the noon train scheduled to leave the day before, on Thursday. My train would arrive in another few hours—if we were lucky.

I got "smart" on the next trip and we rented a four-wheel-drive jeep cum driver; however I had made a huge presumption—that there were in fact roads.

Not so.

My Indian colleagues explained that each year monsoon damage wreaks such havoc on the roads that they require more repair than is possible or affordable, compounded by the problems of government corruption that waylay funds. Yet, after a few trips these natural disasters and unreliable infrastructures became woven into my expectations.

Technological salves help a bit. My two-pound, battery powered, wireless laptop printer obviates my reliance not only on the availability of local hardware and ink, but on electricity. On one of my trips in rural North Bengal, I had deliberately scoured the surroundings and located a shop that would print from disk, replete with a giant Xerox machine that looked like it could survive a war. What I neglected to plan for were the lightening storms that knocked out all the electricity. Now, when I need a consent form at a moment's notice, technology can come to the rescue.

But technological gadgetry only goes so far. Much more importantly, I learned to smile, even laugh, in the vein of my Indian colleagues. The alternative is to pull one's hair out and, probably, not to work in India or the developing world, at least not doing field research with disenfranchised communities.

But this is not my story. This is where my story begins.

Beginnings

My research program in India began to take shape after two years of contact and visits with various stakeholder communities in South India, afforded by my collaborations on other HIV/AIDS research projects on the Indian subcontinent (Jana, Basu, Rotheram-Borus, & Newman, 2004; Newman, 2003; Thilakavathi et al., 2003). In the course of designing and implementing a multi-site community intervention trial of an HIV-prevention program among female sex workers in Calcutta and North Bengal, as well as a behavioral risk assessment among STD clinic attendees at a government hospital in Chennai, I sought out contacts in MSM communities. Even these contacts I did not seek out immediately, knowing that "homosexual behavior" is criminalized in India; and there are not gay community centers or bars in which to easily seek out allies or learn the lay of the land, even less so in the socially conservative South. Gradually, I began to cultivate local allies and met an avid, newly trained STD physician devoted to working with MSM and interested in gaining sociobehavioral research experience.

Many of the public health and government officials I met along the way either did not have much of an interest in the health and well-being of MSM, were simply unaware of this population or any health crisis, or were afraid of the various risks to their own careers. However, I did have the good fortune to meet some very highly placed Indian officials and professionals with an earnest interest in the health and well-being of all human beings, including MSM, who offered support in the form of personal contacts, business cards (an important entrée in India), and encouragement to pursue this line of investigation.

To this day, whenever I travel for my research in India, I carry the business card presented to me by a former head of the Indian Council of Medical Research—akin to the head of the U.S. National Institutes of Health—in hopes that if I ever get in trouble, I will produce his card and will be magically rescued.

Before embarking on any research in collaboration with community agencies, I made several visits to their sites, met with volunteers and clients, sat and talked extensively with a very helpful Community Advisory Board (CAB), and embarked on field trips with the staff to the public areas where they conduct outreach to MSM. My entrée through Dr. Venkatesan was key, as he was highly respected by staff and clients alike, as was my openness to listening and learning. The suitcase full of condoms "smuggled" across the border that I provided to the Director of the Social Welfare Association for Men (SWAM) was not enough, we both knew, to even make a dent in the need; but I believe my good faith at having followed through on my promise—perhaps aided by the uniqueness of multi-colored, flavored condoms—was duly noted.

Without local contacts, and a willingness to wait, observe, listen, and learn—and drink lots of *chai* (Indian spice tea)—I would have no program of research in India, at least not one along the lines of my own design and interest. I also do not take for granted that being a tenure-track academic at a large research university, and perhaps more so with an appointment in Canada, I am afforded a fairly secure home base to which to retreat and recoup. Even some of my American colleagues have been forced to become newly cautious with the interference in scientific investigations, including a crackdown on HIV/AIDS and sex research, that is the policy of the Bush administration (United States House of Representatives, 2003). Most vulnerable, however, are my Indian colleagues who have no place to retreat.

Thus, a secure home base, the ability to travel, and a strong trusting relationship with collaborators in South India have provided an essential foundation to enable me to undertake a risky program of international research.

Program of Research

Two research projects are presently in the field in Chennai, India—a "small" Indian city of about six million people. Two pilot projects have already been completed. Both current projects are collaborations between me, Dr. Venkatesan Chakrapani, and local community agencies serving MSM.

One project, the idea for which was initiated by my Indian colleagues, is an in depth qualitative needs assessment and behavioral risk evaluation among HIV-positive MSM in Chennai. We have just completed 90 minute long, in-depth, semi-structured interviews with 15 HIV-positive MSM, and three key informant interviews with service providers. Participants were recruited through local community agencies, using purposive and respondent-driven sampling. We received small funding from Family Health International, Dr. Venkatesan's MacArthur fellowship, and piggybacked on my project funded by the Connaught Foundation in Canada.

The second, Canadian funded, project is a cross sectional, 30-minute survey of 200 MSM randomly selected from public sex environments (PSEs) in and around Chennai. We assess HIV/AIDS knowledge, attitudes, and practices, as well as contexts of risk behavior, experiences of paid sex, violence and nonconsensual sex, and preferences for HIV-prevention interventions. After a year of planning, hiring, creating a training and procedures manual, staff training, constructing and pre-testing a questionnaire, and mapping out the sampling frame from February to April 2005, our very able local research staff has completed 200 interviews in Tamil.

Methodological and Ethical Challenges in the Field

Much as the road to our program of research has been long and interesting, the challenges in implementing our studies have superseded my expectations. Next, I will discuss methodological and ethical challenges in the field in the implementation of these two studies.

Sampling. Constructing a sampling frame of MSM in Chennai is a difficult endeavor. For one, male-to-male sex is criminalized (U.S. Department of State, 2005); the very criteria on which one is basing eligibility for one's study could land a potential participant in jail. Perhaps even more powerful than criminalization, both HIV/AIDS and "homosexuality" are highly stigmatized in India. Thus, MSM and MSM living with HIV/AIDS are not easy-to-reach populations; there is a substantial investment, based on survival, in *not* revealing either one's sexual orientation or one's HIV status.

Male-to-male sexual behavior in India is also characterized by a complex web of identities and practices, many of which do not conform to Western constructs of sexual orientation or identity (Asthana & Oostvogels, 2001; Chakrapani et al, 2002). For example, many, if not most MSM at risk for and living with HIV/AIDS in Chennai do not self-identify as "gay" or "*Kothi*," a local self-identification among some MSM who tend to adopt the receptive role in anal sex. "*Panthis*," a label ascribed by *Kothis* and one not adopted by the *Panthis* themselves, do not self-identify as other than heterosexual given their role as the insertive partner in anal sex; and many *Panthis* have wives and children, as do some *Kothis* (Chakrapani et al, 2002). In short, there is no uniform, readily identifiable "gay" community.

In each of the two studies in the field, we have addressed challenges around sampling in different ways. For our qualitative needs assessment, we wanted to ensure diversity among our participants. For one, many MSM in India are married. Persons living with HIV/

AIDS also exist across a disease spectrum. We consciously aimed to include a range of MSM, married and unmarried, younger and older, HIV+ and with full blown AIDS, and those who engaged in commercial sex and those who did not, to increase the breadth of our sample.

For our survey, our aim is to generalize our findings to the population of high risk MSM in Chennai. To that end, we chose a rigorous sampling strategy focused on public sex environments (PSEs). Clearly, not all MSM attend PSEs, but those who do are at elevated risk for HIV infection. Additionally, those MSM who attend the very limited community service agencies for MSM are not representative of the larger MSM population, most of whom would never attend such an agency due to fear of disclosure of their same sex behavior.

Our local research staff along with SWAM mapped the universe of PSEs (about 50) in Chennai. We then stratified PSEs by those that were predominantly commercial and those predominantly non-commercial and randomly sampled ten of each type. Within these twenty sites, we used time-space sampling (MacKellar et al, 1996; Stueve et al, 2001) to estimate and map out the flow of men at these sites by days of the week and hours of the day in order to draw a representative sample among PSE-attending men. We then randomly selected among time-space units within the sites that were selected and targeted these units for our recruitment efforts.

An effective field research protocol must be created with backups, alternative scenarios to address the inevitable exigencies that befall the best laid plans. Two such events are notable in our survey of PSEs.

The December 26th *tsunami* wrought devastation on Indonesia and Thailand; the more localized devastation in India was less reported. Two of our recruitment sites in Chennai were deluged. Thankfully, none of our staff or participants was hurt, though many local fishermen were never to be found. Perhaps, more mundanely, we had to re-write our intricate sampling plan as the beaches that were hit would be "down" for a while as PSEs where we recruit high-risk MSM. Well beyond sampling issues, the *tsunami* brought home the realization that as much as I do my ethical best to protect our research staff, and myself, from foreseen trouble, there is much that is simply out of my control. (I happened to be on the east coast of southern Thailand when the *tsunami* hit the western Andaman coast.)

The second and more formidable challenge to our research protocol plan involved dangers to research staff conducting recruitment.

Recruitment. Several concerns present in trying to recruit a highly vulnerable and stigmatized population. The first is the challenge of identifying potential participants. A second is the challenge of mitigating any risks to participants due to research participation.

A recent UNAIDS (2001) study of stigma and discrimination faced by people living with HIV in India provides a case in point; only 1½ pages out of the 70-page report address "gay and other homosexually active men." The authors note that "[d]espite numerous attempts over several months, eliciting the voluntary participation of HIV-positive gay men in this study proved unsuccessful" (UNAIDS, 2001, p. 56).

For our in-depth qualitative study of HIV-positive MSM, we conducted recruitment through local community agencies serving this population. All recruitment was done by word of mouth only; no written materials were printed or distributed, nor were any study identifiers posted at any interview or research site. Participants were recruited either through contact with someone at a community agency for MSM living with HIV/AIDS, or by participants asking their peers if they wished to participate. In the latter case, the potential participant had to make the first contact with the research staff.

Within two weeks we had a sample of 15 participants across a range of MSM living with HIV/AIDS. I am not under the illusion that I personally could have conducted that recruitment, but our staff, known and trusted among many MSM and persons living with HIV/AIDS, had entrée to the population. The level of ongoing stigma, harassment, blackmail, and violence reported by participants, however, suggests the problematic oversight in the UNAIDS report. In fact, several of our participants reported they had an easier time finding acceptance among their families in being HIV-positive than in disclosure of their sexual orientation (Chakrapani, Newman, Shunmugam & Melwin, in press).

Our survey recruitment required outreach at PSE's. PSE's are fairly dangerous everywhere (Frankis & Flowers, 2005). They are particularly dangerous in Chennai, where there continue to be documented reports of harassment and violence against both the men who frequent these sites and the outreach workers conducting HIV prevention (Human Rights Watch, 2002; Kumar, 2000; PUCL-K, 2001; U.S. Department of State, 2005). Indeed, data from both of our studies documents blackmail, harassment, sexual assault, and violence faced by MSM. It is one thing to collect data from participants reporting past incidents and to read published reports:

> *Physicians for Human Rights (PHR) has learned of the detention, arrest, and harassment of 4 colleagues in Lucknow, Uttar Pradesh, India...These men were detained for six weeks without bail and recently released on August 20 but will now face trial on charges of conspiring to commit obscenity and sodomy. PHR believes, as does over 400 organizations, however, that these HIV/AIDS workers were only doing their job, that is, developing and distributing health education materials for males who have sex with males. This is not the first time health workers have been detained in Uttar Pradesh (Human Rights Watch, 2002).*

It is another matter when one's own research staff and potential participants are endangered. In the late stages of recruitment, I received the following email from our project director:

> *Tue, 29 Mar 2005 05:59:10: By this time we should have completed 180 interviews. A few of our recruiters are facing some practical difficulties in recruiting the participants from certain cruising sites; i.e., an unknown person has been murdered in North Chennai, an area very close to two of our recruitment sites. There has been a severe raid by the police at those sites. I have asked the recruiters to stop, temporarily, recruiting participants from those sites and to continue with rest of the sites.*

I immediately inquired further about the incident, and made clear that the safety of our research staff was our preeminent concern, not the sampling protocol. I asked for further clarification of what happened, and received the following response:

> *Thu, 31 Mar 2005 05:19:46: ...the murdered person was a MSM. The murder took place in a ceremony that had been organised by a kothi... Severe raid here means all the kothis who attended the*

ceremony had been taken into police custody for enquiry. The cruising sites in and around North Chennai have been ransacked by the police men and many MSM who visit those sites had been arrested by the police as suspects.

In his well intentioned zeal to adhere to our detailed recruitment protocol, the project director wished to resume data collection at the sites in question. I replied that we must suspend any further data collection at the two sites. In so doing, I wanted to protect, to the extent possible, our research staff conducting recruitment. I also did not want to contribute to any possibility of having any MSM attend the site in hopes that they might be recruited for the study and incentive. I also wanted to assert responsibility so that any alteration in our research plan was squarely on my shoulders. I immediately altered the recruitment protocol by substituting two additional locales and explained that we would use this event as an opportunity to adjust for overrepresentation among non-commercial PSE attendees. In this way, the project director and staff could continue the study and not have a feeling of failure in eliminating the two sites that posed imminent danger.

Data collection. Of utmost importance in both of our studies in the field is ensuring and maintaining the confidentiality of participants and not placing them at additional risk by virtue of study participation. Accordingly, in both studies, interviews were conducted in private rooms at one of three safe community venues known to our research staff. Perhaps more notably, we worked with my University Ethics Review Board (ERB) to revise requirements; following the letter of the law, or usual practice, could place participants at risk, that which the ERB is charged with mitigating. For one, participants were asked only to place an "X" in the box on the consent form: no signatures or initials were required. Understandably, MSM who are at risk of police blackmail and violence are wary of having their confidentiality broken; thus, a request for a signature in an otherwise anonymous survey is seen as suspect.

Indeed as reported in our running project log, the project director wrote, "One of the recruiters mentioned that some participants feared being caught on camera in the room where they had been interviewed." In previous studies we had opted against using audiotape recorders with headphones that would enable respondents to listen to and answer sensitive questions by placing checkmarks in boxes rather than having to respond to an interviewer; some participants feared their responses were being monitored through the tape recorder.

Second, a copy of the consent form was retained at the study site; copies were not distributed to individual participants. In a setting in which MSM are often harassed by police or "rowdies" for carrying condoms—as a sign that they are going to engage in sex, or that they are going to solicit sex for money—being caught with a consent form that indicates MSM or HIV risk presents a potential danger. Indeed, our data revealed sexual assault and violence at the hands of police, and participants' fears of carrying condoms (Chakrapani et al., in press):

> *Policemen took me to police station and during the night one policeman asked me to come to the bathroom...he had sex with me in the back...I did not have condoms at that time since I was only in my underwear. I also could not talk about condoms. Even if we just show condoms they will beat us on our hands with the lathi [police stick].*

Third, given the danger of PSEs, we decided not to attempt to conduct even brief screening interviews in these environments. Both potential participants and research staff could be placed at additional risk in having to remain longer in the PSE than they would otherwise. At the same time, it is usually preferable to conduct one's data collection as proximal as possible to recruitment to increase one's response rate and, thus, the validity of one's data. Furthermore, our sampling plan required not only that visits to sites be scheduled as determined by our time-space sampling, but that research staff enumerate the approximate number of men on site at a given time and, in view of our target for recruitment, approach every nth man (e.g., every 3rd or 5th man) to ask if he would be interested in participating. We wished to avoid having research staff approach only their friends, or more attractive men, or those who arrived on site first, or last—each of which might bias the sample. If we then allowed anyone who happened to appear on site to participate in an interview, our rigorous sampling could be easily negated. In order to try to ensure that only those asked to participate would be interviewed, we gave MSM who agreed to be interviewed a small, discreet card (with no mention of HIV/AIDS or MSM) with a deadline for a study interview, and research staff accompanied those who wished to the interview site. In this way, we both avoided undue risk for study participants and research staff and followed through on a rigorous sampling plan in a risky field project.

Another aspect of data collection, behavioral research on HIV risk, is often bounded by the veracity of participant self-report. One cannot follow someone into the PSE much less their bedroom to document sexual practices and (non)condom use. In order to increase the reliability of our data, we also collected blood samples, on a voluntary basis, to test for syphilis. We made a decision not to make this a requirement of participation, as the Indian staff and CAB reported that some men, who might otherwise respond to survey questions, may be loathe to undergo a blood test. Some men also may fear a loss of confidentiality in returning to get their test results. Two-thirds of participants underwent blood testing: 14% of these had syphilis, documenting high risk for HIV infection.

A related issue of opting to test for syphilis, not HIV, was due to a variety of factors. For one, HIV testing is more expensive and we had a very limited budget. Second, HIV is infinitely more stigmatized and the potential damages wrought by any breach of confidentiality more damaging. Another important concern that we made available in our study budget was that anyone who tested positive for syphilis could receive treatment, paid for by the study. We were in no position to provide that in the case of an HIV-positive test result; at most, we could refer men for counseling and whatever treatment may be provided by the local government hospital, which does not provide state-of-the art HIV/AIDS therapies. Nevertheless, having syphilis suggests high risk for HIV, both in documenting potential routes of exposure and increasing the physical risk of HIV infection. Thus, all men in the study were referred for free, confidential HIV testing on a voluntary basis.

Conclusion

I remain aware that what motivates my continuing desire to conduct field research among MSM and male sex workers in India, even as it is ostensibly focused on HIV prevention and care, is a deeper human rights agenda. Access to condoms may be necessary but is clearly an insufficient condition to effect HIV prevention. The issues when one engages in sex for survival—with a twofold increase in payment for not using a condom; or when one is subject to police harassment for even carrying a condom—are well beyond knowledge and attitudes. The risks of a disease that might cause one to be symptomatic in several years, in the context of more immediate risks due to harassment and violence, may render HIV/AIDS less of a concern. Indeed, the very concept of high risk in application to HIV/AIDS implies that in the absence of HIV one is safe. This is clearly not the case for many MSM in India.

The ability to marshal effective HIV prevention is constrained by the myriad social determinants of risk. This is the larger agenda of our long term project: to ensure basic human rights for MSM in India. Down the road, we can help to mitigate the impact of various risks—harm reduction on a community or societal scale—but our vision is nothing less than the decriminalization and destigmatization of sex between men, as well as a contribution to the recognition of HIV/AIDS as a crucial health issue in India overall.

Conducting field research on HIV/AIDS and its prevention may seem worlds away from clinical social work practice, but parts of my MSW education continue to serve me well. Listening skills, the upholding of high ethical standards, and the ability to negotiate among an array of sometimes competing agendas are all skills that are central to social work practice. Furthermore, social work teaches the importance of parallel process. The very process and experience of implementing our research studies offers profound insights into the lives and perspectives of our participants, an important layer of data beyond the content of more traditional data sources. Natural disasters, police harassment, fear and murder, all of which impinged on our research staff conducting recruitment and data collection, serve to powerfully corroborate the data from our participants in qualitative interviews and survey questionnaires. They maintain thorough records of these daily challenges, both for ongoing safety and training purposes, and as a source of invaluable data.

I am energized as I continue to work with my Indian colleagues to implement community-based studies in and around Chennai: from epidemiology, to sociobehavioral determinants of risk, to needs assessments for men at risk of and living with HIV/AIDS. We are giving voice and listening to many MSM. We are helping to get the word out, in India and around the world, about the discrimination and risks faced by MSM. And we are also contributing to the growth of a cadre of trained recruiters, interviewers, data analysts, and behavioral researchers in India who will support the ongoing struggle for human rights among gay, lesbian, bisexual, and transgender persons, as well as persons living with HIV/AIDS in India.

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Reflecting on Researching the Family Biogram: the Challenges and Opportunities of Working in Violent Contexts

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This narrative is written as a reflection on the dilemmas, challenges and opportunities encountered when undertaking cross-cultural fieldwork in unstable and violent contexts. The fieldwork was carried out in order to design an instrument to capture the experience of internally displaced families in Colombia, and provided a context in which to think about the experience of fear and the meaning of resilience. As a family therapist and child psychologist working in the field of violence and abuse, the author is accustomed to using these words. However, during her time in Colombia, the author gained a deeper understanding of both, which she believes influenced how she was able to present the research.

Prelude

Four years ago, I was one of fourteen students selected to study in an international masters course in public health, focusing on safety promotion and community protection and sponsored by the World Health Organization. The course offered the unique opportunity of studying in four European countries—France, Sweden, Italy, and Portugal—and meeting professionals from all over the world working in areas related to violence and abuse. As someone with a diverse cultural heritage, and as a European professional who has worked in Latin America for the last seven years, I saw this as a rich context for learning.

Early in the course, I formed a special friendship with the only course member from a developing country, J. The relationship was strengthened by our common work context—I was then working in Bolivia—and my interest in Latin America. I admired J who was working in Colombia as a doctor and as a volunteer for the Red Cross in communities affected by the armed conflict. Many communities in the high conflict areas were moving, becoming "internally displaced." The moves were generally triggered by traumatic events following a period of increased tension and fear. In 2001, internal displacement in Colombia reached crisis proportions, with an estimated two million people displaced (Norwegian Refugee Council, 2001).

Apart from the cultural affinities, J and I shared a commitment to human rights and working with disempowered, marginalized communities. Internally displaced populations, as refugees within national borders, do not enjoy the same rights or international protection as cross-border refugees (Cohen & Deng, 1998). On a prior visit to Colombia I had witnessed the plight of internally displaced families as they held the Red Cross building under siege, demanding help and support.

To complete the international masters, all students had to undertake and publish research on a theme related to the course. J was looking to construct a measure of "threats" that would help communities analyze their strengths and weaknesses and understand their options when faced with the threat of armed attack. As a complement to his research, J wanted to use my expertise as a family therapist and researcher to analyze the verbatim interview case material that had been gathered on displaced families. This micro-systemic analysis would focus on identifying the factors that lead to some families moving, others not, some returning, and others not. We were both interested in factors that helped families to become resilient and continue striving in the face of adversity.

J and I discussed modeling our instruments on "the biogram," developed at O Porto University (Agra & Matos, 1997). The biogram was constructed using the "reconstructive biographical" method derived from the work of the Chicago and Louvain (Belgium) sociological schools (Coulon, 1992; Debuyst, 1985; Sutherland, 1934;). The biogram is a temporal and dimensional grid framework that was initially devised to capture the life experiences of criminals and victims of violence and abuse in a structured way. The methodology bridges the gap between professional discourses and subjective experiences. The dimensions of the grid used to collect and structure information are constructed by "crossing" (combining two sources of information) "hetero-biographical" information (information, reports, documentation from sources other than the subject) and auto-biographical information (the biographies of the subjects themselves) for qualitative and quantitative analysis. Explanations in terms of subjective "meanings" are prioritized over causal explanations. Our instrument, the family biogram, was designed to map families' changing experiences over time, including the experience leading up to flight, the consequences of flight, the meaning that each family gives to its change in situation, as well as the family's resources for rebuilding their lives.

Drawing on post-modernist narrative family therapy and the idea of "dominant discourses" (White, 1989), I saw the structuring of biographical information using the family biogram as offering the possibility to develop empowering descriptions. These descriptions would 1) capture the "diversity" of experience including displaced families sources of strengths and their needs; and 2) offer an alternative to the dominant professional discourse that emphasizes trauma, powerlessness, and dependency (Summerfield, 1998). Refugees and displaced families are often considered as "homogenous, undifferentiated masses" (Harrell-Bond, 1999). The dominant discourses used to describe their experiences are often overwhelming and self-defeating (Lucas, 2004) and do not capture the courage and resilience of family members or the family as a whole. Intervention programs based on these discourses often reinforce the powerlessness and dependency they describe. The family instrument was to capture "heterogeneity" in order to encourage more tailored programs that can take into account families' strengths, weaknesses, and hopes for the future. In the case of Colombian families, this was done to draw attention to the need to explore alternatives to following the government's policy of "encouraging" (sometimes coercing) return to the communities of origin.

Fieldwork: Issues and Dilemmas

J and I agreed that I would travel to Portugal to study the construction of biograms and start gathering hetero-biographical information (reports, documents, articles) on internal displacement. This would be prior to the month I would spend in Medellin, Colombia, working with a team at the University of the State of Antioquia analyzing family autobiographies from 120 verbatim transcripts. For security reasons, directly interviewing families was impossible for me as an outsider. J gave me some information on the unit where the transcripts were held and the research conducted there, as well as some of the resulting publications, one of which was on internal displacement.

I felt well prepared and excited at the possibility of joining professionals helping those affected by organized armed violence in a country for which I have a great affection. The most difficult part of the preparatory phase was the anxiety expressed by my family and my husband regarding safety issues. Living in Latin America ourselves, and having already visited Colombia, my husband knew that Medellin is the capital of one of the states where the conflict is at its most intense. We also knew from our experience of living in Bolivia, that universities, like the University of Antioquia where the transcripts were held, are often caught in the country's political conflicts. Whether it was because of my interest, my commitment, or the trust I had in J (he would not have invited me if it had not been safe), I was not concerned about this aspect of the fieldwork. Maybe I chose only to focus on the positive aspect to overcome any underlying anxiety, and I went on the condition that I would not take "unnecessary" risks.

My first week in Colombia was in the private university where J worked and where I would be based when not reviewing the transcripts. I went to meetings to understand more about the armed conflict and peace initiatives, and shared with my Colombian colleagues what I had learned about biogram construction. However, by the end of the first week, I was impatient to start analyzing the transcripts and a little anxious that nothing had been said about going to the unit. Maybe this was not going to be as simple as J had said it would be. I eventually asked J and it was agreed that I should meet his sister, who—unknown to me until this point—was coordinating the displaced families project. I met her first on Friday night at a social event at J's mother's house where she, reluctantly I thought, agreed that I could visit the unit on Monday.

In between time, I experienced firsthand the reality of the guerilla conflict. While driving out to Js' "finca," or farm, someone shot at the car behind us. Back in the city on Sunday night, a bomb exploded near the flat where I was staying, blowing out the windows of surrounding buildings. The risks involved in the fieldwork, and the basis for my family's concern, were becoming apparent. However, since J's family was reassuring, on Monday morning as planned, I set off for the University of Antioquia. We started with a meeting where I explained my research proposal and what material I would need to review and why. Concerns were voiced about using information that would identify the families, and I felt that J's sister was still a little wary. However, she agreed to let me access the cases and permitted me to work there with the full cooperation of the research team.

The following events remain clear in my mind. Halfway through the morning, we heard what I thought, having witnessed the frequent demonstrations in State universities in Bolivia, were firecrackers. The noises grew louder and louder, and then one of the members of the team—a young male researcher—came back from the toilet, white as a sheet. He said that there was someone with a mask and a gun right by our door. Our immediate response was to shut and lock the door. The noises were getting louder and were obviously gunshots. The whole team turned pale, confessing that they did not know what was going on and that this had never happened before in the University. Then there was a loud knock on the door, and we all looked at each other. J's sister, as the senior member of staff present, said she was unsure what to do: open the door, or keep it shut and risk the anger of the gunman.

There was a three- to four-minute pause and I said, "I have my French identity card on me, and they kidnap foreigners here!" They told me not to open my mouth as I did not look "obviously foreign." I told the girl next to me that I did not think I could run as my legs felt like limp "spaghettis," and she confessed that hers did too. The sporadic gunfire lasted for 40 minutes. I managed to contact J on a cell phone and tell him what was going on. He said to stay calm and that it was unlikely we would get hurt. When the shooting stopped, I left for the other University, frightened and confused, to debrief and recover. After speaking to J and his family, I decided to say nothing about the day's events when my husband phoned that night. I knew he would want me to return and, although shaken by an accumulation of events, I was not ready to take that step.

I spent the next day in reflection. I was becoming more aware of the safety risks associated with unpredictable violence, but this had paradoxically reinforced my commitment to the project and helping people who had repeatedly experienced such violence. I had complicated feelings about the risks, the promises made to my family, and how to balance these against my commitment and J's expectations of our research. I also reflected on J's sister's attitude and J's enthusiasm. What explained the difference? I had written an article on the complex dynamics of "outsiders" from dominant cultures working in Latin America and what they represented for local people and professionals. I thought about what I might represent for her, but not represent for J. How could I gain respect and trust for what I was doing?

That evening, J shared some information with me that perhaps would have remained hidden if the events of the previous day had not occurred. Two years prior to my visit, the head of the unit where I was working had been shot due to the information held on displaced families. J's sister had filled his position until a replacement was found. With this information, I hypothesized further about my relationship with the research team and what they thought of me. Could I really understand what they had risked? Was it right that I would fly in for a month, write a paper, and share the credit for the research on which it was based? Would I be respectful to them and the families that they had interviewed? J also told me that the shoot-out at the University was related to the death of a student killed by the military on that date a few years earlier. There was usually some form of demonstration to mark the anniversary, but J explained that the escalation of the violence that had led to the armed interchange was due to the increased number of students whose studies were funded by the FARC attending the University of Antioquia. These students—trained in armed combat—were committed to escalating existing levels of discontent and using symbolic events to generate violent protests.

Apprehensive after the week's events, I returned to the unit on Wednesday. I went every day until I finished the narrative analysis of the transcripts. My relationship with J's sister slowly changed. Going back to the questions generated from my hypothesizing, I can only speculate that "actions speak louder than words." Maybe my return and persistence communicated, as I hoped it would, not only that I was acting as a self-interested foreign researcher, but that I was committed to the project and resilient enough to carry it out with integrity.

Final Reflections

Persisting with the research required thinking realistically of the risks involved in the project and overcoming fear. It also involved thinking of the promises I had made concerning my safety. After the attack at the University, I began to think more carefully about the fieldwork danger. Speaking to people, analyzing events, and "getting to know" my context better, I knew there would always be a risk. I had promised not to take "unnecessary" risks, but sporadic and unpredictable violence made it difficult to disentangle "unnecessary" from "necessary" risks.

In the end, after a lot of reflection, I decided to continue with the research because I "felt" I could and that it was worthwhile. The opportunity of giving a voice to internally displaced families was, in my mind, valuable and unique, especially after all I had witnessed. In Colombia, I experienced fear and "resilience" and understood them better. I knew what it felt like—at an emotional and participative level—to be caught in an armed attack. I overcame my fear and developed some "resilience" through mutually supportive relationships with the team members and the hope we shared of creating a better future for displaced families. Conducting the research with personal insights into what it is really like to be under attack and how relationships are important in developing resilience helped me design an instrument, the family biogram, that has the potential to capture the complexity of the families' responses to fear and trauma. Understanding these responses helps identify not only families' needs, but also their resources in the aftermath of displacement. Like the biogram on which it was modeled, the family biogram that was developed prioritizes the meanings attributed to human experience. It replaces the "categories" as found in psychiatric and criminal classification systems with a temporal dimensional grid like structure which can be used to collect narrative information in a structured way (Agras & Matos, 1997). The process of "crossing" the autobiographical data of the families and heterobiographical material from other sources makes the dimensions relevant to what professionals working with displaced populations think about the effects as well as to the people "living" the process, and can be used to establish a dialogue between them (Agras & Matos, 1997; see Lidchi et al, 2004;). Using the dimensions, information on all aspects of experience can be collected, including feelings of fear and insecurity, loss and distress, changes in circumstances (family, economic, community relationships), sources of support/help, meanings attributed to the consequences of displacement, and reactions to the experience.

The final biogram grid structure, and the codes developed to fill it in, mapped the experience of the families over time and captured the complex changes in circumstances, perspectives, and emotions that resulted from forced internal displacement. Mapping families' experiences - their emotional responses, social relationships, loss and resources (social, economic and psychological) - using a temporal dimensional instrument revealed needs and strengths over time that are not captured in diagnostic categories such as PTSD that emphasize stress and vulnerability. Community support and personal relationships were important sources of strength to help families, often influencing the interpretations of the consequences of displacement. I had understood the complexity of reactions to traumatic events, as well as the importance of personal interpretations/meanings of the events and supportive relationships through my own reactions to the shoot out. Despite being frightened, I continued with the research because of my perception of its importance and the support I received from J and his family to carry it out .

The family biogram and the methodology underlying its construction and use has been published and reviewed positively by professionals involved with refugees and displaced populations (Lucas, 2004). Professionals in Australia working with refugees appreciated the family biogram as a tool that emphasizes the "interpretation" of experience and meaning which goes beyond the classification of trauma. Developing an instrument that collects autobiographical narrative data along a number of dimensions, as well as the meanings attributed to displacement, encourages the design of interventions that take into account family strengths and needs, rather than applying a standard "package" resulting from a specific professional perspective – for example, one stressing the trauma /victim component of the response to forced internal displacement. The family biogram helps:

> *...to engage in a dialogue ... so that we [professionals] may begin to understand the significances which they [the refugees] attributed to their experience, alongside the significances which we as counselors may tend to perceive. In this way we have come to see our clients as survivors rather than victims, and have begun to learn something about resiliency in the face of adversity. We have also learned something about the value of relationship in providing a professional service and of the importance of curiosity as a tool (Lucas 2004, p. 222).*

Taking an interest in families' narratives and involving them in the decision making process is a way of empowering and encouraging them to participate in the reconstruction of a future, as opposed to being passive recipients of aid and reinforcing their roles as victims.

From my position now—safe in Brazil—I judge that the risks were worth taking in terms of the outcome of research that reinforced the need for sensitive interventions that do justice to the complexity and richness of the experience of Colombian families, specifically, and displaced people more generally. The decisions I made regarding personal safety were made on a rational basis but also because they felt "right." Maybe in different personal circumstances, I would have evaluated the risks differently, but I feel lucky to have had the opportunity to experience all this firsthand.

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Conducting Research with Women in Jail

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This narrative is about conducting research with women who are incarcerated at the county jail. The author discusses several aspects of the research process and how doing the research in jail influences the process and the researcher.

"I, too, have to get comfortable with being uncomfortable." These were the words I kept hearing in my head. I stood in the glass "air lock" waiting for the second steel door to slide open and allow me to continue my walk to my interview room at the jail. As a clinician, I liked to hear clients talk about getting comfortable with being uncomfortable; it signaled growth and change. Uncomfortable was a good thing in that situation. Now, as a researcher going into the jail to begin a study, the idea of ongoing discomfort wasn't so welcome. I wasn't sure how the jail setting or the staff would accommodate a research project, but I was going to find out.

The crack cocaine epidemic was the reason I found myself conducting research in the county jail. Crack cocaine had become the drug of choice for women of child-bearing age in the mid 1980's, and I found myself pulled into work with pregnant women and new mothers who needed drug treatment. The setting for that work had been familiar: the hospital's labor and delivery floor, or the home visit after the baby was born. As the crack cocaine epidemic grew and more babies were born prematurely and suffered grave consequences, the public began to take notice. Treatment resources for pregnant women increased, but so did criminal justice efforts to enforce drug laws. One result was that the prison population in the United States grew rapidly. One of the Corrections Officers or C.O.'s at the jail remarked, "We can solve the drug problem – just lock up all the users." I was not sure if she believed that statement or not. I was clear that incarceration for non-violent drug abusing women hadn't shown any promise as good public policy.

Even though women are still a small minority of the overall incarcerated population in the U.S., their rate of incarceration has increased dramatically. A disproportionate number are African-American, Latina, and other racial/ethnic minority. Over half of them are incarcerated for non-violent drug offenses and approximately three-quarters of incarcerated women are mothers of minor children. Compared to the demographic characteristics of women in drug treatment, the characteristics of women in prison are similar. One thing most incarcerated women have in common is poverty (Chesney-Lind, 2002; Petersilia, 2003; U.S. Department of Justice, 2004). Given the opportunity, I thought it would be important to study treatment needs and barriers to services among a sample of women in the jail. Not everyone agreed. One of my friends, a long time volunteer at the jail, remarked, "You need a study to know these women are depressed and need help?"

I had long thought the jail provided opportunities to initiate services and to develop strong linkages with community agencies and professionals. The jail is local, so detainees aren't far from home, and family and service plans could incorporate or strengthen existing support networks. A jail stay can provide the time and space for assessment and treatment planning for women who otherwise might not come to an agency for help. The jail setting wasn't foreign to me. I had facilitated a weekly therapeutic support group as a volunteer and found the work in jail to be interesting and fulfilling. Group work in the community competes with clients' demands and pleasures of daily life. Group time in jail presents a welcome diversion and a structure for the day. I had been gratified by the number of women who faithfully attended group sessions. Women in jail had the benefits of regular meals and a controlled environment in which to get through the early days of sobriety. Many of the women I saw were in better physical and mental shape than they had been in several months or even years. In group I would hear, "I can see my priorities more clearly when I am in jail," or "There aren't as many distractions here." One woman remarked, "Jail has been my window to let me see my life as it was and how it could be." There was a waiting list for the group; most women were eager to participate and were ready to work when they were asked to join the group. "Jail is a group worker's dream setting," I would tell my colleagues when I returned after a morning at the corrections center. When the opportunity to do research in jail presented itself, I welcomed the chance, but I was uneasy about increasing the time I spent at the jail. The jail was fine in small, group session doses, but I questioned my stamina to spend long days interviewing there.

For a number of years, my focus had been on barriers to substance-abuse treatment for women but not in criminal justice populations. I found my research muse in a drawing done by a woman who introduced herself in group by showing a picture. She had drawn herself running from drugs and the police, saying, "I'm running for my life and sanity." She clearly drew what she was running from, but she was running toward a large blank space on the paper. She had no idea what she was seeking to replace the life she was fleeing. With her in mind, I designed the study to examine women's ideas of what would help them to access and use services after they were released from jail.

Research Setting

The study setting was an urban, full-service jail with a rated capacity of about 1,700. The daily count, however, could be as high as 2,000. The jail can exceed its rated capacity up to 125% and still be within the regulations, but on the days of overcrowding everyone, both inmates and corrections staff, were tense. There are ten classifications, from pre-trial detainees to sentenced misdemeanants. About 12% of the inmates in the jail are female and they are housed on two floors. Each floor has three pods of women and each pod has a control office where one of the officers sits to operate the doors and the elevators. Unlike most prisons, jails rarely have any outside space for inmates; there is not an exercise yard or any outside walkways to courtrooms or other buildings. In the small talk before the interview begins, the most commonly asked questions are about the weather: "What's the weather like outside?" "How does the air smell today?" "Is the sun shining?" I began to make some mental notes about the seasons or the weather so I would have more than a stock answer on the subject of weather. There is scant natural lighting in the jail. No matter what time of day or season, the lighting is the same. Temperature, however, does vary. On some days, the jail is quite cold. On others, the heat is stifling and on others, the temperature is comfortable. Try as I may, I could not determine a pattern or a reason for the temperature fluctuations.

As a researcher, I was in the jail for longer stretches of time than I was as a volunteer. In some ways I tried to fit in. I tried hard to make eye contact with each officer I met. I waved to and thanked the C.O. in the control booth, no matter how long I had to wait. I laughed at jokes – all jokes - even the ones I would have found offensive on the outside.

Some of the C.O.'s were interested in my work and took time to talk with me. Some asked me advice about specific problems. Others were tougher and acted as if they felt I was one more nuisance to deal with on the shift.

I was careful to adhere to jail regulations. Jewelry was out except for hoop earrings with easy release backings. Scarves, belts, pins were all absent from my jail day wardrobe. Many everyday items are contraband at the jail. Some of them, like nail files and scissors, are obvious. Others, like pens, paper clips, keys, candy, and money, must be pitched out of pockets, purses, and briefcase. Often I passed the second checkpoint before I realized I had some forbidden object in my pocket and hurried back to the lockers to get rid of it. I didn't want a C.O. to think I didn't know the ropes. Security is the jail's primary product and I wanted to show I respected that, even if I might not agree with all the ways that product was delivered.

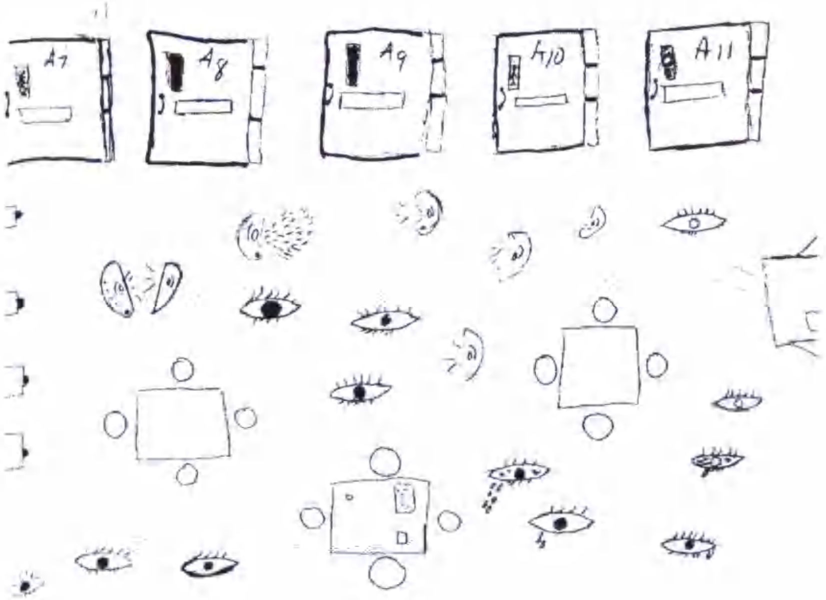
Women jail detainees wear loose-fitting, dark blue tops and pants. One of the former group members remarked on the uniform: "When I stop taking care of myself and slide back into trouble, I see myself in blue again." I never wore denim or dark blue on jail days. I also avoided any shirt and slacks combination that resembled the C.O. uniform. It seemed silly, but I could not bring myself to break my personal dress code. I was not sure why. There was little chance I would be mistaken as an inmate and even less so for a corrections officer. I wanted to fit in so things went smoothly for me, but I also wanted to be separate, to be clear I was not part of the correctional system.

Access and Recruitment

Researchers often have had difficulty in gaining access to jail and prison populations. Because of my years of volunteer work in the jail, I had an insider's view of the schedule, the layout, and the general operating procedures when I designed my study. At first I was concerned about recruiting enough women into the sample. That worry was needless. Even though women in jail have attorney visits, family visits, pre-trial assessments visits, and court appearances, they have a lot of free time. The opportunity to talk about themselves and their ideas seemed to provide an appealing diversion for most, if not all women. My sampling process involved sending a brief description of the study procedures into each pod and asking interested women to sign and return the slip. Purposefully, I did not mention there was a \$20 stipend for research participants. Each day the study staff culled the duplicates and randomly selected names from the piles. After the first couple of interviews, the word about the \$20.00 traveled fast. All of the project staff became very popular. When study staff walked onto the floor, women waved and called out "I will be in your study, call me. I can be interviewed today." While this groundswell of support for the research process was helpful in meeting our sample goal of 200, I grew uncomfortable thinking about the reasons women were so eager to volunteer. I knew part of their enthusiasm was related to the boredom of jail and the need for the \$20. But I also knew from my clinical work how desperate some women were to tell their life stories and to get help for their problems. I was satisfied the study would help to develop additional services for women in jail at some point. But participation in the study would do nothing to help these particular women when they needed help; they needed help right now.

Confidentiality

Jails offer little opportunity for confidentiality. There are eyes and ears everywhere and no place is out of sight. An example of art work from one of the therapeutic groups illustrates this point.



The interview room I used was small, but adequate for our needs. There was an outlet for the computer, a good light source, and a door I could close for confidentiality. Like all other spots in the jail, this space could be seen by anyone who walked by into the pods and by the officer in the booth. For security reasons, visibility is a good thing, even though I never have felt threatened by any woman I have met in jail. For the research interview, however, this visibility created some problems. If a woman agreed to participate in the study, everyone knew. For that reason I was careful what I called the study and how I talked about eligibility criteria. By naming the project The Barriers to Services Study, I felt a woman would not be vulnerable or stigmatized if she were seen talking to one of the interviewers.

I had wrestled with the problems of confidentiality in jail as a clinician and had some success. Each group developed its own confidentiality guidelines and I had negotiated with the staff to run groups without having a corrections officer present. The confidentiality issues were different in the research setting. Women had to understand that participation in the research interview could not hurt them or, more importantly, could not help them. During the consent process women often asked, "Can you tell the judge about my problems and how I have been trying to get into treatment?" "Can you give a copy of this interview to my lawyer?" The university's IRB was clear that the information we collected had to be confidential. If women thought that participation in our study might result in a more favorable judicial ruling, they might feel coerced to participate. Also, it was not clear in every case how the information might be used by the court. Could the information from the study interviews be more useful to the defense or the prosecution? Given our confidentiality rules, few women refused to participate. In fact, only 2 of the 214 women we approached said no and both did so because they had just been released and were going home.

The other problem with such a visible interview space was the opportunity for distractions, and there were plenty of them. Groups of women filed in and out of the pods to go to medical, to court, or for visitation. These sojourns provided opportunities to see and wave to friends from other pods. There were visits from wardens, visitors, volunteers, medical staff, and lawyers. Each time the elevator door opened, there was another opportunity for distraction from the research interview and the need to refocus on the questions and purposes.

Interviewing and Responding to Needs

The research interview was long and contained sensitive questions about substance use, trauma, and abuse. Some of the information was entered into the computer and the rest used a paper and pencil format. Usually I could complete all of the questions in about 90 minutes, but some of the more complicated interviews stretched into two-and-a-half hours. It often was a painful conversation. Even though I had heard many traumatic stories in my clinical work, I found the research interviews to be distressing in a particular way. Many of the women's life stories painted a picture more consistent with victimization than offender. I found myself looking for even the smallest gesture—offering the better chair, making sure the pencils were sharp, allowing participants to use a pen during the interview—just to try to even the lopsided balance of the research relationship. I have developed a deep gratitude toward the women who have offered to tell their stories, to reveal a painful and traumatic past, in order to help me understand the barriers they face in seeking help

The research interviews covered many of the same issues I touched on as a clinician, but the research interview did not end in a treatment plan or a referral for services. It ended with, "Thank you for your help with the project" and a commissary receipt. The research interview didn't provide an opportunity to reframe and discuss a comment, such as, "I don't want services because I am afraid someone will tell me I have a problem. I just want to be normal and try to hide my problems." There wasn't a place in the research interview to counter a comment, such as, "God put weed on this earth and I'm gonna smoke it no matter what." I ended each interview with the question, "Is there anything else you would like to tell me about your experiences?" Sometimes women would ask for suggestions about where to look for help, or would ask specific questions about treatment programs, but usually they just wished me good luck with the study and returned to the pod.

Most of the women were younger than I, but a few were older and have spent the better part of their adult life behind bars. We had some things in common, such as gender and family roles – all were daughters, some were also mothers. Many of their stories were not remarkable, except for the extent of their bad luck of having parents in poverty, few caring adults in their childhood, and easy access to alcohol and drugs early in life. Most women shared their hopes for their children to be successful and the worries about their families. They talked about wanting to find things most women look for in life: meaningful work, loving relationships, and a peaceful existence. Many of them talked about their religious beliefs and how they wanted to have a second chance. Despite a wide gap in education and experience between us, I found I had more in common with these women than I might have expected. However, at the end of the interview, my research participant went back into the jail pod and I to my middle-class life. Many days I left the jail feeling life was profoundly unfair.

Conclusion

The findings of the study to date point to a need as well as an opportunity for a variety of in-jail and re-entry services for women with substance abuse and mental health concerns. I am pleased with the progress and with the fact that there have been many opportunities for me to communicate our results to a broad audience. I have hope that, in some small way, the study will help to improve the lives of women like those who volunteered to talk with us. Have I become comfortable with feeling uncomfortable? Certainly I have found a level of comfort in conducting research in the jail setting and look forward to the next study. It is a worthy and rewarding line of research and advocacy. That nagging feeling of discomfort still exists, though, when I think about the way society has used incarceration to deal with drug problems of the poor. As a result of the research in the jail, I am increasingly aware of the segregation of opportunities for formerly incarcerated women. So many service gaps exist in all arenas. Poverty and its ills loom large and complicate the stigma of drug abuse and a criminal record. I now often reflect upon some of the narratives from the jail study and think, "A middle class person wouldn't have landed in jail." The protection of education, employment, and social support are sorely missing in most of these stories. I wonder why society cannot move away from mandatory minimum sentences and toward mandatory maximum treatment for drug abusers.

Ex-offender women trying to rebuild their lives and the lives of their children have lots of reasons to feel uncomfortable. Almost all of them are struggling with the cravings and pressures common to alcohol and drug recovery. They have problems finding a place to live, a place to work, and a place to fit into a society that doesn't have much sympathy or use for them. The words I hear in my head are different now: "I have to get comfortable as an advocate so I can make others uncomfortable about problems of incarceration and re-entry – so that changes will occur."

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The Quagmires of Conducting Clinical Research: One Team's Quest for Creative Solutions

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This narrative describes one research team's experience with conducting clinical research through collaboration with a local social service agency.

Introduction

Conducting research in collaboration with a social service agency can pose numerous challenges. Not only must research methods follow rigorous standards for recruitment, measurement, and sampling, but clinical considerations are also paramount (Resnicow et al., 2001). Intervention studies contain a variety of elements and these multiple components must have the capacity for determining effectiveness. Planning ahead for challenges in the implementation and the process of intervention delivery is highly recommended (Whittaker, Kinney, Booth, & Tracy, 1990); however, there are often many unforeseen difficulties that must be addressed throughout the course of the evaluation.

The following narrative describes one research team's experience conducting clinical research through collaboration with a local social service agency. It is our hope that some of what we have learned and experienced may help others attempting to conduct similar research. Our discussion will describe experiences through the perspectives of the major roles required to implement this study: principal investigator, research assistants, and clinicians.

The Study

The formative study described here was funded by the National Institute on Drug Abuse (5K01-DA015671) to evaluate the retention and engagement of youth and their families in an in-home, family-based intervention model. The family-based intervention was designed to address drug use and other high-risk behaviors of adolescents who had previously run away and returned to their homes. The major objective of the study was to establish feasibility for engagement and retention of youth and their families in the intervention. We partnered with a local multi-service agency that receives federal and state grant funding to provide services to high-risk youth who were experiencing family conflict difficulties. The agency agreed to allow us to provide services to a random sample of clients seeking services for family conflict and youth delinquency, truancy, or runaway behaviors.

The agency's counseling services acted as the comparison group for the study's experimental condition. The counselors were trained and utilize solution-focused brief therapy (SFBT) and offer families up to 12 sessions of family therapy free of charge. This therapeutic approach encourages clients and counselors to focus less on describing the problem and more on what life would be like when the problem area is solved (Berg & Miller, 1992). Solution building begins with asking clients to describe how they would like their lives to be different as a result of coming to treatment. Future success is the target, rather than where the problems began. Clinicians encourage clients to describe their desired outcomes and note the process of solution development throughout the course of the therapy. Focusing clients on a future in which the problem is solved is central to a solution-focused approach (Pichot & Dolan, 2003).

The experimental intervention was similar to the agency's 'standard care' in that we offered twelve sessions of solution-focused family therapy; however, the experimental intervention was delivered to youth and their families in the home. In addition, "engagement activities" were developed to strengthen the family's motivation to engage in the therapeutic process and were employed during each session. The engagement activities were designed to improve interaction and communication between the youth and their parent(s). Eighteen separate engagement activities were developed, utilized, and evaluated among the experimental families.

Agency Collaboration and Subject Recruitment

Following identification of the agency with whom to develop a partnership and the acceptance of collaboration, the first step was to develop procedures to identify and recruit eligible youth and families. This required understanding the agency's intake procedures and determining how best to incorporate our procedures into those of the agency. This took several meetings with agency staff at various levels of administration. The research team also held brainstorming sessions to ascertain the best approach for successful recruitment. While these sessions were helpful in getting the recruitment process started, the initial process was modified many times and multiple attempts were needed to discover recruitment methods that actually worked.

Initially, the research team and the agency staff agreed that informing clients about the opportunity to participate in the study should take place over the phone at the time clients were being scheduled with a counselor (at intake). It seemed a fairly simple process: when a parent contacted the agency seeking family counseling services, the intake worker would make them aware of the study and ask if they were interested in hearing more about it. If the family agreed, the worker would fill out a bright pink sheet (to ensure staff would not forget to mention the study) with the parent's name, phone number, and address. These were left in a specified mailbox for the research assistant (RA). The RA would then randomly determine the group (experimental or control) into which the family would be placed, contact the family, verify eligibility, and set up a date to conduct the pre-test.

One of the main challenges in this recruitment procedure was the fact that all agency counselors were responsible for conducting intake phone interviews. Ensuring that all counselors remembered to mention the study during intake and follow the protocol consistently became the largest barrier to recruiting families. Although various members of the research team took part in several clinical meetings to remind counselors about the study and answer questions that arose during implementation, this procedure resulted in enrollment of only eight families during the initial five months of the project.

The low enrollment rate indicated that we were simply not being able to access eligible families; therefore, these procedures were modified. Instead of the counselor's being required to remember to offer the opportunity to participate in the study during intake interviews, bright pink recruitment sheets were added to the initial packet of materials families were required to complete before meeting with their counselor. These recruitment sheets contained a description about the study and its purpose, what would be required of participating families, and consent to contact them. Interested families provided names and phone numbers so that the research assistant could call them to further explain the study and then enroll them in it. Again, this new procedure was ineffective and resulted in only two referrals during the course of two months. In addition, several families refused any further contact from the research team to hear about the study.

So, here we were - seven months into a two-year project, and we had only recruited ten families (we had proposed reaching a total of 195 between the experimental and control groups within 2 ½ years). The research team continued to explore the obstacles encountered in accessing and recruiting families. Agency administrators and counselors were as baffled by our difficulties in recruitment as we were. The number of eligible families served by the agency during the seven months was much larger then the number of referrals the study had received; therefore, it was obvious that many families were not being identified through our current procedures. Furthermore, families seemed reluctant to give their contact information based on the study description contained in the pink sheets.

Our next strategy was again developed by collaborative efforts between the research team and the agency staff. It was agreed that the best approach would be to train one of the agency's counselors to retrieve family names from the computer intake system and call *all* eligible families to offer the opportunity to participate in the study and set up the pretest date, even before the family's first appointment with the counselor. Luckily, one of the study's research assistants was also a part-time counselor at the agency and she volunteered to take on this role. These new recruitment procedures resulted in enrolling twenty new families in the study (with very few refusals) over the course of about three months.

This experience is an excellent example of the importance of maintaining a good relationship between the research study team and the agency's staff. Without a working collaboration between both parties, the recruitment procedures would have continued to fail. Utilizing an individual with a dual relationship (counselor at the agency and research assistant for the study) allowed us to bridge the barriers we had confronted. We have learned that it is critical for the person responsible for participant recruitment to be knowledgeable about the research project and agency policies, and above all, have a good relationship with key agency staff that can help open the doors to clients they serve.

Contacting and Interviewing Challenges

Once eligible family's names and contact information were retrieved from the computer's intake system, the RA/counselor would call all families before they were scheduled to attend their first session with a counselor. The purpose of this contact was to explain the study, recruit the youth and their parent(s), and set up a time to complete the pre-test before counseling sessions began. The first challenge was to contact the families. Due to confidentiality issues, a message could not be left on answering machines. Also, if someone other than the youth's parent answered the phone, the RA/counselor could not indicate she was calling from a counseling agency. Rarely was the parent available during the first attempt. More often, the RA/counselor made multiple attempts to contact the family - five attempts appears to be our average for successfully scheduling pre-tests. Conversations with the family member varied from a few minutes to more than thirty minutes. We found that to successfully recruit family, the RA had to establish rapport, explain the purpose of the study, describe what participation entailed, and work carefully to ensure that all of the individual's questions were answered to their satisfaction.

One research team member explained his first experiences calling families to recruit them and set up appointments for the pre-test:

> *"As I prepared for my first calls, I thought, "Who enjoys unexpected calls from charities, credit card companies, or other sales personnel?" What language would help put the family member at ease, communicate their options clearly, and create curiosity about participation in the study? I was familiar and comfortable with some level of marketing a product, but this felt eerily like a form of telephone sales. I have never thought sales to be a strength, or even something I would enjoy. It didn't take long to find answers to these questions and concerns. Relax. Breathe deep. Stay centered in the genuine interest in the family's well-being and desires. Take your time. When in doubt, slow down. And no matter what the outcome, express thankfulness for the family member's taking time out of their day or evening to visit by phone."*

During the initial phone contact, the RA/counselor described the study to the parent, offered either the experimental or control group depending upon randomization, and set the pre-test date. Some families reported interest but asked for time to discuss the matter with a partner or their child. The longer it took to re-contact them, the less likely these families would agree to participate. For the majority of families that were enrolled during the initial contact with the RA/counselor, the pre-test appointment was set during the first or second phone contact. Families who did not agree to participate tended to have strong reasons that impeded them from participating. For example, one family had a difficult divorce with shared guardianship of the youth, another family decided to seek counseling elsewhere due to benefits they could accrue at work, and in another family, the single parent had a health problem that made it nearly impossible to complete the pre-test instruments.

Another immense challenge was the requirement to conduct post-test interviews in both the experimental and control groups following the family's completion of counseling services. Contacting families who had been in the control group was especially difficult as the research team had had limited contact with them. For experimental families, continuing contact was less difficult as the counselor would remind the family that a member of the study team would be calling to complete the post-test. However, even among some families in the experimental group, we were unable to collect post-test information. One family moved before their twelve sessions in the experimental condition were complete and the youth ran away. The post-test was eventually completed with the parent, but we could not re-contact the youth. Another youth participant was sent to juvenile youth detention and could not be re-contacted. As in conducting pre-tests, these families tended to agree to complete post-test interviews within the first two or three phone contacts. Those that were more difficult to locate often were unwilling to complete these measurement tools.

A final challenge associated with contacting families was their frequent failure to keep appointments to complete the pre- and post-tests. Often a family member would call to reschedule, but other times they would simply not be at home when the research assistant arrived. The research team was very persistent, calling them daily or every other day until they completed the testing or the family asked to be dropped from the study. Diligence proved to be the key ingredient in successful completion of pre- and post-testing; however, the research team had to be careful not to invade the family's privacy by calling too often. This tended to be a subjective decision based upon the reactions of the parent on the phone. In sum, multiple calls, at random times, and persistence seem to be the order of the day.

Conducting research in community settings creates many challenges, not the least of which regards the interviewer's perceived and genuine safety. Entering communities that may be uncomfortable at best, and dangerous at worst, is an obstacle difficult to overcome when attempting to collect data from participants in their homes. Another important distinction should be made between situations that are perceived to be dangerous due to internalized stereotypes and those that are realistically unsafe. This distinction is difficult to make in the moment, and the advice of self defense specialists is often to "trust your gut." If it feels truly dangerous, then dedication to a research project should never override personal safety.

This process is exemplified in the following account of one research assistant's experience going into a new area to conduct a pre-test.

> *"I drive up to the big, orange, metal gate that encases the apartment complex in which the family lives that have agreed to participate in our study. The gate will not open for me; I am prohibited from parking within the apartment complex. So I find a parking spot on the street and walk the two blocks back to the apartment complex where I intend to collect my data. As I walk I cannot help noticing the economically depressed condition of the neighborhood. The streets look socially isolated from the outside world and the buildings appear to barely meet the definition of shelter. I know I am in a "rough" area of town, but that has never stopped or fazed me before. As a researcher and social work practitioner, I have willingly been in many situations that might be perceived as dangerous or risky.*
>
> *However, as I enter this apartment complex, I have an instinctual feeling of insecurity and fear- a powerfully strong feeling that I had never experienced before. I slowly start the long walk from one end of the complex to the other, trying to find "building 12". As I walk further into the gated and confined area where there is no outlet, I feel more and more trapped. I am confined in an environment that is not my own and in which I obviously do not belong. I realize that as a white, middle-class, female, wearing a backpack, and looking lost, I obviously do not fit in with these surroundings. The stares and curiosity from the residents escalate as one group of men standing by a corner of one building start whistling and calling out to me. Not knowing how to react, I look straight ahead and try to appear as if I know what I am doing.*
>
> *Upon finally finding the respondent's apartment, heart beating rapidly and palms sweaty, I knock on the door. No answer."*

As a research team we have worked to determine how best to handle these situations. Questioning personal stereotypes at debriefing sessions has been invaluable for the entire team. Continuous sharing of experiences by members of the research team contributed to identifying a variety of strategies to improve safety. First, communication between the research team members about where and when interviews were being conducted was identified as extremely important. We decided that an appropriate initial strategy was to call another research team member when arriving at a home that seemed uncomfortable to "check in" and let them know the start and approximate end time of the interview. In these cases, the other team member would expect another call within an hour or so, the time it typically takes for data collection to be completed. As a result, other team members would be aware of the interviewer's whereabouts and expect a call to know that things had gone as planned.

Second, enlisting the help of the study participants was identified as a way to increase feelings of security. Asking the study participant to meet at the interviewer's car and help the interviewer find the way to their apartment increased the likelihood the participant was home when the interviewer arrived, and also addressed the concern of walking into an unsafe situation alone. We found, however, that this request must be discussed sensitively, without offending the participant. The interviewer never suggested the neighborhood is "unsafe," "in a bad area," or "socially disadvantaged," just simply that they needed additional assistance in finding the participant's home.

Third, we determined that meeting respondents in a neutral, safe location, such as the fast-food restaurant near their home, was an appropriate method for collecting pre- and post-test information that was in written form and required little private conversation. We have found that some participants, especially those in the control group who had little interaction with members of the research team, preferred this as well. Recognizing the role of the researcher and the challenges this role can create is important in implementing a research project in the community. Working as a supportive research team is critical in increasing comfort and safety for the interviewer while respecting participant's needs.

From the perspective of the principal investigator and the ultimate manager of this process, it seems that effective research assistants are those who are enthusiastic about the project, are knowledgeable about and comfortable explaining the study clearly, and are respectful, non-judgmental, and personable with prospective participants. Most important, though, is the ability to persevere and remain motivated to continue working through the challenges that seem to appear at every turn. This project's success is due in large part to the tenaciousness of the research assistants involved in this study.

Conducting Interventions in the Home

Conducting clinical research with youth and families in their own homes leaves one open to all kinds of possibilities, outcomes, dilemmas, and experiences far beyond the variables considered for study. It is this opportunity for unanticipated variation that makes research in the community difficult but also leads to rich and interesting clinical experiences. Providing counseling services in an office is fairly contrived and consistent as the environment is controlled by the clinician and circumstances are generally the same from session to session. However, conducting therapeutic sessions with families in their homes is seldom consistently the same, often amusing and opportunistic, and always challenging. Frequently, in-home sessions create a process far beyond that intended in typical counseling sessions.

Clinician Challenges

From the perspective of clinicians in this study, a variety of challenges had to be overcome. One example is the space in a client's home in which to conduct the session. Unlike office settings, this was often variable. When many people live in one home, there was often a shortage of private, quiet spaces. For example, one family had nine people living in a three-bedroom home with only a kitchen and living room as shared space. During sessions with this family, there were often several small children running around, and almost always someone in the living room watching television. If the family members who were part of the therapeutic session chose to sit in the living room and turn the TV off, the other family members often sat and listened in on the session. This close observation made it extraordinarily difficult to conduct confidential conversations with the participant family members.

Given the flexibility needed to conduct in-home interventions, the clinicians for this study developed their own unique strategies for dealing with challenges. At times they have conducted sessions in the car, creating a portable private office. They have talked to many parents and youth on the front porch or in the back yard. In other households that have a variety of spaces to meet, the youth and parent typically choose the specific space in their home. This often provided the clinician with insights into how the family member participants were feeling. For example, in one family, the parent and youth would always choose to sit at the kitchen table when they were angry with each other, but when they felt more calm and had better communication, the sessions were conducted in the den.

One of this study's clinicians described it this way:

> *"It is a dance of using every moment and opportunity to make the most efficient and effective use of the availability of space and time. Unfortunately, I sometimes trip in that dance, and there is plenty of opportunity for awkwardness or having to make a quick change in plans. In an office, a therapist generally has full control over space and pace. In someone's home, a lot of that control goes out the window."*

Other challenges come in the form of distractions – situations that would likely never happen in an office setting. Distractions come from all directions when conducting sessions in a client's home. Some can be managed and changed; others must simply be tolerated. The distractions in people's homes can be just that, distracting; however, they can lead to discussions of more important matters, are entertaining, provide a release when discussions become tense, and form a bond between the clinician and the family. The dog that sleeps on one clinician's feet seems to have adopted her as the Tuesday night guest – a highly unlikely happenstance in an office setting.

Some distractions can be managed and dealt with, such as the family who lived in an efficiency apartment that did not have electricity when the clinician first met them. The initial session was conducted in the dark. After that, the clinician always brought large candles and matches to subsequent sessions. Even in this situation, however, the clinician remarked at the family's dignity and their motivation to continue their therapeutic sessions despite difficult circumstances.

Television is often a distracting part of family life. Some families kept their TVs on, while others responded to requests to turn it off. For others, muting it but leaving it on is their choice. One family said they left it on because it kept the dogs calm, and "Trust me, you want us to keep the dogs calm." The TV stayed on.

Younger siblings and relatives are almost always a distraction, or at least their trail of things is a distraction – the spilled milk on the table, the Popsicle sticks on the floor, all of the toys in various stages of play. How younger children are managed reveals a lot about family dynamics. One family's three-year old child always greeted one clinician at her car, gave her a hug, escorted her into the home, and introduced her to everyone (again). Another family's three-year old insisted on doing all of the "engagement activities." The clinician noted that the youth client was more patient and caring in response to that child than in any other aspect of her life. Viewing the interactions of the family with each other, within their own environment, can lead to many more insights than is often possible in office-based sessions.

And then there are the animals - the hamster in the living room that drank water incessantly and made odd noises - the three kittens that climbed all over the clinician and snuggled together on his lap - the dog who slept on top of the clinician's feet under the kitchen table each time he met with the family. Animals are usually considered part of a family, and observing how families and their pets interact can not only be entertaining, but revealing as well. Having animals around can be distracting, but they can also add humor to tense moments and provide great conversation, often crucial to building rapport.

Providing therapy in individual's homes is a little harder to conduct than is typically the case in scheduled hour-long sessions in an office. In the home environment, where the clinician has little control, there are many opportunities for unintended developments. This fact makes it more difficult to pace sessions. Some families answer the phone during sessions, and if there is an important call, they stop the session to take the call. This can change who participates in the session and the topics of discussion. In addition, even if the clinician attempts to prioritize and achieve agreement between the participants concerning the focus of the session, the clinician is "on their turf" and has an obligation to respect their priorities, regardless of what the clinician may request – a very different dynamic than found in an office setting.

Clinician Insights

The clinicians conducting in-home sessions for this study have noted that families seem to feel a different obligation to the therapeutic process and the counselor than those families who come into an office setting. Families appear to have a different sense of control; the clinician is in their territory, not the other more typical arrangement in office settings. Clinicians in this study often had to defer to family's schedules and activities more than the family seemed willing to conform to the clinician's schedule. This can appear, and in some circumstances may reflect, a lack of value attributed to the therapeutic experience by some family members. Although most families seemed to appreciate the time and efforts made to meet them in their own environment, many families seemed to expect more flexibility on the part of the counselor than they might expect in office settings.

Scheduling can be a challenge when a counselor goes to clients' homes. It typically had to be in the evening or on weekends, especially when youth participants were in school. Even when schedules appeared to meet the demands and needs of the family, sometimes they would forget appointments completely or were late arriving home. This often left the counselor sitting in a car or on the door steps - waiting. If families did not have phones, it was difficult to remind them about appointments. But no matter what the clinicians in this study expected, they went to the family's home, hoping someone would be there. If nobody showed, at least they got to know another part of the city and to observe a part of the family system (the neighborhood, apartment complex, neighbors) that they typically miss. Always attempting to view experiences from a 'strength-based perspective,' the clinicians in this study took advantage of whatever opportunities presented themselves. That was also part of the positive experience of visiting clients in their homes - the ability to observe them in-context. Although it was not rare that a family forgot or was not home when the appointment for an in-home session was scheduled, sometimes even that irritating experience provided a unique opportunity. One experience exemplified a clinician's taking an opportunity when it presented itself:

> *"One session I showed up at the client's home and the client and mother had forgotten, but the mother's boyfriend was there. He apologized for them and located them, but they were in opposite ends of the city and did not have a way to get to their home in a timely manner. I was ready to leave, but the boyfriend started to tell me all about the family and how things like this happen all the time. He proceeded to tell me about the relationship between the client's mom and her mom and how that affected the relationship of the client with his grandmother. This is information I most likely would have never received, at least not from an outsider, and almost never would I have reason to invite an outsider to a session or request consent to talk to mom's boyfriend to get additional information. But there it was, thanks to a no show. Again, I had to be careful about how I processed that information, and I categorized it as outsider information from a boyfriend who was mad at his girlfriend, but it was interesting data that added a new light to some observed dynamics."*

Other experiences have occurred when the parent was present, but the youth was not. In these instances, it seemed the parent was relieved to have the counselor's full attention so she could receive additional support and have time to talk candidly. Because creating and finding private time and space for individual work with family members was often difficult, these unplanned moments of privacy were sometimes crucial to moving the therapeutic process forward.

From the principal investigator's perspective, it is critical to find clinicians who are creative, invested in the therapeutic and research process, intrinsically motivated to help youth and parent(s) who are struggling, and have a high degree of flexibility. As was identified as the most important characteristic for research assistants and interviewers, clinicians also must be highly motivated to work through the numerous challenges of doing this type of therapeutic work and possess a high level of innate perseverance. Driving an hour to visit a family who is a "no-show" requires not only immense patience, but a constant focus on the strengths of the families they serve.

Lessons Learned

Conducting this clinical-intervention study in collaboration with a community agency and delivering our intervention in client's homes has been extraordinarily difficult, frustrating, informative, and rewarding. This type of research is not for the faint of heart, the unmotivated, or the inflexible as so much can happen within the unstructured boundaries within which this work is conducted. However, it is not simply challenging; the challenges have often led to treasured experiences, relationships, and plenty of entertaining stories.

As we continue our work on this study, we understand that many more challenges lay ahead. However, the opportunity to assist families who are struggling with a variety of challenges makes the effort worthwhile. Social work as a profession has long been concerned with strategies for effectively helping multi-problem families (Whittaker et al., 1990). A critical task for assisting families facing a myriad of challenges is to discover interventional approaches and examine their application and effectiveness. Thus, even with the challenges associated with conducting this clinical research, the desire to increase families' functioning and improve parent/child relationships motivates us to continue.

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The Rise and Fall of a Bad Idea: A Research Narrative

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This narrative details a research process from the development of hypotheses from qualitative data through the testing of those hypotheses in large scale quantitative survey research. Discussed are the value issues involved with the conceptual framing of research questions to be congruent with the social work paradigm.

Consumers of social work research – readers of reports, journal articles, books, and the like –get a sanitized product that seems to be the result of a linear scientific process. It appears that the authors established a research question based on gaps in previous research, converted the question into measurable hypotheses and variables, then tested the hypothesis through the application of specific universal rules of research. In fact, research results emerge out of a messy, iterative, sometimes irrational process. The cliché is that no one should be exposed to what goes into the production of sausages or legislation; the same can be said for the production of research articles.

I still remember my shock when, as a beginning doctoral student, I was first exposed to the reality of the development and publication of social work knowledge. In our first-year socializing seminar, in which prominent social work research faculty were invited to informally discuss their work, we were informed that a successful researcher begins with a table of whatever statistically significant results were derived from the empirical process, then works forward and backwards from that. In other words – in a startling departure from what we were being taught at the same time in our research classes – the literature review, the research question, the underlying theory, and certainly the conclusions and discussion were developed only after the statistically significant results were clear. In another course, an eminent sociology professor referred to the text surrounding the Findings section as "dither" – a packaging task designed to make the article ready for consumption.

The above is an extreme view. In reality, of course, researchers bring to their work a knowledge of the issues in the literature of the field; the research questions and instruments are constructed based on that knowledge, and variables are chosen and measured based on prior judgments as to what is important and constructive to the field. Nonetheless, the finished product of the research process does obscure the messy reality of the process.

At the least, there are numerous unanticipated instances where researchers need to guess at what should be clear choices. What about the survey respondents who insist on placing their marks between two of the five offered responses? And skip pattern responses that make no sense - the respondents who indicate that they are not a member of a union, then go on to report a high level of union activity? The meaning of barely audible audio tapes? Did he say he "did" or "didn't" mean to hit that child? Fuzziness in the sampling frame - the new hire who began the job as an intern, but was not officially hired until later?

A larger issue is the representativeness of samples. Here we hope that readers will not dwell on our brief acknowledgments that our respondents (whatever the response rate) surely differ from our non-respondents in important or unknown ways, or, in qualitative studies, that our small sample of convenience may have little in common with larger populations of interest.

Another major area of fuzziness in social work research is the process of model building. Questionnaires are packed to the maximum possible length with measures of any variable that previous research, theory, or simple hunch indicate might conceivably have a statistical relationship with the outcome. The variables are then analytically reduced to those that, in fact, have such a relationship and packaged as a predictive model of the outcome. Even the most by-the-book researcher will not hesitate to abandon prior hypotheses to adopt post hoc theories that provide a context for the statistically significant results actually obtained. Failures to disprove the null hypothesis do not get published. Nor would any successful researcher hesitate to re-frame a set of conclusions to make them more appropriate for a given journal.

This narrative seeks to elucidate some of the messy research processes that lie behind all of the crisp and clear research articles that appear in our journals. In examining a specific empirical sequence from my own career that is more typical than unique, I talk about some of the decisions faced in the real research process, and some of the ethical dilemmas that inform these decisions. In addition, this is a story of a dead end in the research process, a frequent outcome of our endeavors that never see the light of publication.

The Inductive/Deductive Cycle

This narrative focuses on an instance of qualitative data collection, theory generation, and quantitative theory testing. One of the principles taught in our social work research classes is the inductive/deductive cycle of scientific reasoning. The presentation of this process, which is fortunately quite amenable to clear and instructive diagrams, is a staple of our research texts (e.g., Monette, Sullivan, & DeJong, 2005, p. 29; Rubin & Babbie, 2005, p. 52). Theories, or hypotheses, about the relationships in the world among some set of related concepts are developed through observation of the world, exploratory qualitative research, unanticipated conclusions from quantitative research, or pure thought. These hypotheses are then tested through more explanatory quantitative research. The results of these tests, in turn, become material for the development of further testable hypotheses. One of the assets of this conceptual presentation of the development of social knowledge is its integration of qualitative (exploratory, theory-generating) methods with quantitative (explanatory, theory-testing) methods, appropriately emphasizing the roles and strengths of each approach. Tashakkori and Teddlie (1998) categorize this combination of qualitative and quantitative methods as a "sequential mixed method design" (p. 46).

The inductive/deductive research cycle has some validity as a macro picture of the scientific research process of an entire field of endeavor; as ideas are tried, bad ones are discarded and good ones are kept and built upon, even though the disappearance of some ideas and the persistence of others may owe at least as much to political and cultural factors as to purely scientific ones. The trial-and-error approach is less accurate as a description of the work of individual researchers, mostly due to the specializations chosen by researchers. Most social-science researchers are specialized by method, which limits their participation to only one aspect of the entire inductive/deductive process. In addition, we become committed to a specific theory, measurement issue, or narrow field of inquiry. As a result of these specializations, most researchers do not engage in full cycles of the inductive/deductive process. This brings a looseness to the macro research system as some researchers generate theories, which may or may not be chosen for testing by other researchers, while some researchers report on (successful only) tests of specific theories with little thought given to generating further hypotheses for exploration.

The Retention of Public Child Welfare Workers

The research topic underlying this narrative is the retention on the job of public child welfare workers, especially Title IV-E stipended workers. A statewide shortage of social workers is being experienced and is expected to get worse (O'Neill, 2000). Public child welfare is facing its own acute shortage of social work personnel. Statewide there were 6500 public child welfare positions funded for FY 2000/01, yet there was a need for twice that many to meet minimum standards and three times that many to meet ideal standards (American Humane Association, 2000). Turnover has chronically been a problem in public child welfare (Council of Family and Child Caring Agencies, 1989; Helfgott, 1991) and a number of studies have explored retention in child welfare (e.g., Fryer, Miyoshi & Thomas, 1989; Rycraft, 1994; Vinokur-Kaplan, Jayaratne, & Chess, 1994). Title IV-E, with the dual goals of increasing the number of workers entering public child welfare while increasing the level of education of these workers, represents a significant investment in the education and training of professional child welfare workers. In California, 200 M.S.W. graduates per year are added to the pool of child welfare workers (California Social Work Education Center, 1999). However, the retention of workers who received stipends through Title IV-E has itself become an issue (Dickinson & Perry, 2002; Jones & Okamura, 2000). In Los Angeles County, about half of stipended workers leave DCFS within two years of beginning work (Furman, 1998), a rate similar to that of non-stipended new hires. In San Diego, though, IV-E workers were more likely (89% retention) than other workers to remain on the job (Jones & Okamura, 2000), and the one statewide study to date found a 76% retention rate (Dickinson & Perry, 2002).

Generating Hypotheses

Leaving my doctoral program as an "expert" on welfare reform, I quickly re-cast myself as a researcher in the field of child welfare because at that time money and jobs were available through Title-IV funds. I was hired by a local school of social work to examine the effectiveness of their Title IV-E stipend program. As there was no specific mandate to the research, we began with a general discussion among program staff about the direction of the research and quickly chose to focus on an examination of the factors that impede or contribute to the retention of these workers. We established a longitudinal approach, annually interviewing in depth cohorts of IV-E students, initially as they entered the required internship program (could be first or second year of the M.S.W. program), and subsequently as they graduated, on their first-year work anniversary as they confronted the choice of continuing employment past the mandated one-year commitment, and annually thereafter whether or not they remained with the local DCS. Apart from our guiding research question of identifying the factors that influence remaining on the job, our approach was entirely inductive (Glaser & Strauss, 1967; Strauss, 1987). Though we structured the questionnaires around obvious general areas – family, work experience, M.S.W. education, etc. – we anticipated that the relevant factors would emerge from our interviews. Since our first round of interviews were with students who had not yet begun their employment, our questions addressed issues of preparation for a career in child welfare. We were then able to trace these themes through the early stages of their child welfare careers.

Interviews were conducted face to face at the internship or at the office, written down on the spot, then immediately typed by the interviewer. Interviews were conducted by me and by a number of doctoral students. We paid careful attention to possible interviewer differences. As a group, we analyzed sets of interviews among a cohort at a point in time. Interviews were coded based on the interview guide, while researchers remained open to emerging topics. The list of codes developed over the time of the project. All coded text was moved to separate files per code to form a data matrix by topic. The text within each file was reduced to derive a summary of each substantive category. Categories among each category were derived from the data. Categories across respondents were examined to identify patterns of responses for subjects (Miles & Huberman, 1994).

Over time our pool of subjects became large enough (68 beginning work with DCS) that we were able to do some simple statistical comparisons. To accomplish this, nominal variables were developed from the qualitative categories; interviews were re-visited and coded quantitatively (Boyatzis, 1998).

The data on the lives of students prior to beginning social work school revealed the presence of a certain path to social work as a profession. The interns began their paths toward social work by perceiving a social need in their immediate environment that had a direct impact on them. They then became directly involved with the relief of these needs through personal responses, volunteer work, or paid paraprofessional human service work. These positive experiences led to the search for further education to increase their capacity to address these needs. In this search they were led to social work either by supportive families or through contact with professional social workers.

The initial exposure to social needs was categorized into either direct or indirect exposure. Direct exposure was further categorized into personal problems (those that occurred directly to the subjects) family problems (those that occurred within the families of the subjects) and community problems (those that occurred in the communities in which the subjects were raised.) Indirect exposure was through a volunteer or work setting; these were older students, approaching social work as a second career.

We saw that these categories related with views among students of their upcoming child welfare professions. Students with personal or family problems took a wait-and-see attitude toward the job. Those students with a background of exposure to community problems reported an intention to stay on the job and to pursue administrative and policy positions. Students with an indirect exposure to social problems expressed the strongest determination to remain on the job.

As it turned out, these categories of early exposure to social problems were related to remaining on the job but not necessarily as predicted by the students. Those students who reported direct early exposure to social problems, especially personal problems, were more likely to remain on the job. The greater the exposure, the greater the likelihood of remaining on the job. Of those who reported a history of personal problems, 68% remained on the job after one year compared with 46% of those who did not report such a history. After totaling the number of reported problems per subject across categories, 39% of those with no problems remained on the job after one year, 52% of those with one or two problems, and 70% of those with three or more problems.

These conclusions, of course, remained suggestive. Statistically, given the small sample size, the validity problems of variable development and the lack of any multi-variate analysis (work experience, age, and satisfaction with pay and benefits were also strong predictors of retention), there was not a strong foundation here. Nonetheless, given our lengthy and in-depth involvement with these data, we were certain that something was going on here.

Needless to say, this conclusion – that those social workers with troubled backgrounds were more likely to remain on the job than those without – was completely unexpected and quite troubling. In and of itself, this was not an acceptable conclusion. The immediate association was between troubled workers and troubled organizations, an association which came quickly to mind among those many educators and workers who had become cynical about the possibility of a satisfying career in public child welfare. As one colleague said, "Of course, these people stay; they're sick and DCS is a sick organization."

For this reason, we were not willing to report this or even talk much about it until we could frame it in an acceptable manner. It is interesting that the need to re-frame this conclusion in a more acceptable manner was immediately apparent to us; there was no question but that this was our next task. We could not proceed with the proximate understanding of this perceived relationship.

We went back to the data to look more carefully at the sequence from exposure to childhood problems to coping on the job. We saw that these students had experienced certain types of responses to these problem experiences. They reported overcoming and transcending these problems with the help of community networks, supportive families, strong adult role models (usually mothers), personal therapy, and church attendance. We came to understand the concept from a strengths-based perspective. These people, through their childhood trials, developed strengths that could be converted into tactics of survival in a difficult bureaucracy. I hypothesized that while many people suffer in childhood, those that go on to successfully cope with these problems and overcome them are the ones who go into the field of social work.

We went on in subsequent interviews to examine the behaviors of graduates who remained on the job and were professionally satisfied in public child welfare. Among these behaviors were an acceptance of personal responsibility, flexibility in ways of understanding themselves in relation to their jobs, assertiveness in changing their work conditions and the agency, and an ability to establish large networks of support and communication among peers and other supervisors.

We had the beginnings of a grounded theory. Exposure to personal family problems leads to actively addressing those problems, which leads to involvement in social work, as well as to increased ability to deal with difficult, ambiguous, and contradictory settings such as large bureaucracies. In this way we can have a positive understanding of how and why social workers with traumatic backgrounds may be more likely to remain on their child-welfare job. The example used was the member of a family with an alcoholic father (a situation of many of our respondents) who was compelled to develop in childhood alternative methods of communicating and developing sources of information and support. This translates into the ability in a large bureaucracy to bypass formal communication channels.

Developing Quantitative Measures

The next step in my academic career was as a full-time grant-funded lecturer at another social work school. Though my primary research had gone in a different direction, working with M.S.W. students on their theses afforded me the opportunity to pursue some of the concepts pertaining to the paths that social workers take to their careers. It is a truism that social workers are likely to have come from problematic childhoods, and there is a body of literature that supports this. It has been found that the families of social workers are more likely to include unhappily married parents, members with emotional problems (Rompf & Royse, 1994), victims of violence (Black, Jeffreys, & Hartley, 1993; Russel, Gill, Coyne & Woody, 1993), the physically ill, and deaths among family members (Black et al., 1993) than students pursuing degrees in English (Rompf & Royse, 1994), guidance and counseling, education (Russel et al, 1993), or business (Black et al., 1993; Russel et al, 1993). Social work students are more likely than other students to have been victims of child abuse or neglect, or to have had abuse occur in their families of origin (Black et al., 1993; Rompf & Royse, 1994; Russel et al., 1993). The predominant finding among these studies is the prevalence of alcohol and drug abuse among the families of these students (Black et al, 1993,; Hawkins & Hawkins, 1996; Marsh, 1988; Rompf & Royse, 1994; Russel et al, 1993). Though Hawkins and Hawkins (1996) establish a relationship between past contact with alcoholic families and present mental health problems among social work students, little in these studies connect problematic childhoods with adult concerns. There is little to indicate the relationship between early traumatic experiences and the choice of social work as a career, and little about differences between social workers with past problems and those without past problems.

Together with a group of M.S.W. thesis students, I put together a study comparing 79 entering M.S.W. students with 37 entering M.B.A. students (overall response rate of only 45%) to replicate studies comparing social work students with others, to develop items to cover the range of problems identified earlier – personal, family and community – to measure the degree to which they sought help, and to connect these experiences with choice of career. The sensitive items about past personal and family problems were incorporated into the overall flow of a long survey, and there were no missing data problems with these items.

Social work students were more likely to have experienced personal problems than were business students, especially emotional problems, and more likely to have experienced problems, especially emotional and marital difficulties, in their families of origin than were business students. There were no differences in the rates of experiences of community problems. There was some progress in developing measures. We were able to see which items were meaningful or ambiguous and which achieved a sufficient number of positive responses. And we were able to develop factors which indicated that typology of personal/family/community may not be the best way to organize the items. For example, we derived a family violence scale, a death and illness scale, and an emotional problems scale, each of which included items from both family and personal sections.

It seemed to me that there was an important gap in the grounded theory relating childhood problems to job retention. If positive responses to past difficult situations led to adaptive behaviors on the job, these past experiences should have led to the development of personality traits which could be measured but not observed through behaviors. To this end, we included measures of locus of control (Lefcourt, 1991), but found no correlations between this measure and the occurrence of past problems.

Testing the Theory

Next career stop was a tenure-track position at another social work school. This put me in the middle of a new collaborative project to measure the retention of Title IV-E students from all local social work schools.

The issue of retention continued to matter, especially because of the ongoing investment of funds in the Title IV-E stipend program, and continued to be a source of research funds. Now it seemed there would be an opportunity to quantitatively test some elements of my grounded theory. In the context of an ongoing longitudinal study, we put together a model to test predictors of retention. I proposed adding a shortened version of my items to the model. This proposal was vehemently rejected by our collaborative research group for ethical reasons.

The objections, along with my responses, were:

Objection: We should not include an untested non-standardized item.

Response: How will we move knowledge along if we are limited to already validated measures?

Objection: People won't answer these questions.

Response: They answer questions like this all the time, especially among professional populations. In organizational research, questions addressing issues such as the quality of supervision available are in fact more dangerous to respondents than items about personal lives. In my experience it is queries about salary that are most likely, among all the difficult questions we ask of subjects, to go unanswered.

Objection: People shouldn't have to answer these questions. We don't have the right to pry.

Response: It's accepted practice in social work research to address personal issues where relevant. As in any study, respondents were made aware of their right to not participate and to decline to answer particular questions.

Objection: DCS will use this information against applicants.

Response: DCS already uses a personality test to screen applicants. It's not reasonable that they would add a program to recruit those with a history of personal problems.

Objection: Conclusions from this theoretical approach will not be useful. Conclusions about issues such as salary, workload, quality of supervision, etc. can be used to actually improve the situation for both workers and the agency. But how can insights about the past experiences of workers improve the situation?

Response: There is considerable merit to this argument. Conclusions of this nature will be less immediately useful. However, there may be some benefit from identifying adaptive job behaviors that lead to increased retention. In addition, there could be some larger benefit derived through the insight that it is the positive response of social workers to childhood difficulties that is beneficial on the job.

Objection: Connecting past personal problems with present job behavior violates the value base of social work by putting the blame on the individual for such things as the advisability of remaining on a job. We should not look at the individual to explain behavior that occurs in a social and political context. It makes more sense and is more useful to look at the system itself, rather than at individuals.

Response: This last argument was profound, caused me much introspection, and led to a number of productive conversations with both doctoral and Master's students involved with the research. I was disturbed by the way that the practical exigencies of the research process funneled us toward a focus on the individual, in particular on the weaknesses of individuals. Deficits are easier to measure and identify among individuals than are strengths. Also, individuals are much easier subjects to work with than are organizations, communities, or policies. I became painfully aware that my research had fallen prey to the same value/practical dilemma that plagues social work as a profession. In spite of our professed intentions, we focus on the problems of individuals rather than the deficiencies of systems because, for the most part, it is simply easier. Individuals are accessible, understandable, and changeable, compared to systems.

Another way to look at the situation was in terms of the classic reductionist dilemma, always a problem for social science research, which examines individuals in contexts. In my research, I had stumbled across a generalization about individuals and was now applying this generalization about individuals to a phenomenon of an organization. Is high turnover an attribute of organizations, or is it the sum of individual decisions? Are we concerned with why individuals choose to leave a job, or with why agencies have high or low turnover rates? This reductionist problem is indeed both an intellectual and an ethical dilemma.

In my own internal struggle, I was able to marshal two arguments (one noble; one self-serving) in my defense. The first was articulated by a friend: One has an ethical obligation as a researcher to pursue interesting and potentially important intellectual issues in spite of the above considerations. It is a violation of the ethic of the objective pursuit of knowledge for a researcher to abandon a potentially fruitful line of inquiry because of speculated potential misuse of the information by others, because there may be no immediate apparent usefulness, or even because of possible violations of the value preferences of the profession. Though this argument was comforting, it was the second argument that was persuasive. I thought I was onto something interesting here, with the potential to propel my career forward. In spite of my sympathy for the motives behind some of the above arguments, I saw nothing unethical enough to prevent me from continuing.

I was able to create a situation where I could quantitatively test the proposed relationship between childhood problems and job retention, by successfully responding to another RFP to study the retention of child welfare workers. This statewide study complemented the above local study in a number of ways. While more restricted in scope time wise, it broadened the geographical scope by inclusion of a statewide sample. This allowed us to include local economic conditions as predictors of turnover. The hypothetical model included personal attributes, organizational factors, and local economic factors. Thus, the research question directly addressed the issue of the appropriate level of intervention for turnover. Perhaps it's neither an individual or organizational question, but rather a factor of the local job market, salaries, and cost of living. Included in the questionnaire were my items about personal past experiences. There were no objections from this group of collaborators; no one noticed anything inappropriate at all.

Potential subjects for the study were all new public child-welfare workers hired in the state between April 2000 and April 2001. Survey data collection began in July 2001 and continued through April 2002. Over 1700 surveys were mailed. About 10% of these were inappropriate, e.g., went to individuals who were not child-welfare workers or who were not hired during our time period. With 519 active cases in the final data file, this is a response rate of approximately 34%.

A factor analysis on eleven items addressing specific childhood problems resulted in three scales that roughly captured our original distinction among community, family, and personal problems. Male, minority, and lower SES respondents were more likely to come from a background of community problems. Divorced respondents were more likely to report personal problems. Those workers who reported high levels of childhood personal problems reported low values on a number of organizational variables, including peer support and job satisfaction, and high levels of role conflict. These findings directly contradicted my hypothesis that these childhood experiences would translate into bureaucratic survival skills. More to the point, there were no differences on these childhood problem measures between those who left the job and those who stayed.

Discussion

There are several aspects of this narrative to be highlighted. The first is simply as an example of the process of research with its many dead ends, often with conclusions that never see publication. The second is how the research that is carried out is dependent on the financial and institutional support that is available at a particular time. Few of us are in a position to directly pursue the research questions that capture our imagination. Rather, we respond to available RFPs and attempt to integrate our own research questions with the funded mandates. Third, research is a social, collaborative process. Though the research question discussed in this narrative was of interest only to me, the research process was always shaped, if not determined, through interaction with colleagues.

Most important, however, are the ethical and value issues that needed to be resolved as part of the inductive/deductive process. The strongest lesson for me was in the way that we could not proceed even to publicly discuss our initial findings until we had conceptually re-framed these findings in a manner consistent with the values of the social work profession. The notion that "sick" workers are best suited to survive in "sick" organizations was not a possibility to be seriously considered. Our re-framing was driven not by considerations of validity, but by the requirement for our research to fit with the prevailing intellectual and value paradigms of our profession. In doing research we accept without question results that we expect from our data – results congruent with our professional paradigms – while we diligently search for measurement or analysis errors to account for our unexpected or unwanted conclusions, or are led to a re-interpretation of the conclusions. It is a sobering thought that the conclusions of social science research (perhaps even basic research) are always nested in the value paradigms of their professions, if not explicitly than through the kind of self-censorship in which we engaged in this project.

If the above can be seen as driving the conclusions toward a positive congruence with the values of social work, the second striking aspect of our experience on this project, as was pointed out by our critics, drove the research in a manner not congruent with social work values. This was the way that the course of the research tended toward a focus on individual problems. It is the value of social work that our research include considerations of macro settings and macro factors, yet it has been my experience, with this project and others, that the course of least resistance tends to lead us to micro measures and interpretations. On one hand, this is a failure on our part to properly frame our research questions. The research question here was, "What are the predictors of worker retention?" rather than "What are the characteristics of agencies that have a high rate of retention?" On the other hand, it is because of the advantages of measuring phenomena at the micro level. Passing out questionnaires is social work's default research method. To answer the second question would require a substantial number of independent child welfare agencies, as well as agency-level measures that are not simply aggregates of individual responses.

My idea of the relationship between negative childhood experiences and turnover on the job turned out to be a bad idea. Now it is a discarded idea, never to appear in a model of turnover. Was my time wasted? No. How can knowledge proceed if we aren't free to pursue new ideas? Though it is a peculiarity of our research system that the lack of a relationship between early life experiences and worker retention will not be published. Anyway, it's the difficulties and barriers overcome during the messy research process that ultimately provide the sustenance and the pleasure. Research, like life, is a process, not a destination.

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Influencing Policy within the State Child Welfare Agency with (Less Than) "Perfect" Research

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Asked by the state child welfare agency head to conduct "perfect research" on the educational and training needs of child welfare case managers, the authors met with methodological challenges. The article tells the story of their search for relevant information despite multiple compromises. Using advisory groups, focus groups, online and mailed surveys, and administrative data provided triangulation of diverse stakeholders' viewpoints. In the end, using mixed methods resulted in valid findings that were confirmed and supported by the state administration. With the research and help from advocacy groups, the agency changed its hiring and training policies.

In Indiana, a high school diploma plus six years of experience with children and families qualified an individual for a state position as a public child welfare case manager. The job requirement of six years of combined education and/or experience qualified an experienced home, day-care provider or foster parent for employment. In the 1990s, state administrators attempted to upgrade the case manager position by changing the hiring criterian to include a college degree. This request was rejected by the state personnel department based on a federal requirement called a "bona fide occupational qualification."[1] The state personnel department interpreted the bona fide occupational qualification requirement to mean that the case manager hiring criteria could not be changed without empirical backing to support the change.

In an attempt to professionalize Indiana's child welfare system and earn national accreditation, the state agency administration contracted with our school of social work to answer the research question, "What are the educational and training needs of child welfare case managers?" Our goal was to obtain the empirical evidence needed to identify the educational backgrounds that state personnel should require when hiring case managers. The state agency director asked us to conduct "perfect research." Perfect research was defined as "research with findings that no one could dispute."

The charge to design "perfect research" was daunting and we were intimidated by the prospect. We followed Thomas' (1998) advice to be flexible methodologically and let the methods fit the research questions, rather than the reverse. We chose a multiple methods approach.

This article presents some of the challenges involved in the research process, along with the outcomes. Though each of the research methods undertaken resulted in challenges and flawed findings, using multiple methods led to consistent results across samples and data collection methods. We believe that we concluded the project with valid and relevant findings. The findings influenced both practice and state policy.

Review of the Literature

Minkin (1997) suggests that research can be a "profoundly bewildering and disturbing experience" (p. 2). Researchers often base their methodology on established principles and well thought-out standards used by previous researchers (Thomas, 1998). The reality of research, however, is that some flaws are inevitable. Research can be... "confusing, messy, intensely frustrating, and fundamentally nonlinear," unlike the organized product provided in published articles (Marshall & Rossman, 1995, p. 15). Rather than denouncing flawed research, some social scientists see it as necessary for knowledge building (Feyerabend, 1993; Mellor, 2001). Feyerabend (1993) asserts that violating canonical methodology advances science. Mellor (2001) agrees, stating that "total 'error' elimination is perhaps not possible and probably not efficient; it may be better to reconcile oneself, for instance, to repeating a number of items, rather than aiming for 100% accuracy the first time in all aspects" (p. 468).

An increasingly popular social science research methodology, attractive in its ability to address research flaws, is triangulation. The term, adopted from surveyors, describes a technique that develops a more complete picture by viewing data from multiple vantage points (Padgett, 1998). Mixing quantitative and qualitative methodologies is a type of triangulation frequently adopted in social science (Padgett, 1998). Social work authors on research methods have recently promoted the use of the multimethod approach (Bisman & Hardcastle, 1999; Grinnell, 1997; Sherman & Reid, 1993). Roth and Mehta (2002) promote the combining of quantitative and qualitative methods for the discovery of objective truth and multiple subjective truths.

Competing Demands

We acknowledged, at least to ourselves, that our research process would be flawed, messy, and perhaps bewildering. We did take heart in the current movement toward alleviating the flaws in one method by adding a complementary method. However, as we planned the methodology, we soon became aware of feeling pressured and torn in several directions. Our academic institution strongly emphasized the need for faculty to acquire grant income. It was important to our department that the project be successful, not just for the current monies but also for future funding. Plus, we both needed more publications for tenure and promotion. How could we conduct a study that would provide the funder with the needed information, fulfill the requirements of the peer-review process, and not criticize the agency so much that agency personnel were offended? In addition, we could not understand the agency director's agenda for asking for the research, since the results would probably require that more money be spent and the state was already strapped for funds. Nevertheless, we did not want to appear either unqualified or suspicious by asking him what his "true agenda" was. Our attempts to figure out the agenda while juggling competing demands made us rather desperate researchers.

Multimethod Research Design

We established two advisory groups to guide our research on the educational and training needs of public, child-welfare case managers. One group consisted of case managers, supervisors, county directors, the training director, union representatives, and management personnel from within the state child-welfare agency. The second group included stakeholders from outside the state agency, among them representatives from the state legislature, judicial system, clergy, child advocacy organizations, family service agencies, community colleges, and school social workers. A research subcommittee was established from the latter group to provide input for the research plan.

The advisory groups recommended that we focus our efforts on the educational and training needs specific to the case managers in Indiana. To achieve the dual goals of "perfect" research and relevance, we included both qualitative and quantitative methods in the research design. We hoped to enhance the trustworthiness of the findings through triangulation of data and to report findings that had both breadth and depth. We triangulated the diverse views of people involved in our child welfare system and used multiple data gathering tools, such as focus groups, surveys, administrative data, and advisory groups, to provide a more complete, meaningful picture. All research methods were approved by the university's human-subjects committee.

Qualitative Methods

Our research began with six focus group discussions involving a total of 28 case managers and 4 supervisors to identify case managers' learning needs and the most effective methods of delivering content to them. The focus group participants were a purposive sample of both highly respected, competent case managers and new, inexperienced case managers. Participants were hand picked by the state deputy director, juvenile court judges, and local agency directors.

A DACUM model was used to identify case managers' learning needs. DACUM, an acronym for Developing A Curriculum, is an occupational analysis technique involving focus group discussions with line staff (Norton, 1998). The DACUM focus groups yielded rich information, which was used in conjunction with state training documents and literature to develop multiple surveys. In the focus groups, we asked participants to identify the duties, tasks, knowledge, skills, and values of the case-manager position.

Quantitative Methods

Four quantitative surveys were developed to gather data from case managers, supervisors, juvenile court judges, and foster parents on the learning needs of child welfare line staff. The goal of the multiple survey approach was to understand the perspective of each stakeholder and to identify commonalities in regard to the case managers' learning needs.

The case managers' and supervisors' surveys were constructed to include similar questions and format so that the data from each group could be compared and contrasted. All 704 case managers and 136 supervisors from throughout the state were invited to participate in a survey. The survey was posted on a secure website created with the Flashlight Program (Flashlight, 2003). Participants had the option of completing the survey online or printing the survey and mailing it back through postal mail. The Flashlight Program does not collect "cookies" and we did not request identifiable information, ensuring anonymity for participants.

The surveys included questions on knowledge, skills and values, barriers to formal learning, the best methods for delivering training content, the best timing for obtaining the content, case managers' confidence in their abilities, and the most effective methods of supervision. We included both forced-choice and open-ended questions.

The foster parents and the judges were sent surveys by mail. The content of these surveys included the learning objectives from the state curriculum for case managers. Each group was asked to rate the perceived importance and urgency of each training objective. Importance was evaluated in two ways: 1) each group was asked to rate importance on a scale of 1-5; and 2) each group rated whether the lack of knowledge or skill by a case manager would impact a child's safety or movement toward permanency. Urgency was examined by asking when it was necessary for case managers to acquire the knowledge or skill.

Research Challenges

Methodological compromises were necessary throughout the life of the project. There was high turnover affecting the composition of the advisory board, the participation of line staff and foster parents, and the commitment of the administration to the research endeavor. The top administrators of the state agency turned over three times while we conducted the research, with each administration suggesting different avenues of study. We made continual modifications. Not one of the data collection methods, by itself, met the goal of "perfect research." We consistently had a less-than-desirable response rate.

Challenges and Results of the DACUM Focus Groups

The first compromise in the research plan was limiting the DACUM focus groups from the ideal two days of meetings to one day. Strict adherence to the DACUM model would have required participants to gather for two days of focus group discussions on the duties, tasks, knowledge, skills, and values of their occupation (Norton, 1998). Due to the time constraints on the case managers, we were asked to limit the focus groups to one day. To compensate for any potential loss of data, we conducted six separate day-long focus groups, one in each region of the state. The data collected in each focus group were reviewed after each meeting and compared to the list of state-identified training competencies. Conflicts or gaps that existed between the focu-group data and the state-identified competencies were brought to the next focus group for discussion.

The extra cost in organizing, attending, and traveling to six focus groups was not inconsiderable. It is unclear whether two days with the same participants would have yielded different data, but the consistency of the data across focus groups was striking. In the end, each topic in the state list of competencies had been identified and discussed by at least two focus groups. Learning needs related to home visits, personal safety, and computer competence were also introduced and discussed in at least two focus groups. Additionally, participants expressed concerns about the methods of delivering training content and supervision.

Challenges and Results of the Online Surveys

We used the online format for collecting survey data because we thought it would be an efficient way to collect data from busy case managers and supervisors. The benefits would include a faster turn-around time as well as the reduced expense of disseminating the survey. We anticipated that case managers would find the point-and-click method of responding to a web-based survey engaging and easy although we also offered the option of sending the paper copies through the mail.

The invitation to participate was sent to the case managers via e-mail through a listserv constructed for this purpose. We were unaware of the pop-up nature of the case managers' email system. Whenever a message was received by a case manager, a notice popped up on the screen. This pop-up alert system interfered with the ability of case managers to enter data or complete word processing tasks such as writing court reports. The listserv had a few incorrect addresses due to case manager turnover. All bad addresses produced e-mail messages that bounced to all members on the list. Irritated case managers blasted the list with questions and complaints. Each of these messages was also sent to the entire listserv, generating more pop-ups and further angering the case managers. They were not only getting our messages but messages from their colleagues such as, "I am getting all kinds of emails and they won't stop! Help!" It took 45 minutes to shut down the listserv and stop the messages.

Alienating respondents is not an auspicious beginning to a survey. The response rate was 32% for case managers and 46% for supervisors, somewhat less than anticipated. However, case managers and supervisors submitted consistent responses about both the content needed to do the job effectively and the process of training and supervision.

Survey Challenges and Results of the Judges' Survey

The difficulty with the judges' survey began with an attempt to pilot the survey instrument with a subgroup of juvenile court judges. We presented the study and survey to the judges in a face-to-face meeting. After circulating the survey, we were informed that judges "do not pilot surveys." Rather than completing them on the spot, the judges took the surveys home with a promise to mail them back. Although it was helpful to receive a few in the mail as promised, this change in plans inhibited our ability to obtain feedback and revise the instrument.

The surveys were later mailed to all juvenile court judges in the state with a request to fill out the survey "if they had not already done so." A total of 40 completed surveys were returned, for a response rate of 29%.

While disappointed in the response rate, we were pleased that the meeting with the judges became a teachable moment. It was at this meeting that several judges became aware of some of the systemic problems plaguing the public system, including the fact that no college degree was required for the case manager position. One of the judges at the meeting became an active advocate for a change in the current hiring criterian.

A prominent finding of the judges' survey was that respondents felt strongly that case managers should be trained before starting a caseload. The majority of judges believed it is urgent that case managers know the state's legal definition of physical abuse, sexual abuse, neglect, dependency, and endangerment. They also believed it essential that case managers gather evidence and provide effective testimony in court. In their views, a lack of competencies in these areas presented the most risk to safety and permanency for children.

Challenges and Results of the Foster Parents' Survey

The foster parents' survey, sent by U.S. mail to 600 homes, was marred by the typical problems associated with mass mailings. Low response rates and bad addresses required multiple mailings. The time and expense of this method was greater than the results received. Surveys were returned months after they were sent out due to bad addresses. The last return came back six months later with "addressee unknown" stamped on the envelope. In the end, a response rate of 32% was garnered. A response rate this low lessens our ability to generalize to the population of foster parents in the state. However, the foster parents who did respond agreed with juvenile-court judges that pre-service training should be required for all case managers.

Quiet Desperation

The tension induced by the challenges caused us to lead research lives of quiet desperation. The organization and travel for the extra focus groups were tiring, and being responsible for the online pop-up annoyances for already overworked case managers brought us chagrin. The low response rates were frustrating but not overly upsetting, since we knew that survey research often had this challenge. Perhaps what was most trying was the turnover in the state agency. With each new administration, we needed to gain entry, explain the study, and adjust to the change in focus required by the new administrator. This led to weeks of extra work that was generally discarded by the next person. The goal of producing a useful, timely report seemed, at times, out of reach.

Benefits of Mixed Methods

Despite all the challenges and low response rates in almost every method used, the strengths of these multiple methods approach proved invaluable. The results across methods and respondents were surprisingly consistent. Our findings provided the state administration with comprehensive data about the educational content and training needs of child-welfare case managers. Consistent with the use of qualitative methods, there were also unexpected findings. A summary of these findings is presented below.

Unexpected Results

Beyond learning about the educational content case managers need to be effective in their jobs, we also gained information about the process of learning the content. The participants spoke of the organizational problems of training delivery that interfered with the goal of professionalizing the agency staff, such as gaps in preparatory training. Many experienced case managers felt the need for more training in order to feel competent in their jobs and revealed concerns about training methods.

One such gap was that pre-service training was encouraged but not required. Thirty-five percent of the case managers who completed the survey worked for six months before attending any of the core training modules, and more than ten percent did not attend training for over a year after starting a caseload. This stood in conflict with the desire of judges and foster parents that case managers have pre-service training.

Barriers were also in place for case managers who were interested in attending in-service training. Across the state, case managers were required to pay for their own expenses while attending trainings. Expenses could include hotels, transportation, and food. Case managers submitted their bills to the state, but the state was often slow to reimburse them. As a result, case managers accrued interest on their credit cards, a personal expense which was not reimbursed. This posed a particular hardship on new case managers interested in attending pre-service trainings and served as a deterrent to experienced staff who felt a need for additional training.

Not surprisingly, given the barriers to training, the majority of case managers described themselves in the survey as less than competent in important skill areas. One such skill area included working with sexual-abuse perpetrators and non-offending parents. Even among the case managers with six or more years of case management experience, noneteen percent rated themselves as only minimally competent.

The agency's approach to core training was also revealed to be problematic. Sixty-one percent of the supervisors and twenty-six percent of the case managers suggested that six consecutive weeks of core training was overwhelming. Core training offers an intense and exhaustive exposure to content, but lacks strength in skill development. Case managers wanted more support for transferring their learning from the classroom to the field. When asked to rate the best training methods for *new* case managers, both case managers and supervisors agreed that one-on-one coaching and supervision was more helpful than classroom training. Both groups ranked the teaching methods similarly with mentoring, shadowing, case staffings, and regular supervision all rated highly. Over half of the respondents did not mark core training as effective.

Perhaps the biggest surprise among the findings was the unintended consequence of the agency hiring requirement of six years of combined education and/or experience. In this county-administered system, county directors had interpreted the requirements in a variety of ways. The state director was surprised to discover that most county directors believed that the requirement eliminated traditionally aged B.S.W.s from consideration, even if they had field placements at the county agencies.

Recommendations and Actions

After receiving the report, state employees and leadership said the findings rang true. They supported the recommendations. The newly appointed state administrators accepted the report, disseminated the findings to each county director, and took administrative actions in response to the recommendations. Within one year after submitting the report, significant changes were made within the state agency, including mandatory pre-service training with field experience, increased internal supports to dismantle barriers to training, and professionalization of the hiring criterion. There were four primary recommendations on which the state agency took action.

First, we recommended mandatory pre-service training. Participants in all aspects of the study tended to believe that case managers should attend the full continuum of core training before assuming a caseload. The agency responded by developing a written policy to this effect. Case managers statewide are now required to attend six weeks of pre-service training before beginning a caseload.

Second, we recommended that field experiences be integrated into the pre-service training through worker shadowing, close field supervision, exposure to the juvenile court and local service providers, and hands-on experience in documentation and data entry. We felt this would provide a more integrated approach to learning and improve the case managers' retention of material and skill development. We suggested that attention be given to the Michigan model, which integrates classroom and field experiences (Family Independence Agency, 2004). The agency took action to adopt the Michigan model and to add supervisory positions to serve as mentors for new case managers.

Third, we recommended that a system of financial supports for case managers who attend trainings. The agency decided to pilot a new fiscal arrangement by which case managers can bill their hotel expenses directly to the state. This arrangement is currently in place in three of the six regions of the state and has resulted in improved morale and increased participation in training.

Finally, we recommended that the state professionalize the case-manager position by requiring an appropriate educational background and/or a child welfare certificate. The agency's response was to revise the minimum qualifications for the case-manager position to require a bachelor's degree in a variety of human service fields, plus either two years' experience or the agency's six-week training program.

Conclusions

Our research was not done in isolation. We worked closely with the state agency throughout the life of the project. We listened to concerns and modified our research design in an attempt to keep our study relevant to their needs.

Within one year of submitting our report, all but one of our recommendations had received action by the state administration. It is important to note that not all of the administrative changes were initiated by our report in that several of the changes were evolving as we conducted the research. In some cases, the findings from the research served as outside evidence to support changes that already were in process or being proposed within the agency.

At the conclusion of the study, the director who asked for "perfect research" was no longer with the agency. The methods used in our study were generally imperfect, but the results were recognized by state personnel as an accurate reflection of the educational and training needs of the case managers and the barriers to effectively meeting these needs. The value of mixing methodologies is apparent: from the multiple viewpoints came meaningful findings and an in-depth understanding of the problem. As Padgett states, "... rigor without relevance produces research that is mechanistic and devoid of meaning ..." (1998, p. 103). Hearing the multiple voices in a variety of forums provided relevant results and policy changes.

In the end, we established research findings that rang true to the state employees and the administration. We were excited that despite its problems, the study was used to push for change both administratively and legislatively and by the executive action of the governor. With all its flaws, the research was almost "perfect," if perfect can be defined as "research with findings that *few* could dispute."

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(Footnotes)

[1]Title VII of the Civil Rights Act of 1964 was passed to ensure that employers would not discriminate in hiring on the basis of race, religion, sex, or national origin (*U.S. Equal Employment Opportunity Commission, 2005)*. Age, disability and other aspects were added later. Exceptions are allowed in the case that the hiring practice is a "bona fide occupational qualification," a necessity for the position.

When the Results Disappoint, the Process Matters Little[1]

William Meezan, D.S.W., Ohio State University and Bowen McBeath, Ph.D., Portland State University

This narrative describes a research project done in the community that did things "right" by academic standards and at times felt "blessed by the research gods." It brought together members of the academy and representatives of public and nonprofit agencies, was fully participatory in nature, had adequate funding that was obtained with relative ease, used the strongest of designs and analytic techniques, and was well disseminated. Yet, in the end, politics and behaviors that fostered organizational maintenance trumped findings that led to new knowledge and organizational learning. Despite the academic rewards that the author has reaped and will continue to reap from this experience, it left him feeling more cynical about the system of services that has been put into place for children in foster care and their families.

I arrived at the University of Michigan in fall of 1999 to assume my new position as the Marion Elizabeth Blue Professor of Children and Families. Having left Los Angeles with most of my commitments completed, I was anxious to find my next big project. I knew it would not be easy to establish myself in a new school, with a new set of colleagues. Nor would it be easy for me to break into and learn a new child welfare system. But having to do so with what felt like so many people "watching" – after all, I was in the first endowed chair at what was rated as the top school of social work in the country – was more stressful than I had ever imagined. The self-imposed pressure was intense, to say the least.

This pressure pushed me to be bolder and more entrepreneurial than I normally would have been. I interviewed colleagues at the school, asked people to make introductions for me, and called state officials "cold" to try to get a beginning understanding of the child welfare environment in Michigan and what was "on people's minds." I was pleasantly surprised at how open people were to talking with me. Perhaps it was my position; perhaps it was the fact that I had connected with a well-known child advocate in the state who was willing to open doors for me. Whatever the reason, getting in to see people didn't prove to be as difficult as I expected.

Interestingly, at the state level and to some extent locally, I discovered that one experimental program under way in Wayne County (Detroit) seemed to be getting all of the buzz. Called the Wayne County Foster Care Permanency Pilot Initiative (the pilot), it was a new way of reimbursing some of the private foster care agencies for providing services to children and their families under their care. The pilot had two primary objectives: to reduce the number of days that children spend in foster care and to increase the percentage of foster children that reach a permanent placement.[2] A secondary objective of the pilot was to allow nonprofit agencies to provide services to foster children and families in a flexible manner, thereby (hopefully) better meeting their needs. Who could argue with either of these objectives? I certainly agreed with them.

In order to provide incentives for agencies to meet these objectives, the state child welfare agency – the Michigan Family Independence Agency (FIA) – altered the provisions for agency reimbursement in its foster care contract. Rather than being reimbursed for administrative costs on a standard per-child, per-diem basis, the agencies that volunteered to participate in the pilot provided services on a reduced per-diem basis. These pilot agencies' revenues, however, were supplemented by a substantial initial payment as well as additional bonus payments linked to the achievement of performance milestones. Pilot agencies were allowed to use their initial, bonus, and per-diem payments as they saw fit.

This approach intrigued me; in an era when managed care was infiltrating the child welfare sector and wreaking havoc with a number of state systems, this seemed to be a sensible approach. I wondered if it would work. I wondered how agencies adapted to the new model. And I wondered why there was a significant buy-in into the program on the part of the state and some private agencies.

The pilot was planned in 1995-96, when the Republican party controlled state government in Michigan. My poking around led me to a couple of tentative conclusions about why Michigan had decided to test this particular approach. On first blush it seemed that the decision to experiment with the pilot came about due to a confluence of factors. The first of these had to do with Michigan's approach to child welfare: Michigan law had predated the federal Adoption and Safe Family Act in terms of having stricter time limits to move children through the foster care system, and the pilot was seen as way to facilitate this movement.

The second factor emanated from some of the private agencies themselves – a handful of these agencies were eager to have flexible funds, particularly when a child first entered care, to meet the immediate needs of families and thus facilitate shorter stays in care. These agencies also saw this reimbursement system as a way to refocus their mission and be rewarded for taking timely actions on the part of children. Some, I vaguely suspected, may also have seen this reimbursement system as a way to make their foster care service more profitable.

The third factor was a reflection of Republican thinking at the federal level about what child welfare delivery systems should look like. "Performance-based" and "accountability-driven" service delivery were becoming important buzz words in Washington. In addition, the pilot was seen as a step toward controlling the cost of foster care, as it included two fiscal features characteristic of managed care models popular with fiscal conservatives – cost containment and risk shifting (Embry, Buddenhagen, & Bolles, 2000; Wernet, 1999; Wulczyn, 2000).[3]

The pilot was launched in 1997 with 4 of the 19 private, nonprofit foster care agencies in Wayne County choosing to adopt this new way of reimbursement for services (two of these agencies were instrumental in designing it). In March 2000, when two additional agencies adopted this incentive-based reimbursement system and some modifications had been made to it, I met with the officials from FIA responsible for it. Present at this meeting was Jim Beougher, Director of Children's Services for the state, who was a major force behind the initiative.

It was clear to me that those around the table, and particularly Jim, wanted the pilot evaluated. They seemed sure that it "worked"— that it moved children through the foster care system more efficiently without harming them. They guaranteed cooperation, but no money at that time, and asked that I pursue the possibility with the pilot agencies if I was interested in undertaking an evaluation of the initiative. I was excited about the possibility. Here was an important project that could enhance knowledge about an innovation in child welfare. Here was the possibility of forging important relationships with the state and with agencies that had never existed at the School of Social Work. Here was a way to again test my research skills with a "big" project. Here was a possibility of my "hitting the ground running" and proving myself to my new colleagues.

It seemed like a natural for me. Child welfare had been the intellectual home for my entire career, and I had grown to be a community-based, participatory evaluator, who, while building on the work of others (Patton, 1997), had even developed and applied a framework for organizational learning through evaluation (Cherin & Meezan, 1998). My previous work had taught me that under the right conditions, university researchers and agency personnel could work closely to design and implement high-quality evaluations that had real, practical meaning for organizational and policy change (McCroskey & Meezan, 1997; Meezan & O'Keefe, 1998a, b).

The next step was clear – I needed access to the pilot agencies to ascertain their interest in such an endeavor. I had no idea if they would be interested. I doubted whether such a project could be pulled off. Would they be protective of their practices as many agencies often are? Would they open themselves up to scrutiny, particularly when they knew that FIA supported the pilot initiative? Nonetheless, in my new emboldened state, I decided to put on my "confident" face and move forward.

I decided that the easiest way to begin to try to gain entry to the private agencies was through their association – the Michigan Federation for Children and Families. Its executive director, Bill Long, was extraordinarily welcoming and offered to make introductions. He also let it be known that he would be willing to support an evaluation in any way he could. While he could not offer money, he offered to make introductions to the executives of his member agencies, to help get me into the right meetings, to throw the weight of the Federation behind the project, and, should we get this project off the ground, to designate a staff member to be involved in its design and execution.

In this first meeting with Bill, something else occurred that would shape my time in Michigan. He mentioned that he was in discussion around research ideas with a very bright doctoral student in social work and political science at the University of Michigan who was interested in nonprofit management. His name was Bowen McBeath – "had I met him?" Admitting that I had not, I told Bill that I would contact Bowen to see if there was a "fit" between our interests and in our styles.

Bowen and I met shortly after this meeting, and we hit it off immediately. Here was an intellectually curious young man who had previously worked for researchers I respected. He was looking for a long-term opportunity to learn to do meaningful research in the "real world." He was personable and seemingly hard working. I decided immediately (I often form judgments of people quickly) that I couldn't have asked for a better "second"; he seemed (and proved to be) trustworthy, organized, and committed to his own learning. Perhaps equally importantly, his skills complemented mine – he knew the literature on nonprofits and organizational theory while I knew foster care and child welfare research.

There could not have been a better match; finding Bowen just felt "right." I doubt if he knew how scared I was of this large and tenuous undertaking; I know he was unaware of the doubts I had that the project could be "pulled off." And I'm sure he didn't know how much his presence gave me strength, confidence, and fortitude to go forward. Connecting with Bowen in this serendipitous way calmed me and told me that things might just work out – that the research gods might be shining on me and this work.

When we decided to begin working together, I couldn't promise Bowen anything more than the opportunity to go "exploring" with me to see if we could put this project together — politically, economically, and logistically. But there was a commitment attached. If he took the risk and joined me on this exploration, and if, against the odds, we pulled the project off, I would guarantee him everything a doctoral student could want beyond the classroom: experience in negotiating a community-based research project; exposure to grant writing; the opportunity to directly supervise a complex research project on a day-to-day basis; more-than-fair compensation; data for his dissertation; opportunities to present at conferences; and joint authorship on all publications. I was thrilled when he said "yes," and for the next few months we laid the foundation for what has become, at this point, a five-year journey. He wasn't compensated for his time initially, but he was learning from me and from the people we met. It was evident in our long discussions to and from meetings, and from our grandiose planning, that we were excited and were going to have fun!

Bowen and I began to meet with the State FIA officials charged with the administration of the pilot and with key administrators and foster care program managers from the pilot agencies – they had a regularly scheduled meeting to discuss implementation issues. In May 2000, by invitation, we attended our second meeting with this group, and the possibility of doing an evaluation was the major agenda item of the day. At this meeting I explained the evaluation approach I was proposing. I assured them that they would have the major "say" in the evaluation – that after I taught them some basic research and evaluation principals they would basically design and execute the project under my direction. I used every cliché in the book that I believed: that the evaluation would be "bottom up" rather than "top down"; that the evaluation would be done "with" them rather than "on them"; that the evaluation was not about "I gotcha," nor was it a "compliance audit." Clearly they were skeptical, since some had been burned in the past by researchers and outside evaluators. But they did listen, and even made comments like "Are you sure you're a university researcher? So many don't work this way," and "You seem to still care about the kids."

We agreed at that meeting to go exploring together but also thought that the group at the table (probably 40 in number) was too large to plan this effort. We thought it best that those in charge of supervising the foster care program and some other key players in the agencies should form an evaluation committee. And so, through a series of meetings with this evaluation committee between May and December 2000, we came to a common understanding of the role of evaluation in improving program performance, the pilot-related research questions that we might examine, the data that might be necessary for the evaluation, and how these data might be collected.

In order to form a control group, Bill Long helped us recruit three Wayne County foster care agencies that were operating under the normal fee-for-service contracts (the non-pilot agencies). He suggested potential agencies based on their similarity to the pilot agencies in terms of size and other criteria, including his belief that they would participate. With Bill having greased the wheel, I met with appropriate officials at these agencies and they agreed to lend their support to the research; some really didn't believe that the pilot was good for children or their families and wanted to "prove it" (which is always a great motivator for participation). These executives appointed their representatives to the evaluation committee who were, for the most part, the foster care service directors/managers from their agencies.

From the beginning, the evaluation committee consisted of me, Bowen, representatives from Michigan FIA as well as the Michigan Federation for Children and

Families, and one to two representatives from each of the six pilot and three non-pilot agencies. In addition, there was a time when Wayne County (as opposed to the State) FIA was represented at the table, but they eventually dropped out of the process. Their on-again, off-again involvement with this project could be the subject of its own narrative, but suffice it to say that I was furious, as I believe others were, when they walked away from the table just before data collection began. They had influenced the process and the evaluation questions and methodology, yet when push came to shove they didn't want to be part of the study even through they provided 15% of the foster care services in the county. There was a series of written and in-person exchanges between me and the Director of the Wayne County FIA, but in the end she would not budge. So we went ahead with a simple comparison between Wayne County nonprofit agencies that were and were not participating in the pilot. And I was left to conclude, once again (I had experienced such reluctance in the past), that too many public sector child welfare agencies don't want to open themselves up to the same kind of scrutiny that they expect of those from whom they purchase services.

Other than the Wayne County FIA folks disappearing from the table, there was little turnover among committee members; all but a handful of the agency representatives were involved from start to finish. During its first three years, the committee met bimonthly first to learn about the evaluation process and then to design and carry it out. Between 2003 and 2004, the group met monthly to review new analyses, comment on their meanings, and discuss methods of disseminating study results to state and local policymakers and agencies serving children and families. Everything worked as it was supposed to, the research gods were smiling on us, and we were delighted.

The Participatory Nature of the Evaluation

From its inception, the evaluation committee operated in a participatory manner (Cherin & Meezan, 1998; Patton, 1997). We followed a modification of an evaluation cycle that I had helped to develop earlier in my career (see Figure 1) and accordingly archived each of our meetings.

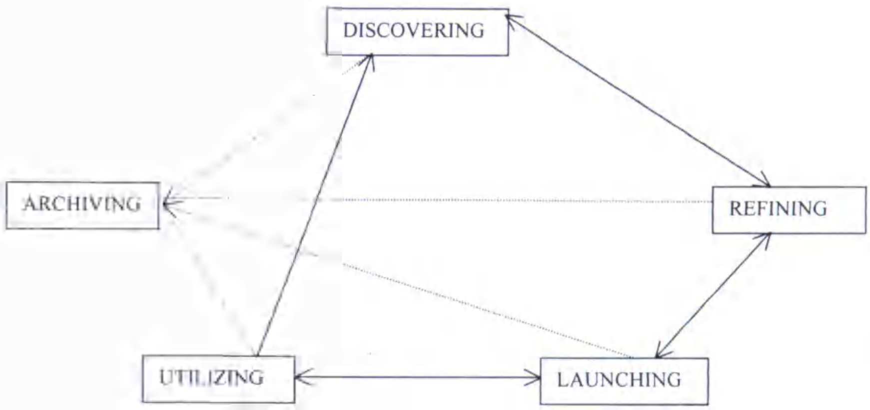

Figure 1 - The Action Evaluation Model

Adapted From Cherin, D., & Meezan, W. (1998). Evaluation as a means of organizational learning. *Administration in Social Work*, 22(2), 1-21.

All major aspects of the evaluation – central research questions, research design, sampling, and data collection techniques and instruments – were discussed with, decided by, and approved by the cooperating agencies. The following notes from an early meeting demonstrate the tone set for all dialogue around these issues:

> *Bill handed out [a] task list and asked all present to read it and see if there were any omissions or incorrect order of task items, as this list would ...become the evaluation committee's working agenda. Verlie asked if there were any more*

> *specificity to the notion of "choosing a sample." Bill responded that there were two questions we need to answer here: a) Who is eligible for the sample...and b) when do we start collecting the sample? ... Verlie also asked about any preliminary ideas concerning the timeline for each task. Bill replied that each task will take as long as we need to take...Rita asked if Bill and Bowen had anything in mind regarding the need to create data instruments. Bill responded that we will bring drafts of questions and ideas for the group to consider, but we'll rely on the group to be expert judges in order to ensure that the questions have content validity. In other words, the overall group will make sure that the questions [are] able to capture the reality...(11/28/00)*[4]

Why did the agencies participate in what would turn out to be an overwhelmingly complex set of studies? I believed at the time that they participated, despite the considerable costs in attention, effort, and time, because they were genuinely interested in learning whether the pilot initiative was effective in reaching its stated objectives. I truly thought (and I believe that they did too) that they had hoped to use the study results to improve the performance of their foster care departments and that they wanted to learn how to carry out rigorous evaluations so that they could be carried out in other parts of their agencies.

As a result of this voluntary and participatory framework, Bowen and I and our research staff enjoyed a close working relationship with the representatives from the participating agencies. This relationship allowed for multiple rounds of data collection, data cleaning, and data analysis, which are often difficult to successfully complete in large-scale, field-based, research projects. This close relationship also improved the accuracy of the databases, the relevance of the analyses undertaken, and the validity of the interpretation of the findings and their congruence with agency experiences.

Funding the Evaluation

It was at the point when we had to find funding for the project that I first used the word "blessed" out loud. I had found Bowen, the partners were on board, the planning was going well, but without money nothing further could happen. Then things really started to fall into place. FIA was shocked that we had gotten as far as we had and were actually "pulling it off." Jim Beougher said that he would give us a small, non-competitive grant for the first year, and a second grant the next year if we continued to succeed. With the first $25,000 in hand, we went looking elsewhere. Each time we looked for money, we somehow found it. Nobody would fund the whole project in any one year – no single funder really believed that we could pull off a project with a complex design, multiple data points, nine agencies and the State of Michigan, in Wayne County, etc. But the more we succeeded the easier it was to piece together money from different sources.

Even our proposals to funding sources were reviewed word by word during the evaluation meetings; yes, it really was participatory:

> *Bill will incorporate the group's revisions to the output evaluation proposal prior to sending it on to Lynn at FIA; Bill will send everyone a draft of the... proposal for an outcome evaluation planning grant (11/01/00).*

And so one challenge grant funded another, and in the end we funded ourselves for four years through more sources than I could have ever imagined: FIA, the Aspen Institute's Michigan Nonprofit Research Program, various offices at the University of Michigan, the Kellogg Foundation, and the Skillman Foundation all contributed at some point. Some grants were bigger than others, and some years were leaner than others, but there was always enough money for the basics. In flush years we could pay the agencies for some of the time it took them to collect data, but when we couldn't pay they collected the data anyway – and they understood.

The Central Research Questions of the Evaluation

We decided that the evaluation of the pilot initiative should include a process evaluation and an impact evaluation. The process evaluation contained a qualitative and a quantitative component; the impact evaluation was strictly quantitative.

The *qualitative* component of the process evaluation examined nonprofit service providers' organizational adaptations to the pilot initiative. Two research questions guided this phase of the study: 1.) What organizational changes did pilot agencies make when they shifted to the pilot contracting environment? and 2.) What organizational accommodations did non-pilot agencies *expect* to make as they anticipated shifting to the pilot contracting environment? The study examined these questions with data obtained through telephone interviews with administrators, foster care supervisors, and foster care line staff across the six pilot and the three non-pilot agencies in the evaluation.

The *quantitative* component of the process evaluation examined the effect of the pilot initiative on service provision to foster children and their families and became Bowen's dissertation (McBeath, 2006). This phase of the study was organized around three research questions: 1.) What types of services did foster children and families receive from pilot and non-pilot agencies? 2.) Were there any pilot/non-pilot differences in the amounts of services provided over time? and 3) What factors were associated with differential service patterns between the pilot and non-pilot agencies?

The impact evaluation sought to identify how and at what rate pilot and non-pilot children moved through the foster care system. This study focused on six research questions: 1.) How many foster children reached each performance milestone? 2.) Were there any pilot/non-pilot differences in the proportion of foster children reaching each performance milestone? 3.) What factors were associated with the achievement of each performance milestone? 4.) Were there pilot/non-pilot differences in the final dispositions of foster children and families? 5.) Were there pilot/non-pilot differences in the length of time needed to reach these final dispositions? and 6.) What factors were associated with the achievement of these final dispositions?

The Qualitative Process Study

We designed this stage of the study to assess changes across various levels of agency employees and with employees who performed different functions within the agency. We gathered information on the effects of the pilot foster care initiative on service delivery, interdepartmental and interorganizational relations, staffing patterns and staff training, staff roles and responsibilities, and financial management and technology use. We interviewed 45 administrators, 19 foster care supervisors, and 20 foster care line staff from pilot and non-pilot agencies based on guides developed by Bowen and me and modified over the course of a number of meetings by the evaluation committee. Members of the evaluation committee purposively suggested respondents for their knowledge of foster care and the pilot initiative. Those who were interviewed from pilot agencies told us about their actual experiences in the areas that were probed; those from non-pilot agencies speculated on what would happen in these areas if their agency had to move to incentive-based contracting (for a full discussion of this phase's methodology, see Meezan & McBeath, 2003).

> *In the next few days, agencies should look through the draft questionnaire with an eye to (1) any important topics or questions that are not present and (2) any topics or questions that seem superfluous. Agency administrators, supervisors and line staff should be given a chance to comment on the questionnaires....agencies should provide Bill with a list of staff (at the administrative, supervisory, and line staff levels) that should be interviewed. These should be experienced, thoughtful people (6/21/01).*

The results of the study delighted us, for they systematically conveyed the experiences of the pilot agencies and showed how naïve the non-pilot agencies were about the depth of change that would be necessary should they have to move toward this form of reimbursement (for a full discussion of these results, see McBeath & Meezan, in press; Meezan & McBeath, 2003, 2004). And the findings delighted our agency partners; those at the table thought that we had really captured their realities.

Our findings described the service delivery changes made in intake and assessment procedures, performance tracking, and managing flexible funds made by the agencies. They suggested that performance-based contracting required the service providers we studied to quickly diagnose clients' needs, focus additional resources on collecting client and service information in databases, and pool funds to address the multiple needs of clients systematically and simultaneously rather than haphazardly and sequentially. And we captured the fact that many agencies felt the need to create new type of positions within their foster care departments.

Our results also showed the depth of change needed in the agency's financial management and accounting procedures, and the difficulties in budgeting and forecasting for the pilot agencies due to insufficient information, hidden costs, and other unexpected barriers. We found that moving to the pilot model required foster care departments to have more communication with other departments in the agency.

Our findings also uncovered some discomfort with the pilot, and I only wish we had paid more attention to this finding at the time. Agency supervisors in particular noted that some conflict existed between the pilot program's goals and their conceptions of the appropriate goals and processes of foster care. For some of them, performance-based contracting was associated with the possibility of reduced lengths of stay for children in foster care and less service to children and their families. Some workers and supervisors feared that agencies might return children to biological families at the ultimate expense of child safety and well-being; others feared that parental rights might be terminated before parents had been given an adequate chance at rehabilitation through the provision of appropriate, long-term services.

This process evaluation allowed us to make recommendations that were developed with the help of the evaluation committee. We agreed that contract incentives, as seen in performance deadlines, directly influenced work environments and employee behavior, and that if performance deadlines were set they needed to be done so that employees had sufficient time to conduct thorough assessments, provide necessary services, access community resources, and place children in living situations that promote safety, permanency, and positive well-being. We agreed that the data suggested that service delivery should be monitored to ensure that client needs rather than cost considerations dictated the services provided to a family, and that if agencies were to be successful under this form of reimbursement, formal relationships with collateral service providers needed to be established and high-demand, out-of-agency services needed to be available. It was also apparent to the evaluation committee that management had to listen to front-line staff to ensure that performance-based contracting did not jeopardize the quality of service provided to the client, and that both vertical and horizontal communication within the agency was crucial for successful implementation.

We were off to a great start! Our findings and recommendations rang true with the evaluation committee, the agency executives to whom they reported, and the professional audiences to whom they were presented. We were capturing what the agencies were living, and we were providing useful information to those who might move to this form of reimbursement system. Who could ask for more?

The Quantitative Process and Outcome Study

We needed to identify a sample of foster children being served by the six pilot agencies and three non-pilot agencies in order to examine service provision to foster children and families and the outcomes of these services. Since the early 1990s, children entering foster care in Wayne County were assigned to nonprofit agencies through the Family Assignment System (FAS). The FAS requires children entering foster care for the first time to be assigned to the next agency in the alphabetical queue of nonprofit service providers. If an agency was at full capacity and did not have a foster care slot available, the next agency in the queue was contacted, and if a space was available the child was placed with that agency.

The 19 nonprofit foster care providers in Wayne County received all of their foster care cases through the FAS. As a result, children were assigned to agencies in a manner unrelated to their characteristics or family circumstances. Thus, assignment to agencies was random and placement with an agency was decided based only on when the child entered care. Who could believe it? Without planning, conditions in Wayne County were such that we had the equivalent of a randomized trial! We were going to be able to isolate the independent effects of the pilot initiative from other individual-level covariates of service provision. Who could ask for more? As researchers we were jubilant. To the agencies, the rigor of the study was a source of pride – nobody could doubt that their work was being evaluated in the most exacting, fair, and impartial manner. The following, from notes after the first data were in, shows the agencies' general pleasure with what was being accomplished because of the use of this design:

> *Bill first led the group through the comparison between pilot and non-pilot children. There are minimal differences between the groups. Therefore, Bill mentioned, any differences in outputs are not due to differences in the children...Rita said, "There goes our first excuse" to general laughter. (1/10/02)*

Clearly the research gods continued to bless us.

We began sample identification in May 2001. The evaluation committee agreed to gather a sample containing roughly 250 foster children – 180 foster children from the six pilot agencies and 70 children from the three non-pilot agencies. This sample size reflected the agencies' estimates of the number of new foster care case admissions over a two-month period. This sample size also provided sufficient cases to ensure statistical power for some multivariate analyses.

Agencies were responsible for identifying all study-eligible children and for completing a sample screening form for each foster child entering the agency during sample selection. As a group, we agreed on the criteria on which children would be admitted to the study. And as became the routine for the development of all data collection instruments used in the study, Bowen drafted the screening form used by agencies to identify study-eligible children, I modified/approved it, and the members of the evaluation committee modified it to better meet their needs and fit into the data collection that was already occurring at their agencies.

> *Homework for each agency:*
> *• Add/subtract to question 11 of the service provider data collection instrument...*
> *• Review the potential aggregate service data collection instrument... (3/21/01)*

> *Homework:*
> *• Bill will be e-mailing final versions of all forms to the agencies. Each agency should run one case through the forms to see if they work and let Bill know if they have problems, within a week (4/5/01)*

> *Bill said that he was very pleased that he'd received requests from a number of people asking for revisions to certain [data collection] forms. He went over a few of the requests... (4/20/01)*

In all, agencies used six data collection instruments to gather quantitative data for the evaluation. These six data collection forms concerned the characteristics of the foster child at the point of his or her entry into foster care; the characteristics of the primary caregivers of the foster child at the point of his or her entry into foster care; the services provided to the foster child and his or her family by the foster care agency; the services provided to the foster child and his or her family by other agencies; the characteristics of the caseworkers serving the foster child and his or her family; and the placement status of the foster child. The evaluation committee designed five of these six instruments; only the form pertaining to out-of-agency services was not constructed specifically for the evaluation, since FIA requires foster care providers in Wayne County to list the services provided by other agencies to foster children and their families on a form called the "FIA 67."

Two of these instruments were completed only once per child. These non-recurring instruments included the child characteristics form and the primary caregiver characteristics form. In contrast, the in-agency services, worker characteristics, and child output data collection instruments were completed 30 days after the child's entry into foster care and then every 90 days thereafter. This was done at the request of the evaluation committee, since foster care caseworkers in Michigan are required by State law to complete regular reports concerning the status of foster care cases at these time intervals. These recurring data collection instruments were collected regularly through 930 possible days in care, or just over 2 and one-half years.

We began sample selection for the six pilot and the three non-pilot agencies on May 1, 2001. Sample selection stopped for pilot agencies on September 1, 2001, and one month later for the non-pilot agencies due to the need to admit adequate numbers of non-pilot children and ensure statistical power for multivariate analyses. The final sample was composed of 243 foster children, which included 175 children from the six pilot agencies and 68 children from the three non-pilot agencies.

Politics Creep In

In the State of Michigan 2002 was an election year, and there was a chance that the Democrats would regain the state house. With this concern, the strong commitment to the fiscal principles that underlay the pilot initiative, and the belief that the pilot was "working," Jim Beougher and his staff at FIA publicly announced that the pilot would go county-wide (all 19 private foster care agencies in Wayne County) in October of 2002 before any possible change of administration could occur. This happened despite the fact that the State was partially funding the evaluation.

I feared that politics was going to trump knowledge and rational decision making, for we were not going to have findings by October. Since I knew from previous encounters that as a researcher I would not be able to influence Jim directly, I worked behind the scenes with the FIA staff on the evaluation committee and their superiors to try to influence his thinking and get him to reverse his decision. I fed them arguments and reasoning to use with him, such as "If the data were not going to be in, and if the pilot needed to expand for political reasons, why not do another phased entry as had happened in 2000?" "Wouldn't it be embarrassing to go county-wide only to discover that the experiment had failed?" My voice resonated with the FIA folks who had been involved with the evaluation, but their voices did not resonate with Jim. The plans to go county-wide were to go forward, and that was that.

Our disappointment was lessened somewhat by the fact that the preliminary data from the qualitative process study would help ensure that new agencies moved to this reimbursement system in an informed and thoughtful way.

Lynn [the FIA rep to the evaluation committee] said that she is giving non-pilot agencies a walk through the pilot guidelines and that Cheryl and Carmine [members of the evaluation committee] have helped a lot. Lynn said that she read some of the qualitative themes at the meetings, so non-pilot agencies are benefiting from our work here. ...Lynn's goal is to use more of our group's data in order to have new trainings later in the fall. Our interview data is helpful...because it helps her keep in mind what's important to talk with agency representatives about. (8/14/02)

The First Quantitative Findings

As findings of the study became available, we began to present them to the members of the evaluation committee. Initially, we were most interested in the findings after 300 days of the experiment, since the first incentive payment milestone occurred at 290 days, and we wanted to see whether the incentive-based payment system was having any effect on service provision to and placement outcomes for children and families.

The findings were, for the most part, disappointing to those at the pilot agencies. What we found was that while controlling for many other variables, children and families served by pilot agencies received significantly *fewer* services from the agencies themselves, and that fewer referrals were completed for out-of-agency services on their behalf than non-pilot children and families. These results fit perfectly with the concerns of the workers and supervisors we had interviewed in the qualitative study.

For months, people struggled to make sense of these findings, and the struggles intensified over time. Agency representatives often interpreted the overall findings as suggesting that their agency was primarily responsible for the changes that were occurring. Even representatives from non-pilot agencies tried to find reasons for the differences to bolster the work that their friends/colleagues in the pilot agencies were doing:

> *Rita then said she was concerned about documenting for services – her workers may not be documenting all that they do…Rita said she'd be disturbed if an interpretation was made that pilot kids were going home faster with fewer services (10/3/02).*

> *Rita suggested that one of the possible reasons why pilot services are lower than non-pilot services is because workers may not be recording the services provided. Bob said that he thought the differences in services could be related to turnover. Kathy replied that this could be true across agencies. Kathy also mentioned that it's possible that pilot agencies have simply found a more efficient means of doing business ... Rita noted that her relative assessments are done outside of the foster care unit and are therefore not recorded. Rita, Cheryl and Kathy said their workers' case aide contacts probably didn't get recorded either. Rita also mentioned that these results made her want to revise how her workers fill out forms. Rita said she was fighting the urge to go back to her cases and change them. She said, "Up 'til this point I've taken a laissez faire approach, and it's killing me now, and I need to hear you tell me not to do it." Bill replied "Don't do it. Trust the process."(11/21/02)*

And we debated some potential rival hypotheses for these findings. Were pilot agencies providing only those services that are seen as absolutely necessary to achieve performance payments and/or improve client outcomes? Did pilot agencies choose to save the initial lump-sum payments and performance bonuses rather than use them for additional services? Were unmeasured differences in organizational goals, structure, processes, and technology impacting service provision? Did this form of payment simply lead to service rationing?

> *[After a discussion of possible creaming in terms of service provision, which the data suggested might be occurring at that point] Bill: "If I made a statement to you that a worker with too many cases spends time first with easier cases, would that surprise you?" Rita: "But that hides the honest effort of workers to spend time with each case. So subconsciously there is this…" Bill: "Remember, we're not judging you. This is for improvement. If these things hold, it'll suggest who you're serving, who you're missing, and your programs. …what lessons can we learn from these data? Kathy: "That's what Jim did at LAS. Cases most likely to succeed/fail received different services. And that was because he was bothered by what he knew about who we were not serving." (12/2/02)*

We obviously could not resolve these questions, but those around the table agreed that whatever was going on was, at least at the operational level of the foster care program, unintentional. In the end, everyone around the table accepted the fact that performance-based contracting was associated with a decrease in service provision. And much to people's dismay, and despite the strong emphasis in professional child welfare training on crafting individualized treatment plans, multivariate analysis led us to conclude that few case conditions influenced the amount of services that were provided to children and family in the foster care system.

The outcome data proved just as disappointing. After 300 days in care, there were, in general, no statistically significant differences in the placement status of children from pilot and non-pilot agencies on the major outcome variables: 36% of the full sample was returned to a relative and 21% was returned home to a parent. And while 55% of the children served by pilot agencies and 45% of children served by non-pilot agencies reached this first pilot payment milestone, this difference was not statistically significant. Furthermore, this milestone was achieved in about the same amount of time under these two conditions. Yes, there were some minor differences that did favor the pilot agencies: they were significantly more likely than non-pilot agencies to have their agency supervision terminated (15% as opposed to 3%); and children from pilot agencies were also more likely to have their court supervision terminated than were children from non-pilot agencies (19% versus 4%), but these did not overshadow the main finding that the two payment systems seemed to be achieving similar outcomes in about the same amount of time.

We did learn some useful information about who did and who did not achieve permanence within this 290-day framework, and that knowledge provided a feeling of accomplishment for those around the table. It was important to know that children that had experienced prenatal drug and/or alcohol exposure were less likely to reach the first performance point; that children that had been neglected or abandoned were also less likely to achieve the first pilot milestone; and that children whose primary caregiver scored highly on the worker-completed assessment of needs and strengths were more likely to reach the first performance point.

But despite these insights, there was general disappointment with the reality we had uncovered. And needless to say, the discussions around the major findings about outcomes were just as intense as those that took place about the service findings.

The Conference

Despite the disappointing findings, the process through which the evaluation took place led to a real sense of ownership of the findings on the part of the participants and their agencies. While Bowen and I had been presenting our preliminary findings of both the qualitative and quantitative studies at national conferences, it was clearly time to unveil them to a more "local," but perhaps a more important, audience. We had money in the budget for a statewide dissemination conference, and we went forward with it.

Held at the University of Michigan in October of 2003, the day-long First Research Conference on the Wayne County Foster Care Pilot Initiative was a smashing success. Fully planned with the evaluation committee over a number of months, over 100 people attended. They represented private agencies from around the state (including the newly inducted pilot agencies in Wayne County), the new top leadership of FIA (yes, the Democrats had won the election and now controlled the state bureaucracy), our funders, academic colleagues, and child welfare advocates. Everyone involved in the evaluation committee had a hand in the conference: Jim Beougher and representatives of the two private agencies that helped design the pilot discussed the background and history behind the initiative; Bowen presented an overview of managed-care initiatives in child welfare in the U.S. and of the pilot; and I presented the design and findings of the qualitative process study. After lunch, Bowen presented the quantitative findings through 300 days. Four agency representatives presented the lessons they had learned both from participating in the evaluation and from the findings, then four other committee members talked about the future – issues that new agencies had to be aware of as they implemented the pilot, and issues to be explored as new data were analyzed (we now had data well beyond the 300 days reported at the conference, and were looking at the termination of parental rights milestone at 515 days and the incentive payment attached to it).

After each of these presentations the questions flowed and the discussion was lively. People were clearly excited; most stayed for the whole day. And different people were excited about different things: about the work itself; about the partnership that had developed between the State, the University of Michigan, and the agencies; about the commitment of the agencies to learn from what they were doing; about the rigor of the study; and most of all about the practical importance of the findings and their implications for agencies starting, or who might have to start, down the performance-based path.

Four things about that day stand out for me. First, the agency representatives were magnificent. They not only knew their stuff and presented eloquently, but they weren't thrown by difficult questions. They "owned" the evaluation process and were sure it was rigorous. They were sure that the data, no matter how disappointing, were reliable and valid. They were honest about what they had and had not learned, both about evaluation and about performance-based contracting.

Second, Jim Beougher was defensive and somewhat upset at the results. No longer in his role as the head of Children's Services for the State, but now a County Director (he had been reassigned after the election), he first tried to attack the design and then tried to find any uncontrolled variable that might explain why pilot agencies had not achieved better outcomes than the non-pilot agencies. He was joined in this exercise by a higher up at one of the agencies that had designed the pilot but had not been at the evaluation table. They finally seemed to take solace in the fact that these were preliminary findings, and that once *all* the data were in differences would emerge that would show the true effectiveness of the pilot initiative.

Third, the new administrators from State FIA seemed incredulous at what we were hearing. "Are you saying that less service is being provided in pilot agencies?" "Yes," someone would answer, "but there might be lots of reasons for that, some of which are not necessarily bad." "You mean pilot and non-pilot children are as likely to end up in a permanent home at 300 days?" "Yes, that is correct, but there are lots more data to come, and we haven't even looked at recidivism yet," someone else would answer.

Finally, I remember someone in the FIA central fiscal office, Vic Bursankas, asking about the cost of the two comparison conditions. The question was asked publicly and we explained that we did not have such data. Later, at the end of the conference he cornered me and asked the questions again. I gave him the same answer. I didn't think much about this interchange at the time.

More Data

Over the next year, data collection was completed, all the data cleaning was done, and all primary analyses were completed. We kept meeting with the agencies to present the findings. And while we had more nuanced information, the basic findings regarding service provision really didn't change between the conference and the 930-day data collection mark. Controlling for all other factors in the study, children and families in the pilot agencies received fewer in-agency therapeutic and non-therapeutic services (but not fewer phone calls), and fewer completed referrals (but not fewer overall referrals) for out-of-agency service over their time in foster care than non-pilot children and families.

At the end of 930 days, there were some interesting and potentially important differences between the two groups in terms of outcomes. There were no differences between the groups in terms of the percent of children who achieved permanence because they were returned home or in kinship care, although significantly more pilot children went to kin and significantly more non-pilot children returned home, which has enormous ramifications for children and their parents but not performance-based contracting as set up in Wayne County. There was also no difference in the proportion of children whose parental rights had been terminated and were either waiting for an adoptive home or placed in one. The only major difference between the groups was that a significantly greater proportion of the pilot children (22%) were more likely to have been adopted than non-pilot children (4%).

Politics Again!

Shortly after the conference I received a formal request from Vic for the state identification numbers of the children participating in the study; he wanted to do a cost analysis. I felt that the release of such information from the research project would violate the approved IRB protocol; it would also clearly go against the spirit of the participatory nature of the evaluation. I took the request to the evaluation committee – the agencies could release these identifiers but I could not.

The group went through Vic B.'s proposal re: cost analysis comparing administrative payment amounts to pilot and non-pilot agencies, focusing on per diem and performance payments...After reviewing Vic's memo, the group was very concerned that his analyses wouldn't be able to capture a number of different types of non-IV-E costs and payments, such as county, local, and even funds that private agencies contribute themselves. The group also expressed great concern regarding the accuracy of the state's quarterly records.

> *Bill asked for some direction from the group. The group agreed to invite Vic to the next meeting to discuss the matter further. Bill said that he would do this, and would give Vic a sense of the group's concern. (1/22/04)*

At the next meeting of the evaluation group, we thought this matter had been put to bed:

> *Bill said that Vic B. hadn't replied to his message, so Bill expects that Vic is no longer interested in pursuing the cost analysis. Carmine asked if FIA could force us to give up our data at some point. Bill said that they could not do this, since he has a contract with FIA that will prevent this from happening. (FIA would have to use a court subpoena in order to obtain such information.)*

Later in 2004 the real bomb hit, and it came from out of the blue. A bill introduced in the state Senate sought to discontinue the pilot. The language in the bill cited a study conducted by (unidentified) researchers at the University of Michigan[5] and cost analysis conducted by FIA that showed the pilot to be more expensive than traditional fee-for-service reimbursement as the reasons for doing this. I suspected, but never confirmed, that the new head of FIA had asked for this statutory language to be introduced. After all, the pilot was a Republican initiative, the preliminary findings that they heard were not "positive," and there was a battle raging between the State and Wayne County around who should pay for what when it came to foster care services.

A number of things about this turn of events bothered many of us at the time, and some of what went on felt like personal attacks. First, people from the State never produced the cost analysis that was done, even when it was formally requested by the nonprofit agencies in Wayne County. The "findings" from the cost study were quoted in very general terms, but the study itself – its assumptions, methodology, data sources, actual findings, etc. – were not. To this day we have no idea whether Vic just went off and did the best he could or produced what he thought was wanted. What we do know is that evidence that was used had to be based on agency-specific, rather than child-specific, data, as the state never had our subject children's FIA ID numbers.

Second, nobody from the State ever contacted me to discuss any new findings from the study or, for that matter, anything about the study at all. When I offered to talk with them about it, I was told they would get back to me, but they never did. They clearly had "heard enough" at the conference despite all of the caveats that were presented. Yet there were innuendos made that such discussions had taken place and that I had confirmed that they had made the "right" decision to close the pilot. Most painful of all was that after all of these years, some of the administrators at some of the agencies with whom I had worked questioned my integrity and thought I had been meeting with State officials behind their backs. After facing their questions and assuring them that I had never spoken to people at the State, my credibility was tentatively (but I never felt fully) restored, and the agencies confronted the people from the State with the fact that they were not aware of the most recent findings from the study or that more findings were still going to be produced.

Third, it became clear that the pilot had become embedded in a battle between the powers in Lansing and those in Detroit – what social services (and social service functions) the State was going to pay for and what the County was going to pay for.

> *Jim updated everyone as to the state of the Senate bill to end the pilot initiative. Wayne County is continuing to argue that it has no more money available to pay for foster care. Some private agencies have retained a lawyer to possibly push for an injunction against the County. Elizabeth Carey of the Federation [Bill Long's successor] met with a number of private agencies, and Bill and Bowen, earlier in May to discuss how to gather data to counteract some of the arguments in the Senate bill. (5/23/04)*

During the summer and fall, political efforts to save the pilot heated up. A number of groups organized toward this end, but the private agencies were fractured with some hiring their own lobbyists and others relying on the Federation to represent their interests. Interestingly, one of the groups that organized to save the pilot was led by a non-pilot agency in the study that had converted reluctantly to the pilot system only in 2002 – it was the agency that wanted to be a control agency because its leadership did not believe the pilot could be good for children and families. Their conversion to "defenders of the pilot" shocked me, and I didn't understand it – I still don't. They would never discuss their position with me, and their representative to the evaluation committee, who had been one of the most loyal, hardworking, conscientious, and thoughtful members of the group, began to withdraw from it. I still wonder if she had been ordered to do so.

Perhaps the executives at this agency, which was now operating under the pilot system, had found what I (and some non-political FIA folks who were involved with the project from the start) had begun to suspect and that the data seemed to be confirming – that less service could be provided and similar outcomes could be achieved and that agencies could profit under this form of reimbursement. My suspicion that agencies were, indeed, profiting from the pilot and doing so intentionally – a suspicion that made me feel very uneasy – started at the conference when the presenters were so severely challenged by the high-placed administrator at one of the pilot agencies, and grew even stronger afterwards.

This administrator was at one of the agencies that had helped to design the pilot and had the largest number of children in the study. Once the initial findings from the study came to light, he started coming to our evaluation meetings. His behavior at these meetings was disruptive (some told me that that was just his way everywhere, but I found him personally attacking and hostile). He questioned everything. Eventually, he also helped force from the agency (and our meetings) the representative that they had been sending to the evaluation committee – a man who was committed to the principles of participatory evaluation and to organizational learning. Ironically, this diplomatic, considerate man who cared deeply about children took a job with FIA in Lansing, but he would never talk with me about his former agency or about what he knew about the dismantling of the pilot.

> *Jim is leaving…in June to assume the directorship of FIA's Division of Licensing. This is his last meeting with us. Thanks so much for your hard work and good cheer, Jim! We will miss you tremendously! (5/23/04)*

I sensed the project had become a political liability when I was asked to meet with the Executive Director of this large agency – someone relatively new to his job. In the spirit of the participatory evaluation process, and hoping that we could keep the pilot alive until we had analyzed all of the data, we met in the early summer of 2004 in his office. What started out as a pleasant exchange about the study quickly turned into a "defense" of the findings. He called his "bulldogs" into the meeting – his data person and his "second" who had been attending our meeting. Together they questioned everything about the study, and when they realized that I could defend what we had done they tried to pressure me to "interpret things differently" (what a euphemism!). When I refused, nicely and politely, the meeting was adjourned. I should have anticipated what that would mean for the evaluation committee.

A Sad Ending

Bowen took his first academic job in the fall of 2004. Fortunately, by the time he left, the data set was complete and cleaned. There had been no fallout for us – yet. The agencies did all they could to help with the final data tasks.

> *Bill has closed three of four grants in order to consolidate the*

financing of the research project. [He] needs all remaining invoices from agencies ASAP. The project officially shuts down 8/15/04. ...Bowen is leaving for Portland, OR on 8/19. Bill thinks that we should have meetings every six to eight weeks in the fall in order to pass out new analyses and begin planning for the late 2004 dissemination conference. We'll need to decide when to hold the conference, how to organize it, and where it'll be held. Bill has already secured the money to hold the conference. (5/23/04)

The summer "farewell" meeting of the evaluation committee was a disaster – only four people showed up to say goodbye to a project staff of five, with whom they had worked closely for a number of years, and to Bowen. And this set the tone for the fall when I was the sole person left at the university who had been involved with the evaluation. Only two agencies consistently came to the evaluation committee meetings, and the two agencies leading the charge to save the pilot – the largest of each of the pilot and non-pilot agencies – stopped attending meetings at which new findings were being disseminated. Their representatives to the evaluation committee stopped returning phone calls and e-mails.

Clearly, with the general findings known, people lost interest and drifted away. And I had become *persona non grata,* not because any of the project's goals or procedures had changed, but because nobody wanted to know any more. The response to e-mails trying to rally the troops to learn more was something on the order of "just send us data memos." And when I raised the need for planning meetings for the final dissemination conference, many didn't respond, and those that did said basically "No, we don't need (or want) another conference." So I wrote and sent an e-mail to the committee, thanking everyone for their efforts through the years, reminding them of the many successes we had experienced, and formally ending the pilot evaluation.

Coda

I left the University of Michigan at the end of the 2004-2005 academic year. I had spent most of my time there putting together and carrying out the pilot evaluation. It was, in many ways, the crowning jewel in my 30-year research career; as a dean I will probably never do another piece of empirical work this challenging or complicated. It was the "perfect" study, done in the "right" way, with a wonderful group of staff and agency people, "blessed" by the research gods who allowed us to find funding when nobody thought we could "pull it off." It was, I believe, a stellar project carried out with integrity that addressed a topic of considerable importance to child welfare systems wrestling with spiraling costs.

But this experience will always leave me only half-gratified. And it leaves me more cynical than when I started it. Yes, there has been much knowledge gained about the impact of performance-based contracting on the functioning of a child welfare system and the children and families it serves. Yes, there will be many academic papers published in top-tier journals with our names on them. Yes, Bowen has written a wonderful dissertation based on some of the data collected (McBeath, 2006). And in the end, I do believe it was probably right to end the pilot experiment – the findings about fewer services were disturbing; and the findings about outcomes have become even more disturbing to me as we have more carefully and completely analyzed these data. I worry more now about what happens to children and families under incentive-based initiatives than I did before I began this project.

Yet in the end I can take no credit for policy change or the new direction that the State of Michigan has taken in reimbursing nonprofit agencies for providing foster care. I believe those were blindly political decisions made without credible data to support them. Nor can I say that I have helped agencies use our evaluation data to make smart decisions that improved their functioning while also improving how children and families are served. In the end, the goal of organizational maintenance clearly outweighed the potential benefits of any organizational learning that was done, and these agencies (as best as I can tell) continue to function in the same ways they have always worked.

But perhaps, most cynically, I have come to believe that most, *but not all*, child welfare agencies, and many of the people who work for them, care more about themselves than the children and families they serve. I am only grateful that counterbalancing this heightened cynicism are the few program supervisors and workers I met along this journey who still believe that children and families can be healed even if the agencies they work for are not healers. And our data show that these few people, working in difficult environments, can provide appropriate amounts of services, even when fiscal pressures, are present to the benefit of those they serve.

And as I think of these colleagues and friends, I am reminded of the principles that I have learned over my career as a community-based researcher and that were reinforced by this project: integrity is essential, so guard it at all costs; create a strong team, not a collection of individuals; question your findings exhaustively, since others will certainly do so; enjoy the process, since it may end unexpectedly; and move from disappointment to new challenges. For me, in the end, crafting and conducting this study has come to mean so much more than the "results."

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(Footnotes)

[1] This narrative is written in Dr. Meezan 's voice and primarily from his perspective. Dr. McBeath is, however, a full coauthor of this work, as he chronicled the process we went through together and was a full partner in the research. Parts of the experience reported here are taken directly from his field notes and his dissertation.

[2] Defined as the reunification of a foster child with his or her biological parent, the placement of a child with a relative ("kinship care"), the establishment of legal guardianship for the child, independent living, or adoption.

[3] While the foster care pilot did not contain some common managed care characteristics, it was considered by both FIA as well as private providers to be a managed care system. FIA chose to neither capitate foster care payments to private contractors, integrate foster care systems across agencies, nor create a single case management agency for the Wayne County pilot. However, official FIA documents mention the foster care initiative as a managed care system. Moreover, private providers have given public presentations in which they have referred to the Wayne County pilot as a managed care system. Agency personnel often equated the foster care pilot initiative with managed care. Therefore, for both FIA as well as involved private service providers, the foster care pilot clearly was an experiment in managed care.

[4] All italicized text comes from the archive notes from the project recorded by Bowen McBeath. Dates at the end reflect the date the evaluation group meeting took place.

[5] I suspect I was not named because of the public fuss I had made when Wayne County FIA pulled out of the initial evaluation, and had I been named in any way in this bill I would have spoken up that the data from the evaluation were not all analyzed.

House of Mirrors: The Messy Worlds of Interdisciplinary Community-Based Research

Karen M. Staller, Ph.D., Elana D. Buch, M.S.W, and William C. Birdsall, Ph.D., University of Michigan

What happens when a female assistant professor sends a young, attractive, smart, female doctoral student into the field with a gentle, older, emeritus male professor? Well, you get the "my assistant" incident or the "soup place thing" and some questions about doing qualitative community-based research with an interdisciplinary team. But you also get some unexpected insights about the research team, the community in which the researchers were received, and the research process itself. In this narrative, using this incident, the authors explore the house-of-mirrors effects of doing community-based interdisciplinary research.

House(s) of Mirrors

At times, doing interdisciplinary research is like walking into a house of mirrors at a carnival. You walk into a theatrically lit, interior space landscaped with concave and convex mirrors. Your image is bent and distorted, reflected and refracted, tossed back and forth in unexpected ways. Somewhere in those images you recognize yourself, your friends, and the strangers traveling beside you, but everyone is stretched and compressed, widened, and elongated in perpetually changing shapes as you walk from here to there. At the end of the maze of mirrors you are left wondering about the "reality" of those images and the one you know that resides at home in your bathroom mirror.

Doing community-based research with an interdisciplinary research team is a lot like walking through the fun house. You leave the landscape wondering which version of reality is being reflected and which version of reality you are capturing. You argue with your colleagues about what *you* saw and heard, witnessed and learned, all the while knowing if he or she was standing just a bit more left, or a bit more right, it would all be a differently shaped—but equally vivid—view of reality.

This narrative is about entering a house of mirrors, wrestling with some images, and learning something about what we experienced from different vantage points. It is about more than just "doing" the research, though. It is also about the temporal nature of crafting and constructing research writing. In telling this narrative we weave together current text with field notes and email that were captured in "real time" shortly after the incidents occurred. Our use of them here illustrates the iterative process of reflecting back and forth through time and space in constructing written reports.

Finally, given the fact that doing research and writing it up is contingent upon where one stands relative to others, it seemed only fair to invite the reader to step into the shoes of those doing the reflecting in their time and space. Thus, inserted boxes will indicate these positional hand-offs in the narrative. In short, we invite you to experience our house of mirrors as we bounce along through a narrative constructed from different temporal moments and situated positions.

Karen

Our Narrative's "Incident"

Bill calls it the "my assistant" incident. I call it "Elana's soup place thing." Either way you characterize it, the event occurred outside of my immediate sight. But I was the project overseer, so their experiences were reflected back to me from their different perspectives in fieldnotes, conversations, and emails. My responsibility was to do something with it. Elana was "my" doctoral student. Bill is an emeritus professor at the University where I was situated well down the academic chain from him, in my untenured, Assistant Professor position (but just up from Elana in the academy's scheme of things).

So what happens when a female assistant professor like me sends a young, attractive, smart, female doctoral student like Elana into the field with a gentle, older, emeritus male professor like Bill? Well, you get the "my assistant" incident or "Elana's soup place thing" and some questions about doing qualitative community-based research with an interdisciplinary team. But you also get some unexpected insights about the research team, the community in which Elana and Bill were received, and the research process itself.

An Interdisciplinary Research Team Studying Interdisciplinary Practice

Beginning in 2002, an interdisciplinary team embarked on a community-based study of a small rural county located several hours drive from our university home. The research team was an eclectic mix by any standards. Several folks floated in and out but there were six core members. We ranged in rank from doctoral student to emeritus professor but the team also included all ranks in between: full, associate, assistant professors, and lecturer. Our earned degrees included four Ph.D.s, two J.D.s, a divinity degree, and four master's degrees. Our disciplines included social work, psychology, law, economics, religion, and anthropology. Our practice experiences included policy analysis, forensic social work, and public interest law.

The core members of the original research team (excluding Elana and me) had worked together for years and had spent a good deal of time studying this community. They had primarily used quantitative court file data and sought to explain how and why this county obtained such an extraordinarily high rate of confessions and convictions of those charged with child sexual abuse. Their research had focused on explaining the relationships between variables found within the court records.

When I joined the research team, with my background in qualitative methods, it seemed intuitively obvious that the "better" way—or at least a different way—to ask and answer questions about the county's success was to approach it as an "exemplar" case worthy of in-depth observation. Questions framed about community success seemed to cry out for qualitative inquiry and a more ethnographic approach, although this was not an approach that any of the other team members had used in the past. My earliest tentative suggestions that we consider such a study weren't immediately heard, but ultimately and notably, with Bill's enthusiastic endorsement, this idea gained favor in the group.

We applied for and received a small University grant and immediately began our study. Although I was not a senior member of the research team, nor among its founding members, I was largely responsible for supervising this new research endeavor. I brought Elana, a doctoral student from the School's joint doctoral program, along for the journey. Often it was her training in ethnography, her understanding of the methodological approach, and her positive energy and support that kept me grounded as I tried to introduce an entirely new way of thinking to a more senior and better-established group of researchers.

The project involved targeting and talking to the professional practitioners in the community who were involved with child sexual abuse cases and their criminal prosecution, including judges, prosecutors, police officers and state troopers, defense attorneys, and social workers. So, notice for the moment if you will, the complicated sets of embedded hierarchies involved in this project. We were studying a group of community professionals associated with the legal system, a system with its own entrenched and historic pecking order with judges at the top and social workers off to the side. We were studying a type of crime, child sexual abuse, which involves issues of exploitation, power, position, age, sexuality, violence, and gender. We were studying this from the organizational home base of a university, an institution with its own equally hierarchical arrangements of rank and status. So it should come as no surprise that as our eclectic group gathered empirical evidence from this community setting and brought it back to argue about what it meant, we would encounter some tensions involving difference.

Strangers in a Strange Land

I had emphasized the importance of field notes to the research team for a project such as this one. Since the research site was located several hours drive away (first along a long stretch of interstate and then through meandering back roads of the rural countryside), and since the endeavor took time (over a year of doing interviews, collecting documents, reading local papers, and watching trials), it is not surprising that evidence of the passage of time and familiarity with people and place was recorded in fieldnotes.

Of course the more important observations in this kind of study came as researchers built relationships with people in the community. One measure of our growing familiarity with the research site is recorded in the evolution of notes on the "soup place." Over time we came to know the soup, the regular crowd including a local judge, and the owner Tim, and they came to know us.

ELANA/BILL

Soup today was good. Though it's hot and muggy, the menu is still Chili and soup of the day. Today it was Italian Wedding Soup, which was delicious. Two of the Judge's lunch crowd were there, though no Judge. (Elana's Fieldnotes)

We were a bit early for our interview, so we went…around the corner to have a cup of soup and hear the gossip. There was a 'Support Bush, Support Our Troops' sign in the front yard of the shop. (Elana's Fieldnotes)

Nell and I had soup at Tim's behind the court annex. Owner's name is Tim. Hellos all round. (Bill's Fieldnotes)

Got to town at 11:45 in good time to have soup with the good old boys at Tim's. The two guys who are the Judge's buddies…greeted "The Professor" and I joined them. (Bill's Fieldnotes)

KAREN

Notice in these excerpts the relatively different "eye" that Bill and Elana bring to filter and record information. This is critical since this information becomes the "data" for our research endeavor. Relying on one or the other alone would decrease the richness of the overall available material. Elana notes tastes, flavors, mugginess, and environmental context. Bill records his developing interpersonal relationship with the "soup place" inhabitants through his references to the "good old boys," "Tim," and "The Professor." Our observations that Elana would unlikely be welcomed in the same manner as "The Professor," and that Bill was unlikely to register details like the taste of the soup in his notes, lead to two implications. The first is that there is an interactive effect between and among research team members and community players which influences the very data from which study reports will later be constructed. Second, different kinds of recorded observations—interpersonal and environmental—make available a rich mix for reconstructing the scene and the community in later research reports. While Bill's notes help us consider the relationships among the various players, Elana's illuminate the context in which to situate those actors. Our sense of the scene would be different had the soup choices included lemongrass or miso and the sign planted in the front lawn read "Kerry for President" or "Another Family for Peace."

It is in local places like this that incidents transpire in which specific interactions result in deeper understanding, provoke more interesting questions, and generate more situated and complicated knowledge. It is here, at Tim's, that "Elana's Soup Place" incident took place.

The House of Soup

Bill and Elana had driven to town to interview a judge who was central to the project. It was our very first introduction to the "soup place." The day was reported in Elana's eloquent and rich fieldnotes, so excerpts pertinent to thinking about the subsequent "incident" are reproduced here with only minor editing. At the time she wrote:

ELANA

We went to find the judge, but he had already gone to lunch and left his assistant with directions on how we could find him. Turns out, he eats lunch every day across the street from the courthouse at a little soupstand/coffeeshop. The place (which didn't have a name advertised anywhere outside but—I gather from a poster inside might be called The Brew) is attached to the back of a beautiful paint-peeling Victorian home. If you weren't told about it, you'd never know this place existed. Inside, the restaurant looked pretty new (the Judge later said he thought it had opened about five years ago). The place consisted of a serving counter, a side counter with 4 or 5 thermoses of coffee (several flavors) and two tables with four chairs each. The wall behind the tables was made of rough-hewn boards and shelves holding empty glass canisters and empty baskets (which looked very quaint). The owner, dressed in a flannel shirt, jeans and white apron lives in the Victorian and seems to be one of the Judge's pals (although I don't know if he's his pal because the Judge eats there or vice versa). When we arrived, the Judge was sitting with three heavy-set, casually dressed men—all of whom were laughing hard as we arrived. At the other table, a lone woman was sitting drinking a milkshake and eating soup. The Judge asked her if we (Bill, himself, and I) could join her since there was no more room at the guys' table. After a few minutes of banter, the Judge told us to order—the place serves two types of soup each day. The Judge always gets two 'small' cups of soup—one of each flavor. Bill and I each ordered a large bowl of chili from the owner (I think the other flavor was some sort of tomato meat soup. They didn't look very different to me.).

Throughout the meal, the Judge, the owner and the three guys at the table proceeded to carry on…talking, laughing and teasing one another. One of the guys seems to be retired/

unemployed, one owns a construction company, and the other didn't say much nor was much said about him (so I can't even guess what he does). I'm not sure if the Judge is always the ringleader or if he was made so by our presence, but throughout the meal he would incite one or another of his buddies to recount a funny story, anecdote or practical joke for our benefit. Lunch was apparently a daily routine for this group—the owner even joked that one of the guys arrives every day at exactly 11:37, after the 11:30 stock reports. The others also joked about how this man is a "professional funeral attendee"—they (and he) joked that he goes to funerals and family reunions for the free lunch. They mentioned this again and again, each time getting a huge chuckle (probably as much out of Bill's and my incredulity that a person would really have the gall to attend funerals for food as by the general hilarity of such a practice). The owner joked that this guy hadn't bought groceries in two months...Given this man's schedule, he seemed either retired or unemployed and the Judge said he used to work at a nearby factory that has closed. The same gentleman seems to be the butt of many jokes, as the Judge told us (and then repeated the story to two other people who came by for coffee later in the meal) about how they had fooled him with some sort of elaborate plot involving a borrowed truck.

At lunch, the Judge talked for a while about college football. He told us a little bit about his family. Memorable details include a discussion about a man he met a few months ago while he and his wife were at the Mayo Clinic for their annual checkups!! The man was an African American WWII vet who had become a doctor and apparently has a photographic (or otherwise exceptional) memory. The Judge's discussion of going to the Mayo clinic, the Bowl game, and spending every football weekend attending games, as well as his suit contrasted with the more working-class aura of his companions and their mutual ribbing. The Judge's teasing and practical joking might be seen as a way of building camaraderie that spanned class or employment boundaries.

The Judge also spoke about how he ended up going to law school because he couldn't get into the Marines during Vietnam and "didn't have anything else to do," a story he told both at lunch and in our interview. Also, when asked if most people involved in the legal system and law enforcement in the county were local, the Judge reckoned locality by where the person had played high school football. I think we need to keep asking about the meaning of football and serving in the military in the community. From this discussion I got the sense that playing football and serving in the military were indicators both of proper masculinity but also of membership in the community (a feeling reinforced by the veteran's memorial at the corner of the courthouse lawn).

The judge also spoke of his father's childhood—he told one story about how his dad was one of four sons (don't know how many daughters). Their family was poor

> (maybe his dad's father died when his dad was young) and they had only two pairs of boots between the four boys, so they had to alternate which ones could go to school....
>
> Finally a few comments on gender—the Judge's buddies are all big, jovial men and when I asked the crowd at The Brew if they were all regulars, the lone woman shrugged a bit and said, "Ooh no, not me," as though that should be perfectly obvious. At lunch the Judge spoke a bit about how his dad used to have a lunch crew "just like this" when he was younger. I wondered who comes to this café when the Judge and his buddies aren't there? At the beginning, the Judge addressed the majority of his conversation to Bill, although he started to pay more attention to me as I began asking him to tell me more about his background. Interestingly, during the interview, the Judge theorized that the empowerment of women has been integral to the legal system's awareness of how it often put victims on trial in sexual abuse cases.

KAREN

In these fieldnotes, Elana speculates about class and gender and wonders how they play out in the community. She also recorded a personal observation that the judge did not start off addressing her as attentively as he did Bill, and juxtaposed that observation by recording the judge's personal philosophy on the empowerment of women and its impact on criminal prosecution of sexual abuse. Elana's notes suggest a certain tension between this observed behavior and his articulated philosophy that may be relevant to the subject matter of our study and worthy of further exploration.

The research team had observed previously that the professional practitioners in the community adhered to highly gender-specific roles. Judges, lawyers, and cops were primarily men. Social workers were primarily women. In fact, the one social worker who had infiltrated the "boys" network that investigated and prosecuted child sexual abuse cases was an anomalous and often renegade male social worker. This social worker, now a faculty member at a nearby university, was also our research team's community insider. Without him as a research team member, serving as community gatekeeper, it is doubtful any of us would have been invited into the community for study.

We came to see that among professionals in this community, masculinity and male responsibility were defined and elaborated through a set of common histories and contexts. Several prominent male prosecutors, cops, and a polygraph operator were all Marines in Vietnam. They were men with a strong sense of right and wrong who seem to have been shaped partly by their experiences in that war. They were particularly committed to protecting children who were victims of sexual abuse. More often than not, that meant protecting little girls from the adult men around them. Though our research team easily deduced these basic divisions of gender and status in the community, we began to hypothesize how these elements played out to explain this community's unusual method of prosecuting child sexual abuse. What intersections of historic context, personal histories, and professional position helped explain this community's devotion to prosecuting child sexual abuse over the past twenty years?

Returning to the House of Soup

The research team returned to the "soup place" on several occasions. Bill and Elana went together. Bill took another team member, Nell. Bill also went by himself. These are his fieldnotes from one of those solo trips:

BILL

Got to town at 11:45 in good time to have soup with the good old boys at Tim's. I got there and the two guys who are the Judge's buddies, Rob (the butt of the jokes) and the Bronco owner. They greeted the professor and I joined in. They talked to one another at first.... They asked me about whether I was retired, the aegis of the research and who was financing it. I said a small university grant. They asked me why their county and I said because they had a protocol that was very successful in prosecuting sexual abusers of children. We talked a little about that and Bronco brought up that his daughter (granddaughter) went to school with the 14 year old girl who ran off with the 56 year old guy—with a car and $4,000 of her parents money. It is a case much in the news. They have been spotted in the west here and there.

The judge comes in and he asks if they had told me about our last visit. "No." Tim, the owner, asks me if I remember the first question they asked me when I came in minutes earlier. I could not remember. They said it was, "Where is your assistant?" Turns out they (Rob and Bronco) were grinning and winking when Elana and I were having lunch because, I take it, she is good looking. So I will be a lot more welcome if I bring my assistant next time.

ELANA

Unlike me, Bill does not process the meaning of these incidents in his fieldnotes (although he will do so shortly). Nonetheless the pieces are there to think about. A man identified by his car (a Bronco, no less), questions Bill about work and money. Though it is difficult to say if these questions were motivated by concern about Bill's research motives or pure curiosity, they represent "Bronco's" attempt to understand Bill's status and position within the community and the academy. Just as members of the research team were working to understand the hierarchies and relationships between community members, community members strove to decipher the complex relations of power within the research team. Notably, Bill's mention of our focus on child sexual abuse prosecutions engendered the telling of a grand-daughter's 14 year old classmate who "ran off with" an older man, suggesting that in this small and intimate community, many people had knowledge of, or ties to, families involved in child sexual abuse. The subtexts of these two discussions— the first about positioning the research team and the second about gender, age and sexuality—merge when the soup place "guys" were alone with Bill, giving them the freedom to speak about me as "Bill's assistant." As they do with each other, the "guys" seem to be using teasing and joking (this time making sexualized references about Bill and me) to build camaraderie that spans class and employment boundaries with Bill, "the professor." Furthermore, just as we engaged in conversation as a research team about "them" in the community, so too the community seems to be engaging in conversations about "us" in our absence. Finally, I was intrigued to notice the parallels between the story of a 14-year-old girl running off with a 56-year old man and the guys' innuendo about Bill and me.

KAREN

Back at the Academy

I was troubled, although not particularly surprised, by the references to Elana recorded in Bill's fieldnotes. Ironically, in many ways Bill looked on himself as Elana's assistant, since she had had considerably more experience with ethnography than he. Bill always deferred respectfully to Elana and to my directions when it came to this project. Nonetheless, the direct reference to Elana as Bill's assistant by the "soup place guys" made it clear that was not the community's understanding of this relationship. Conversations with Elana revealed she was not particularly surprised. For the record, this is not the first (nor is it likely to be the last) time Elana has had to deal with being thought of as a sexualized object during field research. Such is the price women pay for being young, attractive, and engaging. Her most immediate emotional response to the situation was one of embarrassment. Being older and a bit more battered by life experiences, mine was anger. I get so tired of the same old battles that women have to fight to be taken seriously.

I found myself facing a dilemma. I did not want to leave the reference to Elana unaddressed with the team but was also not sure how best to bring it up. I was keenly aware of Elana's position. I didn't want to embarrass her further, nor did I want to exploit her, but I also didn't want her to have to absorb the indignity in silence. I pondered what to do, given my position as junior faculty member, team research leader, and Elana's supervisor and (hopefully) mentor.

While contemplating a course of action, I received an unexpected but very welcome email from Bill. A lifelong learner, ever willing to think about new things in new light, Bill had also been chewing over the issue. This is what I most respect about Bill, his intellectual curiosity and his willingness to explore the unknown. It was these characteristics that led Bill to advocate with the pre-existing research team members for trying these new methods of inquiry in the first place. Now, it was his intellectual curiosity and his willingness to tackle problems head on that compelled him to reach out to me and opened the door for addressing the issues. His initial musings were presented by contemplating the role of fieldnotes in the research project, and what ought (and ought not) go in to them. He wrote to me:

BILL

> I think most of the thinking about what should go into field notes is based on the lone researcher model, not the team model. I think that we are generally not giving impressions we have of one another on these trips. I got to thinking about that when I was thinking whether or not I should write up what the "guys" said to me about "my assistant" Elana, both as present and absent. I would never have conveyed those comments to Elana except in an ethnographic study. Further, I would probably have forgotten them in ten minutes.
>
> Because of what you told us about the importance of recording what happens and impressions, I could not have skipped them. I think they are particularly important because my impression is increasing that "we men" are getting something different in these interviews than "you women" would get. I would not have noticed that the Judge always talked to me, even in response to questions posed by Elana.
>
> The more I reflect on being kidded about my assistant when she was not present, the more I suspect that I was being kidded about the possibility that she was somehow more than that to me. I don't know. I do remember the '60s and '70s when

some older professors essentially traveled with roadies. Unless there was something like that on their minds, I don't think they would have been laughing so much about the winks between them that I missed when with Elana, or the laughter in telling me about that in her absence. Anyway, it probably doesn't matter much. (Bill's email to Karen, March 23, 2003)

KAREN

Of course it matters much. Bill's observations are important and he is right: when working as an interdisciplinary research team, observations of other team members, in addition to the "research subjects," necessarily become an object of the research process itself. Thus, reflexivity between the multi-person, inter-disciplinary research team and the community exponentially compounds the reflections that reverberate back and forth between observed and observers.

Furthermore, Bill's observations were important because without the imperative of fieldnotes, Bill would not have conveyed this information to Elana or me directly. Instead, he would have shielded us from the comments of "the boys," but for the fact that he felt he needed to "come out" as a requirement of the ethnographic process. What would otherwise have been a sexualized private conversation became a piece of information for public analysis. It seems central to the topic under investigation—child sexual abuse—to consider how interactions between researcher and community produce and reshape the intersecting dynamics of gender, age, and power. First, Bill had to trade on his inclusion in the soup place group of older men and their camaraderie in order for the research team to observe that members of the community were positioning researchers in ways related to their relative age/gender. At the same time, in order to reveal this information to our research group, Bill had to break the meritocratic norms of the academy in which individuals are expected to relate to one another based on intellectual identities and where sexualized identities and relations are generally taboo.

Finally, it is interesting to note the significance of what is hidden and revealed and the choices made in the context of the research process. Just as Bill noted that Elana's presence or absence changed his experience at the "soup place" so, too, did the fieldnote imperative shift Bill's decision about what to hide and reveal of his experience. Bill says that not only would he have not communicated the "guys'" comments to Elana and me but for the imperative of fieldnotes, he would probably have "forgotten them in ten minutes." Indeed, Bill probably would have forgotten about them and that would have been one of the privileges of his position. It was a forgettable incident for him, but not so for Elana and me. On the other hand, it was Bill's position of privilege and his openness towards those situated in ranks below him that facilitated the process of putting the issue on the table for further investigation.

Caught in Between: Her & Hims, Here & There

"This is a teachable moment," I thought after reading Bill's message. So I sent an email back to him with a list of questions to ponder and I asked if this was an issue that could be brought back to the research team meeting for discussion. Interestingly, although the team routinely spoke graphically about child sexual abuse—the object of our study—turning our gaze to sexual innuendo and power differences within our research team felt taboo to me and although I had long wanted to do so, I was unsure of how. In Bill's email, I saw the opportunity to illustrate the arguments I had made to a research team that wasn't used to thinking about the roles of power, gender, sexuality, and situatedness as integral and important parts of the research project itself. I had always talked about these concepts in the abstract; now this incident provided a perfect concrete example for demonstrating what I had meant. Furthermore, I saw an opportunity to protect Elana, or at very least insist that the group pay attention to her position, without having the burden resting exclusively on her shoulders. Finally, given my junior faculty status on the team, Bill's email created an opportunity to raise issues that made me feel vulnerable with my senior colleagues in a safe, acceptable, "intellectualized," and "academic" way. So I wrote Bill my own email. It read:

> These are wonderful reflections and go to the heart of this kind of research project. Among other things, this work assumes there is no one "right or correct" reality only different presentations and interpretations depending on where you sit—particularly where you sit relative to positions of power (race, gender, ethnicity, class, education, etc.). Anyway may I share this email with the group? I would like to put your fieldnotes, Elana's fieldnotes, and this email together for discussion at the next research group meeting. I think it would be very helpful. (More than helpful, I think it is critical to team development.) In the meantime, I have some questions for you to ponder:
>
> 1. If the guys at lunch did think/imply that there was more going on with you and Elana than meets the eye, did that enhance or detract from your credibility with them? Did it occur to you to correct the impression at the time? Why or why not? Did you gain something by not addressing their remarks? What? What are the bigger conclusions to be drawn? How far does this community of ideas and attitudes extend?
>
> 2. How do you suppose this environment made Elana feel? Even without direct comments do you suppose she "got" the social signals and signs? (The correct answer to that question is 'yes' so let me move past it.) Given the underlying message what are the implications for Elana's work in the community? For the project in general? What are the implications for other women in the community? How might this, in fact, be related to the development and working of the child sexual abuse protocol itself? Furthermore, how should we, as a research team, honor, interpret and understand data that we collect given our different positions relative to the information and its implications?
>
> 3. What other evidence do we have that either adds credence or discounts the notion that gender, power and access are related in this community? Who gains access? How? To what extent? Whose voices are amplified in the process? Whose are silenced? Are there ethical and unethical ways of proceeding by using gender?
>
> 4. For the purposes of this project, could interesting questions be emerging about gender, power, and sex in this community? Why the interest in sexual abuse at the expense of other things? Why the graphic language about cucumbers and such in the male-to-male interviews about child sexual abuse? Why the lack of such talk when there is a female present? Have there been any women who have been significant players in the operation of the protocol? Why

or why not? Have any women tried to be part of it and either been discounted or left out of the process?

5. What's sex got to do with it? What's gender got to do with it? Perhaps nothing. Perhaps a lot. What's professional? What's personal? Where are the boundaries between the two and what are the implications? I'm still of an open mind but won't dismiss any working hypothesis at the moment. This is where this kind of research gets interesting, dangerous, and complicated. However, in my personal (and very biased) opinion, this is where the complexity of the real world begins to get revealed in a meaningful way.

Research as a House of Mirrors

Our next research team meeting was devoted entirely to the soup place incident. The team was very open to pondering questions raised in the emails and fieldnotes, exploring feelings and reactions associated with the incident, and discussing implications flowing from it. We didn't answer all the questions raised in Karen's email; nonetheless the meeting did deepen feelings of team trust and respect. Both were nurtured through the exercise of revealing our individual vulnerabilities and explicitly acknowledging relations of gender, age, and power among us. The comments of the "soup place guys" forced us to publicly acknowledge our various positions relative to one another and to think critically about how gender, age, and status influenced what we observed, what we shared, how we interpreted, and what we wrote about this research project.

Much of what we've learned involves the methodological and interpersonal consequences of this kind of research. First, we were reminded anew of the very old adage in qualitative inquiry, that the researcher is an instrument in the research process. The data collected from any community setting depends largely on who does the collecting. As Bill, Elana, and Karen ventured into the field and found the Judge, Tim, Rob, and Bronco—among others—we each were told and we recorded different stories filtered through different observational points of view and interpreted through different lenses. This filtering happens throughout the process, including at the fieldnote stage, thus influencing the essence of the study data. For example, Bill recorded—almost unconsciously—his growing interpersonal familiarity with the group of guys at the soup place by labeling them "the good old boys," and learning the first name of the owner. Bill's choices, in the way he recorded what he saw and heard, revealed something to us about status as recognized in the community. The fact that the "guys" chose to call him "The Professor" tells us something about how they saw him. The fact that Bill chose to record the comments about Elana helped shape our understanding of who these community members were relative to us as well as who we were relative to each other.

Second, by sending different combinations of researchers into the community we captured different views of the same incident from different situational perspectives. For example, perhaps it was because Bill was so personally interconnected to the "good old boys" that Elana had the space to sit back, observe, and reflect on the environment—the taste of the soup, the peeling paint, the politics—as well as to pose hypotheses about the role of class, military service, and football. It may be that Elana was freed up to observe context because of the fact that she was not one of the guys; Bill was quickly immersed in developing interpersonal relationships at the "soup place" and therefore had less opportunity to observe the environment.

Third, sending different combinations of researchers into the "soup place" at different points in time resulted in access to different kinds of information. Not only is different information shared based on who is present, but selective absences are also relevant. So, for example, it was Elana's absence that created an opportunity for having a conversation *about* her. All of these "lessons" about gathering evidence are relevant to doing community-based research because they are directly related to the "data" collected and thus available for interpreting and reporting.

We also learned something about the processing, interpretation, and reporting of evidence. Interdisciplinary research methods need to include interpersonal processes ensuring that information can be shared safely among group members. The various recorded versions of truth, as witnessed, observed, and recorded by Elana and Bill, would be nothing more than flat, descriptive observations if the team hadn't found a way to create a safe space for public discussion about sensitive topics relating to power, gender, and position dynamics. We learned that it takes someone in the "power up" position at the beginning to give permission to discuss team dynamics around sensitive topics. This happened as I created space for Elana to voice her feelings and when Bill opened the door to Karen to bring the "soup incident" to the whole group. Furthermore, having dealt with these dynamics once, it was easier for the team to recognize and characterize them as recurring themes in subsequent incidents. For example, later in the project Elana made repeated attempts to schedule an interview with another older, male judge in the community. He was virtually unavailable to her (and the contact they did have was unpleasant). The problem was solved when a male attorney in our research group offered to deal with the judge directly. He received prompt and courteous responses. In the post "soup-place" atmosphere, no one blamed Elana for failing to schedule an appointment but rather immediately suspected that status and gender issues were at play and sought to address the problem by substituting the contact person with someone to whom we predicted the judge would respond, which solved the problem. We gained access to the judge and his views by utilizing what we had learned. Thus the understanding gleaned from "the soup place" incident was incorporated into the structure of the research group dynamics and utilized in our approach to this community.

Given our varied backgrounds, the research team has been remarkably cooperative in working out interpretations of data as we attempt to negotiate text for a book on our research. Although that is a project and process that is still underway, an interesting outcome of this interpretative process can be seen in the fact that individual team members have used the same pool of qualitative data to write up extremely different kinds of manuscripts based on their own disciplinary or professional inclinations. For example, Elana has written a piece on "confessions" drawing on her theoretical training in anthropology, and Bill contributed to it with his knowledge about the role of confessions in religion; one of the lawyers on the team has written a law review article which challenges myopic arguments generally made by feuding sides in legal debates about child sexual abuse; Karen has written an article on one of the trials we observed, using an experimental narrative voice; and Bill is working on policy implications drawing from his background in policy analysis. These individual articles, written out of the collected pool of data, have taken on a variety of literary and scholarly forms, are likely to find homes in very different kinds of scholarly journals, and will be made available to diverse audiences. Thus the interdisciplinary nature of the endeavor has allowed each of us to capitalize on the unique backgrounds we bring to the table and to the process and promote different kinds of findings.

These different approaches may give rise to the question of how the different perceptions of our team members effect our research conclusions. There is no final way to answer this question based on our experience. Bill offers a comparison from another study, and a different methodological approach, to make this point clearer and to raise it more generally for research communities:

> I have carried out multiple regression analysis of self-reported drinking and driving in a national survey sample. I did this with two women, a colleague and a student. A variable we focused on was gender; women, in fact, are much less likely to drink and drive and this difference is statistically significant. We report this result in the context of several previous studies of women's drinking and driving. If you ask me how I would have reported the empirical result had I carried out the research alone, the only appropriate answer is I don't know.

That is, of course, the only appropriate answer in qualitative research as well. We don't know how the diversity we observed influenced *all* of our conclusions. This does not mean that our experiences are not important for other researchers. We think they are. We have described how differently men and women, young and old are treated by community members with the obvious consequent limitations on the perception of any one person or even any homogeneous group.

In short, what we saw, what we learned, what we understand, and what we report reverberates in a complicated interplay of images here and there, between and among, them and us, I and you. Through this experience we learned that doing community-based research with an interdisciplinary research team required us to engage in individual as well as collective reflexivity. In the end it is all very much like a house of mirrors where you can't help but wonder about the variations of realities that are shaped and formed by the process of walking through.

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"The Good, the Bad and the Ugly": A Doctoral Student's Reflections On Doing Grounded Theory Research In Henry's Light

Kelly F. Jackson, LMSW, University at Buffalo, Mark Cameron, Ph.D., Southern Connecticut State University, and Karen M. Staller, Ph.D., University of Michigan

Kelly: *"It just isn't talked about," I thought to myself as I diligently searched through the available literature located in a popular database of research in the fields of psychology, psychiatry, and social work. I only uncovered one or two articles discussing the difficulties that MAY arise (not do!) in group qualitative data analysis settings. Is it taboo to even mention the conflicts that can and DO emerge in data analysis and interpretation where two or more researchers are involved? Working as a doctoral student research assistant on a grounded theory project, I DID encounter this kind of conflict. By reflecting on my discontent, I was able to learn more about myself as a person, student, and social work researcher.*

Mark: *Research teams, especially in qualitative inquiry, can bring a richness of perspective and other individual strengths that can help to produce rigorous studies of high quality and worthwhile findings. The interpersonal processes of these teams working together can make or break these efforts. This paper is about the latter. It is also about the clashing of paradigms; the painful misperceptions, indignities, and hostilities of race; and ultimately, about the unconsciously and compellingly attractive, insidious, and unblameable out (of these and all kinds of problematic human encounters) that is disengagement and abandonment.*

Introductions: Kelly

I entered the first year of my doctoral program eager and willing to get my hands dirty in research. I had previously served as a research assistant on an R01 grant that examined the impact of substance and alcohol abuse on children using quantitative methods. I had conducted multiple three-hour CATI interviews and I was excited about the new experience of using open-ended interviews available to qualitative researchers. I was quickly linked with a new and energetic junior faculty member who was rumored to be "the golden boy" at the University. Golden because Mark was viewed positively by the higher ups in administration and had a green light from the school to conduct his research however he found fit. He was a highly recognized educator—Mark had received the "educator of the year" award at the School of Social Work—and qualitative researcher who had several ongoing projects in the field environment of an urban city high school. When I first met Mark, I couldn't help but think how much we had in common: like myself, he had attended an Ivy League school for his undergrad studies and played collegiate sports (he played football and I played basketball)! In addition, albeit being a "white guy," his research focused on minority youth, the majority of whom were African American. I thought, "This is it! I found my niche!"

With the help of a senior faculty member, Henry, we designed a new, longitudinal study examining the effects of school discipline on students receiving special education services and their families utilizing grounded theory. I was not crazy about the focus of the study because I had no real interest in school discipline, but having limited experience in qualitative research, I was excited to jump in. Little did I know the events that would unfold would challenge me both personally as a minority and professionally as a social work researcher. The following will capture my reflections of "the good, the bad, and the ugly" times I experienced as a grounded-theory research assistant.

Mark

This is a paper about "doing" research in a troubled urban high school. But mostly it is about three people "doing" the research: a new assistant professor (Mark), a new doctoral student (Kelly), and a full professor (Henry). The research involved interviewing students of color and their families about (first) school discipline and (then) the realities of their school closing and what that was like for them. Mark is male, white, 47, out-of-the-box, unafraid, but still learning. Kelly is young, female, biracial, capable, nervous, and as you will learn, confused (at times) and angry (with me, after awhile, constantly). Henry, a long-tenured professor in the school of social work, is established, traditional, impatient with what is new and uncomfortable with challenge. This piece is about a number of things: the complexities and uncertainties of "doing" qualitative research; the clashing of research paradigms and the ideologies and biases and preferences that lie behind them; the challenges of field research in a school; the relationships of researchers and research participants; and the ways in which relationships and research endeavors fail. It is also about race and racism; about the indignities and hostilities of those kind of researchers vs. our kind of researchers; about the sheer and daunting anxiety of uncertainty; about desire for control; and ultimately about why truths contained (by tradition, quantification, and polite distancing) are preferred over truths unfettered.

We will present our stories by describing key moments in our work. Kelly will speak. Then I will speak. Karen acted as editor, mediator, counselor, and protector. Henry was not asked to participate until a draft of the paper had been written, and he declined to contribute at that point.

The "Pivot" Meeting: Kelly

A mandatory staff meeting was announced late in the afternoon over the loud speaker at Trend High (as I will call it), our field site. The principal informed students and staff that they should "act appropriately" and not respond to media cameras surrounding the school. The principal also announced a mandatory meeting for staff, which was to be held in the cafeteria immediately following the dismissal of students. The whole day was full of "buzz" about the presence of the cameras—along with the rumor that the school board was considering closing the school. At the faculty meeting, the principal stood over the teachers and staff who were sitting at the cafeteria tables. I remember feeling all this anticipation about what the principal would say to the group, almost as if my job/career hung in the balance. The principal announced the decision by the school board to "review" Trend High and decide whether or not it was "worthy" of staying open. The principal remained very formal in her announcement, but then leaned in slightly to the group and in a lower voice said, "Please look out for yourselves—I recommend looking for another job." The principal sounded sincere, mentioning she wanted to "look out for staff." It felt as if the Principal had some additional information she was unwilling to share and that it had already been decided, unbeknownst to the rest of us, that the school closing was inevitable. The principal ended the meeting by reminding teachers and staff they should respond "no comment" to the media gathered outside the front of the school.

I was very eager to meet with Mark to relay what I had just observed during the high school faculty meeting. I was out of breath, perhaps from running up six flights of stairs, as I tried to share my news and accurately report the happenings. I remember Mark turned to me in his swivel chair and asked me what we should do now. *[The swivel chair was always turning and twisting during intense moments during our data analysis meetings. He would often sit with his feet up on a corner desk and turn swiftly to the office computer in order to jot down a comment in the document that outlined our developing themes. It seemed like he was permanently linked to these inanimate objects.]* Usually in these moments of uncertainty, I deferred to "the boss" replying "I don't know, what do you think?" This time was different. I felt this opportunity was too great to pass by and out popped, "I think we should change the focus of our study!" Mark agreed! For the first time—in what seemed like an awfully long time working with Mark on the "School Discipline" study—I felt like I had a say. I felt empowered; I felt like a 'real' researcher!

We both agreed that there was little merit in continuing the longitudinal "School Discipline" study, considering the strong possibility that the school was closing. The next hour or so seemed frantic as we calculated what would have to be done in order to transform the old study into the new. Recruiting students receiving special education services for the previous "School Discipline" study had been painful and I remember fearing Mark would have me begin recruitment all over again.

We decided to change the focus of the study but keep the recruited participants (whew!). We plotted how to maneuver through the IRB-approval process in order to start collecting data and drafted an entirely new IRB proposal, Mark diligently typing away, turning around every once in awhile to face me in his swivel chair asking for my opinion or a suggestion. I remember feeling exhilarated, thinking, "This is what it really means to do qualitative research!" As Mark swiveled and typed, I envisioned myself out in the field, like Alex Kotlowitz uncovering the inequities of "our" urban youth. I could almost hear my heart beating wildly "Save our School!" as I fantasized about being with "my people" and fighting with them for their academic survival. We received IRB approval less than a week later for the project now dubbed "The Impact of a School Closing on Students Receiving Special Education Services and Their Families," and before I knew it, I was out in the field, tape recorder in hand, speaking with the people I had grown to know and care about.

Mark

When I heard the school was closing my first thought was, "Shit, months of work down the drain." Tenure pressures are pre-eminent for tenure-track faculty. We had spent a lot of time putting together the discipline study; Kelly had bent over backwards developing relationships with school personnel and recruiting students and now it looked like it was going to go sour. We sat in my office and talked and after about an hour it dawned on us that what we had to do was shift gears and study the closing process. It was, actually, a terrific research opportunity. I think our "discovery" stance, part and parcel of the grounded theory/qualitative research perspective, allowed this to happen, or even promoted it. Our research was not ruined. We could invite our participants to be part of a new project, one that was also compelling and that allowed us to continue our focus on organizational behavior and the lives and well-being of vulnerable students.

I think when we realized what we were going to do there was something like a moment of joy. We were happy in our research roles in that moment. I think we felt that we were responding to the realities and invitations of a compelling social situation and that we were acting, as researchers, responsively and humanely. Is this what researchers feel, ever? Is this how they view themselves, ever? Probably, but you would never know it. Quantitative research reports feel like the work of robots, to me. Should researchers have feelings? How do they work in the research process? We were happy doing something called for in the moment, something responsible and worthwhile, and I think we were a little proud of ourselves, too…Can research be in the moment? Might it be better in some way when research projects are spawned by events? Has this been done before? Are immediacy/responsivity valuable in research? In knowledge building? Many other questions are raised for me by this experience. Should researchers be more "portable" in this way?

Experimental and Control Groups: Kelly

Henry, a senior faculty member, was the study advisor and joined us in a meeting to discuss our progress on the project. I enjoyed Henry's presence, specifically because we tended to share similar views on research that differed substantially from what, at times, felt like Mark's chaotic "emerging theory" approach. Henry also frequently complimented me for my diligence in the field, an added bonus for any beginning researcher. On many occasions Henry agreed with me about Mark's reluctance to get directly involved interviewing and his bewildering views on what was happening with these families. Henry and Mark often disputed their views on their very different epistemological positions. Mark appeared to be a pragmatic constructivist (note the contradiction), while Henry a post positivist. I found myself more comfortable with post-positivist views because they had been the foci for the majority of my research and evaluation courses. This led to some frequently heated debates where Henry questioned the merit of Mark's method. At this point in the meetings, I would envision them as two male peacocks desperately spreading their plums in order to intimidate the other male: "Oh yeah well check out these feathers!" These two-hour meetings frequently felt unproductive considering I was the one who had to actually go out in the field to do the research, leaving the two peacocks behind to groom for their next "display."

During one philosophical wrestling match between Henry and Mark, I interjected an idea about transforming the project into a more deterministic study by incorporating a control group. My thought was to add a control group in order to compare the long-term effects of school discipline on non special-education students versus our participants who were receiving special-education services. At the time this option made sense (I learned this in school – this is what you are supposed to do as a researcher!) and it would finally add the clarity and solidity I so craved in this study. Henry agreed, both of us nodding our heads happily about the proposition. Mark quickly quelled our idea. He leaned back in his signature swivel chair, angrily shrugging off our suggestion, and re-established himself as Principal Investigator. With that and after nearly two hours of battling, Henry stood up, told Mark he should do what he wanted, and left the office. I watched feeling defeated as Henry walked out the door. Henry eventually left the project and I remained alone with Mark amidst the chaos.

Mark

So much for nobility and joy. What was this? Why were Kelly and Henry talking to me about setting up experimental and control groups? Doesn't anyone around here know anything about qualitative research? Doesn't anyone care about this stuff? Am I talking to the wall? I suppose the fact that this approach matches my personality and style and approach to the world (unstructured; challenging; empathic; inquisitive;

unconventional) that I simply fail to understand why others don't get it. I wanted Henry to participate because of his experience, wisdom, and interest in school-based research. But I knew Henry did not value qualitative research and I was unsure how it was going to play out. As we talked early on about the study, his opposition and exasperation with my sense of how to go about things were apparent and, at times, appalling to me. His mocking, teasing, and hostile challenges to my ideas, expressed in front of Kelly, saddened, angered, and confused me. I couldn't understand why he so harshly attacked me and my authority. I was surprised at Kelly, too. I thought she "got it." I wasn't sure what she was expressing. My sense was that the unstructured, spontaneous, and loosey-goosey nature of what we were doing had gotten to her, and she longed for something surer, more solid, more structured, more, perhaps, legitimate. Did she feel like this wasn't real or real enough? Was she scared about not knowing what she was doing? Was she scared that I didn't know what I was doing? Did I?

What does it mean to know what you are doing, in research? Does it mean knowing what others expect you to know? Which others? How much sense does this make? I remember a comment made to me at a conference by Terry Wolfer, a social worker and qualitative researcher, that one thing he had learned from doing his qualitative dissertation was that you had to make it up with each study you did. I often talk to my practice students about this, that good practice is created, that it is responsive in micromoments to lots of realities: the client's needs, problems, preferences, and what they are communicating in the moment; you; the agency; history; other things. Students struggle with this and lots of our colleagues don't buy this or want it. Structure. Evidence-based practice. Which equals, in my view, doing "what's right," and not what the complexities and emotions and exigencies of the moment call for. Is this too difficult and frightening for us?

If it is in practice then obviously it must be in quantitative research, where control is king. I sat in this meeting, having thought that we (all of us - the "team") had dedicated ourselves to a grounded theory study, exasperated, and feeling like an outlaw. Or a dummy. I tried to explain what we had set out to do and to explore Kelly's concerns. Henry shook his head.

Henry, by the way, had a habit of turning on the overhead fluorescent lights in my office every time he walked in. I prefer lower lighting and lit the room with a few table lamps. But in these meetings, when he walked in, click, the lights went on and things got (for me) intolerably bright. But I deferred to Henry, a senior colleague, and his predilection for light.

It may have been in this moment that I knew I was in this alone. It wasn't going to work. I think it may have been in this moment that Kelly decided to jump ship, which she did. I'm stubborn, and I wasn't going over the side. No experimental groups. No control groups. Bye Henry. Bye Kelly. I will carry on. I guess. Somehow.

The paradox: Kelly, the biracial female, vexed by my liberal approach, demanding structure and the good old fashioned way of doing things. Mark, the old white guy, pushing her to see things more freely, to let go of some of the established frames that in this case offered little but an opportunity to do work that might allow us a better chance of being seen as "legitimate." Henry, the even older white guy with authority over me, appearing to her as her anchor and voice of reason. What was really important to her? What were her real ambitions here?

Henry left early— he always left after one hour. "Who makes meetings for more than an hour?" He couldn't understand my predilection for three hour research meetings. I liked to get into it. [Maybe he didn't? Was this the problem?] After a little more discussion Kelly left. I sat there, alone, in Henry's light.

The Data Analysis Meeting: Kelly

I cannot stand data analysis meetings! I sit in Mark's office where, for nearly four hours, I have to watch him rotate around in his chair interpreting what the study participants meant by a word, phrase, and/or sentence in one of my interviews and record it in his computer. It was very trying because many times we did not agree and despite my contact and involvement with the family, my views were often trumped by his. In one heated argument we had over a statement, paragraph, sentence, or possibly a word (I cannot remember exactly what) I remember feeling like my head was going to explode. This particular transcript was of a married mother of two whose home I had recently visited in order to conduct the interview. I felt particularly loyal to this parent having been welcomed to the home twice for over-two-hour interviews. This mother was also a grandmother who along with her husband raises a son, a daughter, and a granddaughter in their home. Ms. Williams (as I will call her) is a very kind and supportive individual. She was happy to meet with me in her home and expressed appreciation for it since she would have had difficulty traveling because of a bad hip. We discussed for what seemed like hours, her negative experiences attending a school down south, and her hopes that her children would overcome their learning deficits and attend college. All the while Ms. Williams' granddaughter, Tonya, curiously inspected the tape recorder and adorably competed with it for our attention by repeatedly running into the room to say hello. Ms. Williams lived on the second floor of a two-family home and her mother lived below her. During my second interview, Ms. Williams introduced me as a friend from her son's school to her mother. I felt honored. [Sadly, Ms. Williams would later refuse to speak with any of the other research assistants when I left the project. When I attempted to reassure her on the telephone, she politely said she felt comfortable with me and was not interested in sharing information with anyone new.]

During this particular data analysis meeting, Mark was reviewing Ms. Williams' transcript and came across some words that, for some reason or another, stood out for him. These words began to transform into an idea which had him swiveling in his chair to add this "code" to his computer. His idea was that parents, this one in particular, seemed detached from their children and, because of this, had unrealistic expectations for their future. I quickly stiffened in my chair feeling the blood rush to my head (this was often a visible sign that Mark had triggered a button with me). I pictured the home I had just sat in, the photographs of the children framed on the walls, even little Tonya smiling up at me. I calmly voiced my disagreement, and Mark challenged me. I restated my point, emphasizing my experiences with the family in the home and describing how I thought his interpretations were misplaced. Mark challenged me again demanding evidence. (My most frustrating experiences with Mark revolved around these words. Why were my individual observations and experiences with these families not evidence enough? What kind of evidence is this man looking for? I felt like he needed me to be forensic scientist diligently swabbing under a participant's finger nail to provide him with 'real' proof. Do not my observations and experiences with the family count for anything?) I got very angry and argued against the injustice he was inflicting. Mark shouted back, "Where's your humility?"

I kept thinking, "Kelly you're out of line," yet I really could not respect this person swiveling before me in the glow of his computer light. I had to stand up for these families and students and, in a way, a piece of myself who has always wished I could have been more attached and connected to my African American heritage. I felt used and abused in the meetings constantly taking personal offense to what I thought were very racist statements and misperceptions. I had to often "stand up" for what I knew were good people because Mark characterized them as unloving, aloof parents. How often had I heard these opinions from white people who surrounded me all of my life? How many times had I not said anything, but wanted to? How many racist jokes had I heard by white people who felt I was "different" from the typical Black person who was the subject of their jokes? And here I was again in this very uncomfortable situation with what seemed like yet another "typical" white person condemning "those people" for not caring about their children and preferring to take the "easy" route of living off welfare. "They are to blame for their misfortune!" Isn't this the Moynihan report or the "welfare queen" belief reincarnated? Is not this the process of how "researchers" in the past spread horrible misperceptions that were quickly indoctrinated into American society? Are we not as social workers responsible for combating racism and putting these myths to rest? Yet here I am in a Ph.D. social work program across from a person who would prefer to blame the individual and continue to perpetuate the stereotypes that Blacks are "bad people."

I did not agree and could not help but speak up, even if it was disrespectful! This is MY work and to see it twisted and turned like this and recorded in his computer was way too painful. I remember sitting in these people's homes, listening attentively to their stories of misfortune and disadvantage, observing the toll poverty took on their lives and the lives of their children, and yet I, the stranger/researcher was still welcome in their homes and offered conversation. I watched the look on their faces when someone seemed interested in them—I saw what it did, I felt for them and relished in their many strengths. I am not stupid; I realize some of the parents we interviewed had limitations. The one parent that comes to mind herself appeared to have a substantial disability. She had difficulty reading words on our consent form and staying focused during interviews. I had met her at school where she had come to attend a school conference with the vice principal. She was very pleasant and open to speaking with me. As I sat in another infamous data analysis session with Mark and she became the target of critical debate of her abilities as a parent, I remember thinking, "Please don't play the race card on this one; the poor women is mentally disabled and probably has difficulty taking care of herself." Besides, despite her disabilities this woman worked a job and was raising two teenagers, yet I met her at school where she had made time for a meeting.

Many of the disagreements between Mark and me had a lot to do with my own personal beliefs and professional values as a social work clinician. As a therapist, I tend to look at individuals through an ecological lens, examining the circumstances, situations, and events that may or may not influence a client's well-being. For this reason I often find it very difficult to ignore the realities shaped by "isms" (racism, sexism, ageism, etc.). This makes it nearly impossible for me to blame the individual. I embraced the strengths perspective in my work with clients. I often felt this process of reflection was one sided. My passionate and very personal interpretations were not allowed in the data analysis process despite the merit of reflexivity in qualitative research. I was constantly called under the carpet for "being too concerned and connected" to these families to have an "objective" opinion.

But what about the PI himself? Had he mastered the ability to put his personal values and beliefs on the backburner while reading a transcript and recreating the circumstances from which it occurred? Can anyone really do this? His tendency to blame and point fingers seemed highly subjective for someone who had not spent one minute in an interview with a student or family member. I understand it is important to have someone who is outside of the interview process to assist in the interpretations of the data, but is it appropriate and responsible for the PI to be completely absent from the field yet have full reign over the conclusions that are drawn? Was I alone in this experience? Had other research assistants disagreed with their PIs over interpretation, or is this phenomenon just not talked about?

Mark

I love data analysis meetings. I love their literary quality, the study of words and their meanings. I love getting at what people are thinking and feeling and experiencing, at least to the extent that you can reading their (transcribed) words on a piece of paper. I like the twists and turns of thought and the supposition and wonderings that are a part of reading these testimonies as part of a team of people who are curious and wanting to learn something that is important and that can be shared with others. I also love the arguments. I like when I think this and you think that and then we have it out. I like the challenge, the thinking, the need to bare to another the quality and power of what you believe and then the need to defend it against principled, careful, thoughtful, crazy, stupid, emotional, worthless attack. It is good and it is good fun and my belief is that this can lead to truest truths about things, or, at least, really really good questions that can be asked next time. I like all this.

However, this makes other people crazy, and angry with me. I have run seven or so qualitative research studies in the past four years and in each case the research assistants have at one time or another become absolutely enraged with me. I am sexist, says one. I am racist, says another. I ask too much. I am disorganized. I am sitting contentedly in my office while they are in the field ACTUALLY DOING THE WORK. Why do I get to say what it all means? Because I have the degree? Because I know what I am doing and they do not? Because I am white? Because I am male? Because I am a pain in the ass and demand compliance? Or is it because I believe in what we are doing, in its goodness and promise, and because I feel a great and persistent need to fend off forces that seem to be designed to make it less vital, less true, and less idiosyncratic? Who knows?

Kelly and I had an awful time talking about the mother of one of our students. The mother struggled. She wasn't clear what school her son attended. he didn't understand many of our questions and seemed at times to be telling Kelly things to please her. I was taken aback by this. Other mothers also "struggled"; I am still not entirely sure how to describe this with respect and sensitivity, given Kelly's upset with me. It occurred to me that these mothers had vast limitations in terms of how well they could support their children's schooling and how well they could partner with teachers and advocate when they needed to. This seemed like an important finding in this study. I believed that what was important was that the school seemed to fail to appreciate the needs and challenges of these moms and that that would be important to say. Kelly thought that I was hateful. I didn't know how to talk to her about this in a way that convinced her otherwise.

I am not a proponent of the "strengths perspective." I loathe sloppy euphemism and fuzzy thinking gussied up to please a constituency, especially if it obscures truths. Social work *is* a strengths perspective. Social work research should not capitulate to the sentimentalities and the I-dare-you-to-contradict-me ideologies that hurt and that angry people demand in place of facts. This unfortunate crusade works, in my view, with some social work educators and CSWE, and we need to speak much more candidly/ bravely about the problems this is causing. But knowledge production should be as free of this as possible. That's the point, entirely, of the grounded theory approach: to escape imposed frameworks that disable and skew the discovery of what is real, complex, and lived, not theorized.

The Split: Kelly

I just can't do it! I have to step back from this project. It has become too personal and my disrespect for the PI is beyond obvious. I have to give up my attachment to this project in order to regain my sanity. I do not feel I want to be associated with a study looking to uncover yet another theory on how bad Black people are. (Shit! All anyone has to do is watch the news!) Is not this project about the unfortunate events of the school closing? I really would prefer to not be involved, though it kills me to give up that control – connection with families, all the hard work of recruiting, etc. It is too painful to watch all my efforts be torn apart by what seems to me to be yet another disconnected white guy who would prefer to stay in his Ivory Tower and point fingers from the turret at the undeserving poor. I do not have it in me—God help the other research assistants—who ever they are—Lord knows they come and go—why do I remain anyway? (Approximately six research assistants came and went during the course of our study, constantly threatening the project's stability and, particularly, the comfort level of our participants. Each time a new researcher came aboard, and in spite of my distance from the project, I had to write an introductory letter to families and personally call them to tell them about the new contact person.)

I eventually stopped coming to data analysis meetings. At first, I told Mark I needed to step back from analysis. He agreed and allowed me to leave research meetings before analysis. Eventually I found myself again an observer of Mark's interpretive process and felt disengaged. I realized my opinions ultimately did not matter. I recall one of the final meetings with Henry when Mark asserted himself as PI, leaving Henry for the most part displaced. I no longer saw the benefits of doing this kind of research. I could not see how our study could help our families and students or social workers. I no longer saw the purpose. It was time to leave.

Mark

Kelly abandoned the study to work on a diversity project with a female, Native American faculty member at the school. I thought this was perfect: she runs from a study of kids who are mistreated by design in schools, kids of color and their families, who are victimized by institutional insensitivity and racism to help someone set up a cabinet of videos on oppression. Fine. (The study continued with a succession of research assistants, a few of them, mostly overworked master's students, came and left as well.) Kelly went to a safe place. It is probably true that I climb out on limbs that most wouldn't want their cats on. But I don't understand the retreat of people whose lives speak to them of the necessity of risk-taking and convention-breaking, and I don't understand the allure of the conservative. Things have been turned on their head: faculty are making careers for themselves and fabulous livings doing "diversity training." It's become an industry. Has it served us well? Where are the politics in this approach? Does it have anything to say about power in ways that help us to change problems of power and disempowerment? Or has it become a feel-good exercise for us? Is all of this changing lives, changing conditions of life for the oppressed? Or is it another way for US to feel in control? For US to feel empowered? Why is this study (of some of those lives) less important than a video cabinet?

Temporarily Coming Together: Kelly

Why is this so difficult? Is this really how qualitative research works and is this really the method for me? All these questions kept coming up to the point when I began to feel it would be better in the future to do quantitative research and avoid these highly agitating experiences. Instead I took an advanced qualitative research course focusing on Grounded Theory in a different department at the University. I was motivated to take this course to "prove Mark" and his disorganized approach wrong. In the class, I was able to see a different perspective of qualitative research and embrace the rigor that ultimately supports the method. I learned that there are different ways to do qualitative research and I found what seems to be a better process. Although I do not think the researcher should avoid interviewing subjects entirely, I do feel it is important to have a peer who is unassociated with the study to review tentative conclusions as a form of triangulation. I have also learned how to incorporate more organization into the process by using qualitative software and memo notes. I felt informed and was excited to once again experience the green grass of qualitative research.

I received an e-mail from Mark after about a year of being away from direct involvement in this research project (from time to time I would receive e-mails from his research assistants requesting participant contact information or from Mark requesting numbers of recruits—which always made me feel connected to the project and the most well-informed assistant) asking if I would be interested in co-presenting with him at an upcoming presentation by a center at the University that was involved in funding parts of the study. The e-mail initially triggered feelings of apprehension considering some of our past heated debates and the inability in our working relationship to agree on several key findings in transcript interpretation and data analysis meetings. I admit the pull to re-involve myself with Mark was not very strong and my initial e-mail draft was a polite "no thank you." However, as I looked over the e-mail, I realized the offer would provide a two-fold benefit; the presentation would be something useful to put on my CV (something every doctoral student wants), but the opportunity to add my "slant" to the overall themes of the project and once again "advocate" for the disengaged families. This made reengagement irresistible. Subsequently, Mark contacted me to discuss the presentation and form a draft outline of the presentation. As I walked into the office, I felt a bit nervous considering some of the many "battles" that had taken place in that room. I focused on the other chairs in the office, thinking about how many different research assistants had occupied those seats. Mark was characteristically sitting in his swivel chair. Why was I doing this to myself again? Could I really expect to influence the "voices" that emerged from his computer?

Mark

I called this our rapprochement. I said this word to Kelly and then felt a little funny about saying it with a slight/lousy French accent. It just seemed truer to say it that way but I told Kelly that I was concerned that I would seem arrogant using a French accent. It just seemed like an apt way of describing our reunion to present this work and to write this paper. Even in the reading of the (I think) unfair characterizations that Kelly has written here, I am immensely pleased. We are now working together on something good and important and we agree that it is good and important and that perhaps telling our stories might somehow help other researchers struggling to understand the boundaries and wall-less and unechoing vastness of uncertainty in so many moments of qualitative inquiry. I love her honesty. I love (mostly) the sting that it produces in me. However, it remains to be seen if this rapprochement is real and stands for something, or if it is a way for her and for me to go on record about what happened (to get a publication out of this fiasco) and to say something like "it wasn't my fault."

"Writing Up" This Narrative: Kelly

In order to accurately portray my experiences on paper, I needed to revisit very powerful memories. This was a very arduous process for me as I attempted to regurgitate the past. Many of the memorable thoughts and feelings I have are of the students and families I interviewed and observed during my involvement with the project. The majority of the students who lived in poverty were of African American descent, and had various learning deficits. They had, unfortunately, for one reason or another, ended up at a school that many "outsiders" considered a breeding ground for future criminals and illiterates. This was a harsh and difficult reality for any researcher to ingest. It became particularly hard for me because I connected to the participants with whom I shared a cultural heritage and with whom I had worked as a social worker in a local child and family service agency. I remember often feeling overwhelmed by the inequities these students and their families faced on a regular basis. Here I was observing them like a zoo attendee until my "shift" was up and I headed home back to white suburbia. Yet I was told this was very important information to capture (really?).

I kept thinking, "Wouldn't my time be better off spent working as a volunteer social worker in the school linking students to different after-school programs or helping families understand their rights within the IDEA laws?" Was that not my ethical duty as a social worker to help people? Isn't that the purpose of conducting research in the field of social work? I remember really wanting to help students and families in any way possible to perhaps change even a small area of their lives in a positive way. One student, for example, would occasionally show up at the research office (a space the school administrators gave us to house our computer and interview paraphernalia) "just to talk" to me. It was obvious this student had some serious concerns, having told me about his gang involvement and conflicts with peers in school. Browdy (as I will call him) was a very social 15 year old who often "annoyed" teachers by missing class and performing poorly on school exams. Browdy also had a medical condition that caused him to have to go to the bathroom frequently throughout the school day. His mother told me in an interview that Browdy had come home from school one day with wet pants. Supposedly one of Browdy's "annoyed" teachers refused to let him go to the bathroom. His mother looked tired, having just returned from a graveyard shift at a nursing home; she was even more tired of dealing with unsympathetic teachers and administrators at her son's school. Listening to stories such as this made me question my role as a researcher. I felt uncomfortable just uncovering the injustices that were continuously affecting our six students and their families. As I sat in data analysis meetings, the idea that these families were living this unfortunate reality haunted me and subsequently affected how I would interpret their words and draw conclusions about their lives.

It's a Thursday night and I am typing away at my laptop recalling my experiences on the "School Closing" project. In the background, the local news mentions that Trend High School's doors will reopen for students from another, more prominent public city school in the fall of 2006. The students will be "housed" at Trend High until renovations are completed at their former school. I wonder what effect this will have on our students who in a sense were kicked out of their building only to be replaced by more privileged students who are using the school like a hotel. I am immediately drawn back into the study and the lives of our students and families. I wonder if we did them an injustice by just studying them. I wonder if spreading the word about our study will in some way help, even in an indirect way, similar students and families who may be faced with the closing of their own neighborhood school. Is this not what every researcher hopes? I think we can make a difference and in realizing that I feel compelled to stay the course with Mark and our grounded theory study - for better or for worse.

Mark

We (I?) will get back to analyzing the data, which had to be put on hold for a while. The study will describe, primarily, the ways in which this school failed to keep its promises to work as partners to these students and their parents in support of their transitions to new schools and other opportunities. It will be about the ways in which the school made promises that it knew it could not keep and then systematically did things the ways that most organizations do things: it did what it usually did, primarily, things that had been done before; things that were comfortable; things that involved (generally) less rather than more work; things that involved little communication and power sharing; but also, things that to other people would look like reasonable practices. It will be about how organizations like this school are faced with complex and, arguably, impossible tasks that are both unachievable and undiscussable (see Argryis, 1990). In these situations, people have to manage their work and their energy and their stress by doing "good" practice that they know (at least subconsciously) will fail. That, I think, is what the study will say.

I think there is something about us and our experience together that parallels this. I think that Kelly and Henry wanted "reasonableness" in our work together. Kelly wanted fairness and kindness and compassion and a story that was less disturbing. She wanted me to agree with her sentiments and her advocacy position and her longing for structure and her need to see rainbows and to help. Henry wanted me to listen to him (to reason) and agree with him and do what he did: learn from one of the BIG SCHOOLS how research is done and do it that way. He wanted me to be less creative than I was and he wanted me to be as unwilling to take risks as he was. He wanted me to be in the field. (I could not be in the field though I agree that that is optimal. I had taken on too much and could not; also, though, I had learned by being thrown into the field and this responsibility helped me in ways that I thought would also benefit Kelly.) Henry also was disappointed that I wasn't more interested in finding out what the school personnel and students and families wanted to learn. He wanted me to have them to co-design the research with me. I believe, participants in my studies design the research with their words, their questions, their relationships with the people to whom they are telling their stories. Each study is unique. During each study the process of "doing" research creates, in my experience, many, many unique moments which then invariably change the following moments and the studies in which they occur, indelibly. This, to me, helps to get at truths. It is this openness that allowed us to go from study 1 to study 2 and to capture some important, perhaps even profound, things. Study participants did help design this study, but in a way that Henry did not favor, or perhaps, understand.

My research is important to me. I want to do it the best I can, which to me means carefully designing and modifying as I go to position myself for maximum and "truthful" discovery. My best work is intensely personal and creative and not (much or often) like others' work. It is unique. But it was not how Kelly or Henry would have done it. I think to them it didn't much feel like other things they had done or wanted to do or, ultimately, how research "should" be done. They didn't understand what I thought was important nor did they believe in me. They did not want to follow me. So they left.

"Writing Up" This Narrative: A Coda - Mark

One of the reviewers of this article asked us to address certain aspects of our team experience here in a final section. Among other points, the reviewer asked that we discuss the impact of "power differentials" and "non-democratic teams."

What were the power differentials among the three of us? Henry was tenured. Henry voted on my tenure. I was an untenured faculty member; I had no formal nor any meaningful informal means of influencing Henry's career (or views, apparently.) Kelly was a student. In my role on this project, I was not Kelly's teacher (I could not grade her), and I was not her boss (I did not pay her). I knew the most about qualitative research (I was often—not always—in the best position to judge what made sense to do.)

Ultimately, did my knowledge make me the most powerful? Henry remains a tenured full professor. Kelly is a doctoral candidate who performed well in her courses, is teaching in the master's program, is well thought of, and now has a first authorship publication. She will likely be applauded by readers of this piece for giving voice to important concerns in the face of an oppressive other. She continues her work with other faculty. Her future seems bright.

I was told in my most recent faculty review (a process in which Henry participated) that I was "untenurable" due to my "pattern" (not "numbers") of scholarship. This refers to fewer publications during data collection and then more as data are analyzed and manuscripts are written, a flow type that I would think to be unavoidable for most beginning qualitative researchers (do other untenured, qualitative researchers also have "pattern" problems in their review processes?) I had to leave.

Who had the power?

On "non-democratic teams": the editors' reading of this piece persuaded them that this was an apt characterization of our team. I don't think a qualitative research team can be anything other than democratic. Qualitative data analytic teamwork in some ways is and must be ultimately democratic, actually. At least in a grounded theory approach (see Strauss & Corbin, 1998), the (very) free flow of ideas, feelings, tangents, biases, conceptions, and misconceptions shared with analytic partners, then carefully examined and tested against data, theory, and knowledge constitute the heart of the analytic exercise. This is democracy at its best, in a way: all can have a say, and must, really, to achieve deepest understandings of what is being studied. In my view Kelly was a full participant and partner in this, and her input in data analysis was profoundly important and, I would argue, honored. While we argued over, for example, how to best characterize the mother who struggled to remember what school her son attended, Kelly's poignant and compassionate pleas that we not be disrespectful to her were critical in shaping our best and fairest portrait of this woman and her life. But I did not simply agree with Kelly because she was being nice; I challenged virtually every assertion so that we could substantiate our views and build together the truest stories of these people and this school. Honoring someone's opinions does not mean the kind of polite, unquestioning capitulation that seems to be what Kelly and others sometimes seem to be after. To me, democracy is not about groupthink/Candide-like optimism; it is about a group's capacity to consider matters of importance to all of its members fully, complexly, and honestly, and to share those considerations openly with one another without fear of coercion. I thought that we had achieved this.

But this kind of democracy is not what some prefer. Is it possible that there is a new democracy, in which the views that ostensibly are intended to honor and protect those deemed at risk may not under any circumstances be challenged, or even examined? Is this what we have (unfortunately, perversely) achieved with our diversity curriculum? And what democracy exists within the university tenure system? Were my questioning style and unconventional points of view what drove Henry away and motivated my review committee to reject (eject) me? If the point is to do things as everyone else does them, what will we ever challenge? If that is true then what is social work? Have we really become this conservative? If I am threatening to both Kelly and Henry, then what is it that I am threatening? Who was coercing whom?

And to you, the reader, please allow me to ask: When was the last time *you* actively encouraged someone under your supervision to criticize you this directly, this vehemently, this publicly, this recklessly? (If you haven't, why haven't you?) Do you think that this publication (Jackson, Cameron, & Staller, 2006) is likely the product of a "non-democratic team?" You, the reader, must decide for yourself. Who here was the good? The bad? The ugly?

Kelly

During my days on the project, interchanges between Mark and me, especially during data analysis sessions, often left me feeling affronted. It was my experience that my interpretive positions were often challenged and my values and beliefs about the participants regularly assaulted whether grounded in the data or not. Though Mark may have a differing opinion about the interplay of power in our research relationship, serving as his research assistant and enduring his unconcealed discrediting of my interpretations **was** a very devaluing experience. This contributed to my exiting the project (two semesters after my official year-long research assistantship ended).

I continue to struggle with Mark over our very different beliefs and values, and unfortunately the dynamics of power that afflicted our research relationship before were repeated again in writing this manuscript. Old lines of power were redrawn and feelings of disempowerment reestablished, inevitably producing even more conflict. My desire to leave was reignited. I am hopeful that being reminded of my feelings of disempowerment instigated by these very familiar dynamics have led me to tell a more accurate story of what happened. In writing this piece, it was also my hope that the harbored frustration could have been resolved between the two of us, or we could have at least moved from a less angry place to one where we 'agree to disagree', but as Mark said to me in a recent telephone conversation – "Sometimes there are no Hollywood endings."

Food for thought: This is a controversial issue that some readers may shrug off as attributable to my inexperience, complications of counter-transference, or miscommunication between researchers. Nonetheless the question still remains: why is it not talked about in the literature? We social workers are constantly walking on thin ice in our attempts to protect clients' well-being, and in this case our research participants. Yet we are naive to the fact that controversies and ethical issues arise within research teams on qualitative projects. Do we not have different backgrounds, beliefs, values? We are taught as Master's level students to discuss and reflect on our personal triggers in preparation for working with certain clients in a clinical setting. I believe it is just as important for us to learn that these tools are also necessary in research. We must be constantly in touch with ourselves and keep not only our clients safe, but ourselves as well, from the very strenuous and challenging aspects of qualitative research.

A Closing Note from Karen

At the time I got called upon to intervene in this writing project—or perhaps to intervene in the relationship between "research team" members Mark and Kelly—they were trading barbs through penned paragraphs. For me these two projects, writing and relationship mending, melded during collaboration. The net result was a smoothed out, toned down version of their original manuscript. When the editor called for minor revisions because that work was "missing a final section" and asked "now that the relationship is either over or redefined how do people feel?" I thought it would be easy to finish off. I was wrong. I witnessed first hand the explosive and dynamic nature of the relationship between Mark and Kelly. The resolve that I thought was cleanly negotiated as a product of the first manuscript draft turned out to be nothing more than a temporary truce.

Let me briefly explain my relationships with Mark and Kelly and my role in the project. Mark is an old friend from our doctoral student days. I have great respect for Mark yet am also familiar with his challenging style of engagement. We are now in faculty positions at different universities. I had not met his student, Kelly, but I quickly identified with her position. In our most recent conversation, we all acknowledged I was brought on board in order to level the playing field between Mark and Kelly. I hope I have served them both, while always openly acknowledging my long-term friendship with Mark and my inclination to align my sympathies and arguments with Kelly. Now, at the risk of alienating both friends, old and new, I step back and try to articulate the great gulf I see between them and its relationship to their research and writing projects. I had not anticipated taking on this role in public. In my view, it would have been better if they had written their "lessons learned" together. However, it was clear, given how personally and emotionally involved each one was, that synthesizing their experiences as a third party was virtually impossible. Of course, I can only give you my outsider interpretations of where I see the fault lines.

Power. Kelly saw Mark as "in power" given where he sat relative to her. Included on the list of factors that put him in power was his gender, his race, his university faculty position, and his role as PI on the research project. Certainly this is how most of us have been trained to understand the notion of power. Mark, in turn, reminds us that power is relative. He was conducting research from a setting that had its own power dynamics. He was powerless in a tenure decision that changed his life and disrupted his research but had little to no effect on Kelly or Henry. He was upset (and maybe even a little threatened) when Kelly employed Henry's help in siding against him. So I'm left wondering about the activity of parsing out power relations into bite-sized pieces for analysis. When must you step outside the "research team" dyad and consider broader contexts? Power dynamics are neither simple nor unidirectional. Perhaps to better understand their workings in a research project we must look at both local interactions between members as well as the institutional, political, and social settings in which those interactions are taking place.

Democracy. Mark believes that their data analysis sessions were democratic. Kelly does not. It is a deep-seated disagreement. Democracy, Mark argued is achieved when we "all can have a say" and he reports Kelly was a "full participant and partner." Kelly didn't feel like a "partner;" she felt like an "assistant" or a "student." Mark believes democracy doesn't mean "unquestioning capitulation" to another person's views. Kelly believes that Mark, by virtue of his PI role and faculty status, simply had the final word regarding interpretation, thus defying the very notion of "democracy."

Of interest here are contested notions of democracy in a research team. Mark believes that democracy is achieved when all parties voluntarily and vigorously participate. After all it isn't every faculty member who would invite a doctoral student fully into the debate. At the risk of putting words in Kelly's mouth, she believes democracy is achieved when a minority voice is heard in the final outcome. Students of democratic theory will recognize a classic line of argument from the voting-rights literature here. Should success be measured by equal access to the polling place, or should it be measured in terms of political outcomes? Gerrymandering can undermine democratic outcomes even while presenting a facade of equal access. Returning to a research context, which form of democracy is better? While there is scholarly debate about the extent to which to honor interpretations offered by research *subjects* in qualitative inquiry, to the best of my knowledge there isn't an analogous discussion about what to do with differing voices within research teams. It is an important topic because the very essence of the "results" reported depend upon it.

Conflict. It became clear as Mark and Kelly argued over the final sections of this manuscript that they have diametrically opposed views on the role of "conflict." Mark sees difference as an invigorating and stimulating part of the creative research process. He loves heated debate. Mark can engage in passionate discussion and, at the end of the day, still be your friend. He can be hurt in the process but he promises to get over it. Kelly and I, frankly, are not so readily comfortable with this kind of conflict in this kind of context. It is possible to experience it not as a creative force but as a destructive one that eats away at relationships, rapport, and trust. This is particularly true if meaningful reconciliation can't be achieved –or what Kelly calls "agreeing to disagree"—in its aftermath. Unresolved conflict can morph into anger. There seems to be a fine line between having open, honest communication on the one hand, and hurtful, mean spirited, bickering on the other. Mark and Kelly fundamentally disagree on the relative nature, force, and usefulness of conflict in the research endeavor.

Racism. Throughout these discussions there has been intense disagreement about the role of race, the notion of racism, and the label of racist. Mark is tired of being labeled an oppressor by virtue of his gender and race, by society in general, and by Kelly in particular. He sees this as a perverse, but perhaps inevitable, consequence of uncritical acceptance of the diversity movement. He endures being labeled by others because there is no alternative, but denies the allegations are accurate and would like explicit empirical evidence from those making them. I believe he might say his more nuanced and critical position in this regard offers him an excellent vantage point for understanding the people he studies. This angers Kelly, who feels that you need to have stood in the shoes of those who are oppressed to fully understand its nature. You must walk inside the homes of "her people" and bear witness first hand, in order to disentangle one form of disadvantage from another. It was her experiential knowledge, among other things, that she offered to the research project, and she believed that this insider knowledge and insight ought to have been better respected during data analysis sessions.

It is too bad to leave such provocative issues on the table and unresolved. There are no Hollywood endings, nor will simplistic answers suffice. In the end both Kelly and Mark might just agree on this.

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Reflections on Funded Team-Based Social Work Research: Collaboration or Cold War?

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This narrative examines the challenges and opportunities of funded team-based research with a focus on the collaboration aspects of the work. What is presented is a reflection on what the authors have learned during twenty years of collaborative research. The authors wish to take a long view from years of research experience to help others be better prepared to face the rewards, frustrations, challenges, and tribulations of this kind of research. They also suspect that we are trying to normalize the experience in all its incredible peaks and valleys.

Collaboration is one of those wonderful words that has fallen from grace, and frankly we're sorry to see it. Several years ago it seemed to describe something almost mystical, a coming together of the minds, a synergy in which partners join in well-intentioned efforts to make something important happen. Collaboration was characterized as an opportunity to go beyond individual competence to a place sometimes even beyond original expectations. We still know that can happen (indeed we have experienced that in our own collaboration), but we probably need to invent some new language to describe this type of productive mutuality we so admire because recently we note collaboration has been co-opted by the federal government and other funders who seem to be using the term when grantees are working to the funder's specifications, not as equal partners. Collaboration has become murky territory, especially in research and particularly in funded research. We continue to believe that collaboration is a wonderful impetus for creativity and collegiality, but we do have some concerns.

This is a series of reflections about a long-standing collaboration and what we've learned in the process of joining with others to conduct funded research. We offer this patchwork of stories almost as a parable to better prepare the new researcher or even the seasoned researcher undertaking collaborative work for the first time. We wish to take a long view from years of research experience to help others be better prepared to face the rewards, frustrations, challenges, and tribulations of this kind of research. We also suspect that we are trying to normalize the experience in all its incredible peaks and valleys.

Our Collaboration

We began our collaboration as two faculty members in 1997 when we wrote a chapter together in a niche book on incorporating gender into the social work curriculum in which we realized that women were essentially invisible in organizational theory (Netting & Rodwell, 1998). We actually knew this already, but in writing the chapter we discovered what an impact that omission had had on our own education and subsequently that of our students. That chapter began a process in which we developed syllabi, taught courses, started research on the influence of women in early human services, and began a virtual writing campaign to expand our and others' thinking about the potential for alternative, non-dominant perspectives in influencing the next generation of social workers to be more multi-paradigmatic in their thinking and practicing (Netting & O'Connor, 2003).

One of the first pieces we wrote (O'Connor & Netting, 1999) was entitled

"Teaching our Students about Collaborative Approaches to Organizational Change." In this piece we assumed that if we raised consciousness about how incredibly reductionistic and non-inclusive rational, planned change models can be, and then proposed a collaborative alternative in which multiple voices were heard, social workers would have more options. Looking back on the article we are amazed at our naiveté. What we failed to say in that article was that there are different kinds of collaboration. Occasionally they are one and the same, but probably true collaboration occurs less often than we would like (or have been led) to believe. There is the kind that funding sources and others leverage people to do that goes away when the funding ends. And there is the kind that *really* happens. The kind that really happens requires incredible stamina because if the collaboration moves people to a new place, it is met with resistance when powerful forces recognize that they might have to relinquish control of "what is." Yet, if that can be worked through, what a transformation can occur for the people involved and what they can produce! The leveraged kind, however, means that many collaborators go through the motions of working with mutuality. On the surface it may even seem progress is being made, but most participants have absolutely no intentions of letting go of their own agendas, assumptions, and control in the interest of the work. On the surface there is cooperation, but there is fundamentally no notion of change. In fact, much effort is exerted to avoid the modifications necessary to truly work collaboratively.

In our earlier article we blithely danced around the reasons why the second type of collaboration is so liberating, not even acknowledging the first type. When we read our own work several years later, we were amazed and somewhat embarrassed that we had written the article without fully explicating the nature of power in either types of collaboration. In a way this narrative is our public recognition that the power dynamics in collaboration may be temporarily suppressed, but should never be completely denied. We want to be clear about those dynamics because we now believe they are central to the very possibility of true collaborative work.

More recently, we have collaborated with others in writing about the challenges of doing team-based research in specific projects (Garland, O'Connor, Wolfer, & Netting, forthcoming). As we continue to reflect about our team-based research experiences, and as we have had more of those experiences, we think our reflections might have something to offer. Our goal here is not to dissuade anyone from engaging in collaborative research, because when it is done well it can produce exciting, rewarding, and useful results. We think some of our observations may help ensure success when facing what can become tough times.

Our reflections about our research experiences with several local and national projects with differing funding sources and different research questions suggest to us similar themes of ethical dilemmas and research-rigor challenges that we wished someone had talked transparently about many years ago. What follows are a series of stories that capture our experiences and give rise to what we have learned. We tell the stories as composites in order to protect both the innocent and the not-so-innocent. None of our "collaborators" have been asked nor have agreed to participate in this reflection, so in order to honor their confidentiality, we wish to provide examples of consistent experiences across multiple projects.

Team Research Projects

Over the last twenty years, we have been involved in multiple funded research endeavors that have required teamwork. We brought these experiences to our collaboration when we started writing together and compared notes on what we had learned. The variety of experiences made us realize just how many different ways in which these research collaborations could occur. For example, one of us participated in a federally funded program evaluation that was funneled through state agencies for a collaborative of contractees who engaged in work that could not be done by state personnel because of ideological/political concerns. The researchers knew that all collaborators wanted certain program evaluation results, but none except the researchers had the necessary research skills to adequately judge either the quality of the research design or the results that were produced.

Our involvement has included a number of foundation-funded grants with interdisciplinary teams of researchers to evaluate projects across the country. One project involved collaboration across social work, public health, and veterinary medicine. More recent projects have joined social work with business, geriatric medicine, and nursing. Another involved a multitude of collaborators from various research institutions and disciplines ranging from sociology to business. We have been principle investigators, grantees, contractees, and sub-contractors. In many cases, facing the challenges of working across disciplinary cultures was compounded by the geographical spread of projects, all having their own cultural contexts. Between the two of us, as collaborators we have covered a lot of territory from child welfare to geriatric case management research. These experiences have provided us with the themes identified in our narrative.

It is important to point out that we have greatly benefited from our affiliations with these various projects and from our association with these funders. And we acknowledge that we are telling the story from an *outsider-insider* perspective, with all the accompanying biases this may imply. By *outsider-insider* we mean that we are the beneficiaries of research funds and are, therefore, part of an *inside* group of persons who are funded to implement projects. In that regard we are part of the grantee or contractee network. On the other hand, we are not part of the internal structure of the funding source and are *outside* the daily activities and decision making that occurs within its walls. This means we often receive information and feedback through filtered sources without having been privy to the complex decision-making process or the politics among foundation trustees, program officers, or others involved in funding decisions.

All this sets up interesting relationship dynamics and power and communication issues that are very much a part of our narrative.

Relationships with Funding Sources: Who Saves Face?

We suppose that trial by fire is a good way to describe the manner in which we learned about funded research with a team twist. It was enough to try to get grants alone, but writing them with others was a mixed blessing. We always said that "there's good news and bad news – the grant's funded and now we have to do what we said we'd do." Then a project would begin and it would lead in unforeseen directions, no matter how we tried to be control queens about it.

We quickly learned that no matter how much foundation officers say they are glad to leave the implementation to us, they never really truly mean it. We had to learn to think like program officers. Our first clue should have been when the program officers would literally rewrite our grant applications or tell us exactly what words to say so that it would get funded. We even had the experience once when a program officer contacted us, literally sought us out at a national conference, and said, "We need a site in your geographical area. If you write the grant, we'll fund it."

We should have known from those interactions that there was something going on rather than a kind of mentoring activity. But it took us awhile to realize not only that this was an issue of assuring good research, but that it was about the role of the program officer in selling an initiative to a board of trustees and then saving face at every opportunity when the project moved forward.

Our foundation grant proposal for a study of multiple dimensions of service delivery within private agency contexts was accepted for funding, but the project officer told us that she "had just a few ideas in order to strengthen the project." We had already secured the agreement to cooperate with the project from researchers and practitioners in four cities throughout the U.S. We had clinical and administrative social workers and faculty from four universities ready to begin the process when the program officer "suggested" that we also include another site, a non-profit research center that had had a long-term association with the foundation. "These people are great. They will add a great deal to the process. They have contacts in their part of the country that you do not have. It will just improve the coverage of the research," he said. We discussed between ourselves if we really wanted to add an unknown entity to the project, but we were also interested in assuring four years of research funding for ourselves and our doctoral assistants, so we agreed to make this adjustment and began the process of subcontracting with all the project participants. It was only later that we learned the politics of the situation. The site that was added to our project was not added out of a collaborative spirit; it was added because they had applied for funding and had not been selected. The new collaborators had been our competitors and someone on the foundation's board was intent on their having a stake, no matter how small, in the project we had designed. This was because the president of the university where our new collaborators were located had called one of the foundation's trustees and threatened to create problems if some compromise could not be reached.

We had learned early on that foundation officers were interesting folks, but it has taken us a long time to really understand their role in research, and even when we thought we did, we have been surprised. We were working on a project that took us to nine sites around the country. We were cast in the role of "evaluators" but we were told not to use the "E" word because it would scare the sites. Indeed, they were scared, but it wasn't because a team of academics was evaluating anything. They were scared because the program officer was apt to embarrass them in public. We witnessed one encounter in which a project director was being dressed down by the program officer for asking probing questions about the intent of the initiative. "If you alienate me, you alienate every foundation in the country. We talk to one another!" she hissed. "You'll never get funded again if you alienate me." As she turned on her heel to exit, the tension in the room was palpable.

Another situation arose when we were setting up another project that involved ten sites around the country, all in large health care systems. We were traveling to each location, talking with each project coordinator and their staff, and assisting in the preliminary stages of project development. The targeted population was frail elders and each site had to conduct multidimensional assessments in order to fully know how to intervene. As researchers, we immediately asked what tools they were selecting, only to find out that they were choosing different ones and that a couple of sites were developing their own. Tickled to have some expertise that might actually be useful, we chatted with the program officer about the importance of using standardized tools. There were a number from which to choose and it made sense to have the sites decide which one would best meet their needs. If the same data were collected at each site, then obviously we'd have comparable data, make our biostatistician's life much easier, and be able to really evaluate what was happening. We waited. When no one moved to proclaim the selected tool, we persisted.

With the program officer's support, we found a leading geriatric instrumentation guru, invited this person to our first joint site meeting, and focused on the importance of assessment across sites. We were certain that once the presentation had been heard, and with the encouragement of the funding source, all sites would move to select a standard tool to use across sites. We hosted a small conference with three or four key staff from each of the ten sites coming together for a two-day event. Everyone came and the event went brilliantly, as the presenter did a fabulous job. Staff across sites began chatting with one another about how they could coordinate their efforts. Networking had begun! Imagine our chagrin when the program officer got up at the end and said, "This was a great opportunity to get together and we hope you'll find tools that are just right for your site. Perhaps some of the ones we've discussed in the last two days will be helpful, but if not, feel free to be innovative and develop your own."

We were dumbfounded. For months we kept asking ourselves "Did we not make ourselves clear?" "What about standardization did she not understand?" "What are we missing?" One evening in a bar, following the next get-together of the projects, we confided in one of the site administrators with whom we had a long-term relationship. "Why couldn't we get our standard tool message across?" we asked. He said almost too matter-of-factly, "Oh, don't you understand? If they use the same tool, then what if nothing works? If the sites all use something different, some will work and others won't. The funding source will highlight the ones that did well and simply not mention the others. You can't do that if the whole initiative stands on one dataset. This way, something is bound to work!"

A similar episode occurred in a project that had fewer sites and was dealing with a different set of issues. We discovered as we participated in the research that one site was having real difficulties. The professionals who were part of the treatment and comparison groups were becoming intermixed as the system in which they were located reorganized. In short, the treatment group was seeing professionals in the comparison group and vice versa. The integrity of the project's design was not only at risk, but it was like fruit basket turnover. We debated for a couple of days about what to do. It was nobody's fault; it was just the craziness of being in a system that was in turmoil and in doing "real world" research. After a long dialogue, we finally concluded that we had to call the funding source and tell them that the project was in jeopardy. We fully expected that they would terminate the contract and we hated to be the whistle blowers, but ethically we had to say something because it was clearly impossible to do the research.

We remember making the call as if it was yesterday. This was a huge grant and ending it would have an impact not only on us as researchers, but on all the programs that were receiving overhead money from the grant. Sitting in a conference room with a speaker phone in front of us, we laid out the problems we had discovered in great detail. The program officer carefully listened, then said he would make a special trip to the site, instructing us to meet him there the following week. The key stakeholders gathered, everyone thinking the worst, but ready to face the realities. No one could argue that the grant as written could not be implemented. The program officer talked in vague terms for a while, then said, "I know it will be hard, but I expect you [looking at us] to work with everyone here to figure out how to make this right. We're going to declare victory regardless of the results!" It was then that we realized that the program officer's reputation was on the line and that he had convinced a board of trustees to fund this multi-site, multi-million dollar project. It was not about us; it was not about the site; it was about that program officer's reputation. Our role was to figure out a way to redesign the project and cobble it together in the interest of saving face, not doing good science.

Grantees tend to believe that funders know what they want when a research contract is developed. More likely they know what they do not want when they see it. Assuming that there is a pre-ordained plan about how the research should be enacted can be troublesome in the long run because the only workable rule of thumb is that program officers tend to react negatively when things don't seem "right." The other rule of thumb is that grantees will try to figure out what is "right," but the program officer really doesn't know until he or she sees it. The result is that grantees (and their contractees) feel manipulated at best and jerked around at worst. Actually, this lack of clarity does not appear to us to be an intentional play for control; it is just the way the process seems to unfold. However, we have seen power plays in other contexts with other members of our teams.

Team Relationship Dynamics: Who Has Power?

Clarity in communication and ground rules about process are necessary in collaborative research, but one can not be naive about the power dynamics that are interwoven into the communication and ground rule structure. There is formalized decision making, and then there is informal relationship development – both are important. When the same terms are used, there will be different definitions and understandings. Even when there are rules for engagement in data collection and analysis, team members or subgroups will do it differently. We've found that the trick is to allow for individual idiosyncrasies while protecting rigor, especially in qualitative research.

In one project focusing on program efficiency and effectiveness, we were the only researchers who were familiar with both the practice contexts and the mixed research methods that were to be used. Based on negotiations with the funding source, several additional researchers representing business and advanced statistical analysis were added. These "new" participants joined the team after the research design had been determined, but wished to add their expertise. During the five years of the project, those representing business continued to expect that the project would assess programs using standard for-profit business practices and that we would always do what they said was necessary. When we tried to explain that non-profit organizations and management styles were probably different and required a more open assessment strategy, we heard them grumble to themselves, "What do social workers know about running an organization?" We chose not to address that, but we were forced to directly discuss the qualitative/quantitative design conflicts that also arose because we knew for certain that none of them had ever engaged in an interview, let alone a fully developed qualitative project. They did not trust us and we never fully trusted them. We thought that we would be able to handle all the qualitative parts of the project, but those who had never engaged in this type of research "wanted to learn from us." They did not trust that qualitative research would produce anything usable, so they wanted to be involved when it came to making decisions about the qualitative aspects of the work. The quantitative researchers kept saying, "We want to do it all," meanwhile resisting our advice at every turn. We double checked their data in very subtle ways at each stage of the process just to make sure that both their collection of information and analysis of the data were consistent with our expectations. When it came to the quantitative part, they went off on their own, never once asking for our participation or advice.

Most of the time we have really liked the people we have worked with in teams; however, in one project, it was clear that certain team members (mostly men) neither liked nor respected us. The whole situation became clear over conflicts related to what constituted rigorous qualitative research. In meeting after meeting, decisions would be needed about the appropriate next step. The quantitatively inclined team members would argue that the best way to proceed would be based on what they knew to be true based on their reading of their discipline's literature. We, on the other hand, because of the emergent design of the project, continually argued in favor of letting the data speak for themselves. Generally, about five minutes into the discussion, it would get heated, with their side making broad statements about what was necessary to guarantee the quality of the project. Our response always took us back to the research proposal and our understanding about what was necessary to comply with the proposal. Soon it felt like we were just talking to ourselves because small subgroups would begin chatting or others would start looking at "important" documents. Finally, one of the business people would say "Okay, okay, these are human service agencies. Do what you want." After months of these kinds of experiences, the breaks, social hours, and meal times became more and more uncomfortable. We really didn't like each other very much.

Just because a funding source brings researchers together in a joint venture, leveraging into a team may not work. When really disparate parties are brought together, the parties may never coalesce into a bonded team moving in the same direction, no matter what the incentives. Bonding exercises are necessary and appropriate, but they may or may not work for team development. If deep underlying assumptions are in conflict, no amount of meetings in nice places, meals together, chances to conference, drives in beautiful settings, agreements made face to face, or formal contracts will control individual researcher behavior vis-à-vis the team. This means it is also important to try to maintain an ability to make choices about who will be teamed with you and what you can do to protect yourself or the project when the team is not working and the project is in jeopardy.

Teamwork can be difficult, tedious, and time consuming. It can also be a whale of a good time. A team is not an efficient mechanism for getting the work done, even when the research project is huge. However, the quality of the work will be better, if the team is able to work together. It is important to realize that getting to teamwork requires time and it usually takes longer than anticipated. With the challenges we have had, we also have had an incredible team experience of such synchronicity that we would end one another's sentences. One person would begin writing a report or an article and hand it off to another person, who would pass it to another team member. As it moved through the team, we totally lost touch with who had written what. In the end we were all certain that the product could not have been as good had any one of us done it alone. When this happens there truly is an incredible sense of trust in the power of the team.

Trust is difficult to achieve across geography where researchers return to their "home environments" without the need to face their team members every day. In another project that had multiple sites around the country, a whole substratum of research associates and project managers worked for the faculty members who were the PI contractees at their respective universities. It was a complicated structure. Each site had a handful of research associates and a manager who did the daily work of the projects. These folks were in constant communication, keeping email buzzing about the day-to-day activities. What we hadn't fully realized was that they were becoming totally connected and we, as the PI's at one site, were actually dependent upon our associates for knowing what was "really" going on. This situation came perilously close to disaster when one of the associates learned information about faculty members at another site and the negative things they had said about one another. Soon we were privy to information that we didn't want to know, much less should not have known, but we had to admit somewhat sheepishly that it did explain a lot of the dynamics we were picking up on in phone conversations and face-to-face meetings.

There was also an undercurrent of persons "in the know" and persons "out of the loop." The substrata had built trust among themselves. It was as if a secret society had been built among the research associates who learned how to work together under the radar screen of the faculty team members who were not getting along. We as researchers realized that our associates had managed to cross ideological borders that we could not cross and there was a temptation to use them as informants. It is in these border crossings that a network of relationships across these troubled teams grew and upon which real teamwork was being built. We also realized, rather fearfully, that information exchange was the glue that held this secret society together. And if we were privy to dynamics within other sites, perhaps our own dynamics and internal politics were leaking out to others!

Unfortunately, in this situation the connections were not being built among the faculty researchers at different universities. When we tried to build those bridges, we encountered tremendous resistance as the project culture had already been built around lack of trust. One particular problem was a contractee who had had funding from this source before, and she was always bringing up her relationship with the funding source. This was particularly painful for the current PI who was actually at the grantee site and who had contracted with this woman. When meetings got difficult and stalemates arose, the contractee would pull out the "I know the funder card" and say things like, "Well, when I ran into the program officer in Philadelphia at a meeting last month, she mentioned something entirely different. But perhaps she's changed her mind. I certainly trust that you know better than I." The power dynamics inherent in pulling this card were stifling and would bring communication to a standstill. We imagined that the PI would have liked to have said to this person, "Get a life! It's totally inappropriate for you to be talking with our funder. That's my role." But that never happened. Instead we continued to bicker while the secret society of research associates, mostly graduate students, would kick one another under the table as they watched the faculty carry on.

In every team there is an archivist who reminds the group where they have been and in what context they are operating. Because they hold the details, they hold great power in the team. They may engage in historical revisionism to support their own perspective, especially when all discussion has not been recorded. They may play a pivotal role in the ability of a project's members to coalesce into a viable team in the way they shape the participants' understanding of process and decisions. No archivist is a neutral scribe.

In one collaboration, we had an experience in which there were multiple archivists, all with their own version of what had been said and their own interpretation of what the notes of each meeting "really" said.

We would attend a meeting of the various sites, leave the meeting feeling like progress had been made, and then be sideswiped time and again when everything happened literally the opposite of what we thought we had experienced. We began to question our own sanity. When we would get notes from the meeting, we would wonder if we had even been at the same meeting. It was obvious that what we had to say was either so controversial, or seen as so off track by others, that people would go to great lengths to change the course of history. Sometimes, when the revision negatively impacted the research design we would ask for clarifications, but most of the time we would just let the social construction stand.

A good sense of who you are as a professional and a good dose of honest self-awareness are critical to a team project. You must first trust yourself. Then, even if it takes work, there must be some level of trust, along with a high tolerance for different perspectives in the team. Respect for the project's leadership, experts, participants, and processes help. Trusting the team means that not everyone on the project must participate in all aspects of the project to have a functioning team with great teamwork. Tolerance and respect for both process and method will be needed. It is only when tolerance and respect exist that you come to realize how detrimental to the research process their absence is.

Dissemination: Who Gets Credit?

Finally, and most interestingly, when it comes to results dissemination, even those persons who did not think the data were worth pursuing seem to develop a curious desire to control what is written. No matter how elaborate the publication plan, there will be assumptions made that subgroups are taking advantage of everyone else. No matter how clear the agreement about targets for publications or presentations, no matter what level of screening is put in place, someone on the team will not be satisfied. Some members will expect more help in conceptualizing publications and presentations; others will resent team oversight

In one project several years ago, the funding source wanted us to edit a book in which the results of each of the project sites was published. It sounded like a good idea, but we ran into some very strange reactions. Because everyone's job status was based on publishing, faculty members wanted to get an edited book chapter done, but they wanted refereed journal articles more than books because articles "counted" more in their university settings. They wanted to write chapters for the book, but were unwilling to share any of their results, saving the data for journal articles. The book became a description of each project, without anyone really knowing what had been found as a result of the research.

On another project we developed very detailed guidelines for what could be published, who had access to the data, who would qualify as an author, and where publication would occur. We had some specific interests and expertise and others had the same. This was a huge project with data available from many dimensions and of interest to many disciplines. We proposed to write several articles that had either content or method specificity. We assumed that, because there had been so many challenges to the qualitative aspects of the project, most of our team members would be uninterested in the parts of the project that we found most fascinating. We followed all the guidelines about publication and dissemination; but we were surprised to see that when we began the dissemination process, both for conference abstracts and articles, all sorts of consternation arose. Several untenured faculty felt that we "owed" them the opportunity to be published with us though they had not participated in the conceptualization or construction of the particular work. Others felt we were "hogging" the data and that they should get an opportunity to write from it. We still are not certain about the meaning of all the confusion since no one was jumping forward to write anything but us. We did include several of the other team members in various efforts, but we now look with sadness on a huge amount of data that was never mined to its depth after we moved on to other projects. It was a major lost opportunity for the project and other members of the research team.

In order to make a difference, it is important not to lose sight of the purpose of the research when dissemination time comes. It is essential to get your findings out to others, not to get stuck with internal dynamics about who writes what. If you can maintain the focus on the reason for the research, you will be able to move forward in the creative process of dissemination. We have learned that regardless of our level of participation in a project we can help to shape the discussion about what was learned because many times others in the projects are not comfortable carving out how to share information. In service of the findings, we have even shaped articles and presentations for others. This is not to suggest that we are willing to do the work for others, but we are willing to get ideas started without the need to be identified as co-authors, especially when we think findings can really contribute to increased quality of practice. It is important to know when you are choosing to do that as opposed to being manipulated into doing the work of others.

Conclusion: Achieving Collaboration Rather Than a Cold War

Even with the best intentions and care in enacting a collaborative process among researchers, some challenges will always exist because much of what the researcher does remains outside his or her control. We offer the following musings as words to the wise for those considering engaging in team-based research. We think they have potential for creating productive collaborations rather than cold wars.

Even when there is a cold war, there are great opportunities to learn from the warring parties. Any researcher will leave a difficult project experience as a fuller, more sophisticated research practitioner when consciousness about roles, needs, and processes are critically analyzed away from the heat of the situation. We do not typically like to use war metaphors to describe team experiences, but we intentionally use "cold war" now. The reason is that war is about power and we want to be clear here – power is something we may have glossed over in our earlier, rather naive meanderings about collaboration, but we have no intentions of doing that again. There are substantial power dynamics at play in team-based research, dynamics that one should never ignore or flee from. Recognizing and managing the power dynamics that accrue due to gender, discipline, university status, research design, and methods will always be needed in team-based research.

As we reflect on the narrative of our experiences, we are somewhat surprised to recognize how negative we may sound at times about our history with the collaboration process. We suspect that like some of our collaborative colleagues, our feelings may have gotten hurt along the way, and sometimes that hurt was intentional. But most of the time we believe it was not intentional at all. Collaboration is what happens when people bring their passion, their identities, their reputations, and their disciplinary cultures together. It is inevitable that there are strong feelings and perceptions when that happens. Clashes will happen. In fact, clashes should happen in order to achieve a better state due to the synergy of group. When collaboration works, the process and the product are much better than what could be achieved alone.

Similarly, we all bring our identities to the process and we stake our reputations on what happens. Funding sources stake their reputations too. Therefore, it is hard at times to meet everyone's needs in the collaboration. But at other times, the creative energy that can occur when everyone joins their passion for the project into a truly collaborative venture is worth repeating again and again and again. We hope some of our ruminations will serve to be useful when confronting similar situations, just as they have been useful to us when we continue to agree to participate in team projects. As for our current collaboration, we literally describe it as "play" when our minds meet and the ideas flow. That keeps us going through the tough times. That makes it all worthwhile.

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Working for Two Communities: A Community-Based Researcher in a Research-Intensive School of Social Work

David Crampton, Ph.D., Case Western Reserve University

This narrative describes the experience of a community-based researcher working as a junior faculty member in a research-intensive school of social work. The goal of the author is to offer reflections that he hopes will be useful for other community-based researchers who are considering an academic career while maintaining their passion for doing work "on the ground."

Graduate students who are pursuing a Ph.D. and a faculty position in the field of social work are in an enviable position. Unlike our colleagues in the humanities, there is an insufficient supply of new Ph.D. graduates relative to the demand for new social work faculty members. It is truly scary to read the first-person essays written by faculty job seekers in the *Chronicle of Higher Education* who finished their Ph.D. in other disciplines but now cannot fulfill their dreams of a tenure-track job. In painful detail, they describe applying for a faculty position announcement that receives hundreds of applications, having to land one or two brisk conference interviews at their annual discipline meeting. My experience as a social work doctoral student was very different. Even a few years before I finished my Ph.D., I asked social work faculty to meet with me to discuss a potential application from me, and they cheerfully accepted.

Being in a sellers' market still includes challenges. What sort of school do you want to join? The typical typology is to distinguish between teaching-intensive schools (which I describe as schools that expect pre-tenure faculty to teach more than four courses per academic year) or research-intensive schools (which I describe as schools that expect pre-tenure faculty to submit more than three manuscripts per academic year). One of my graduate school mentors told me to look into my soul and decide if I wanted primarily to be a teacher or a researcher and then to apply to schools based on my choice. Most importantly, he said, avoid schools that try to be both. By the way, I discovered that if you are interviewing with a school that is trying to be both, don't explain this typology to them as they will adamantly insist it is possible to be both, and they won't hire you unless you agree to be both.

This brings me to the topic of this essay. Suppose you are the sort of social work researcher who does the work described in this special issue of *Reflections*. Which type of school will best support your work? I think it is a very difficult choice. I expect that most people reading these essays love to teach and to do research. The passion that drives us to change practice on the ground inspires both community work and teaching. Which direction should we turn? I have thought about this issue a lot and have started living through one approach, but I won't claim to have definitive answers. In this essay, I will describe how I first became interested in doing research on the ground, how I pursued this work in graduate school, how I looked for an academic position that would support this work, and how I am managing doing community-based research as a faculty member in a research-intensive school of social work. At the conclusion of this essay, I will offer some "tips" I have gleaned from my experiences.

Developing a Passion for Research on the Ground

I came to community-based research accidentally. I started out working in electoral politics, helping candidates get elected to the U.S. Senate. Eventually, I found myself on Capitol Hill, advising a new Senator on a wide variety of policies. My primary qualification for this job was that I had helped the Senator get elected. I could tell you who was on what side of any debate back in our home state of Nebraska, but substantive policy distinctions were usually outside my expertise. Hoping that better public policy would result from knowing more than just what the interest groups wanted, I decided to go to graduate school to study policy analysis. After a lot of training in quantitative methods, I finished my master's degree in public policy and got a job in a research center that studied child welfare services.

At the center, we had a database with the placement records for all children in foster care in Michigan. We used the data to create fairly sophisticated models of topics like length of stay in foster care. I suppose I understood at some level that the numbers in my computer represented the experiences of children, but I knew very little about children or foster care. I read the codebook, played with the data, and tried to intuitively build models that made sense.

Part of the center's work involved collaboration with state child welfare officials. At some point, our relationship soured, when the child welfare officials felt as if we were just trying to make them look bad and we thought they were unwilling to critically examine their service delivery. I tried to use my old political skills to mediate between the two and after several months we developed a model of foster care length of stay that both sides supported. I then received a letter from a child welfare official that said:

> I circulated a copy of your revised paper regarding permanency planning. As noted in our phone conversation, we were very pleased with your efforts to address our concerns. The new model has a logic to it that we find persuasive. We are more inclined to believe the results because the data seems more logical. .... **We are not sure how to use it in our policy making strategies. Any suggestions would be appreciated** (emphasis added).

Despite the friendly tone of the letter, I was devastated. A year's worth of work had produced a model that was "logical" but without practical value. At that moment, I realized I wanted to do a completely different kind of research. I wanted to do research that people would use. I decided to go back to school to get my M.S.W. and Ph.D. I planned to read everything I could about child welfare, work directly with children and families, and learn how to do research that could improve services on the ground.

Learning How to Be a Graduate Student Doing Research On The Ground

I went to graduate school at a research-intensive school that places a premium on federally funded research. Many Ph.D. students there work in federally funded research centers and write their dissertation using data generated by the centers. It is a very good model for training faculty for research-intensive schools of social work. I chose a different path. Another graduate student and I essentially hung out our shingle as community-based program evaluators. We were fortunate to receive several contracts to do participatory program evaluation in a small urban community that was a two-and-a-half hour drive from campus. After so many trips there, we could now probably drive that route with our eyes closed.

It was a wonderful experience. For seven years, we came to know everyone in that community who worked in or with their child welfare system. They gave us whatever data we wanted. We analyzed the data on campus, presented our findings throughout the community, and re-worked it again and again. On those long car rides, we vigorously debated what it all meant. The community sincerely wanted to know how to improve their work, and we did our best to help them. We even found just enough sympathetic faculty members in our home university to put together dissertation committees and write dissertations based on our research.

After fulfilling my dream to do research that was more relevant than elegant, I also experienced the dark side of policy relevant research. We were evaluating a program that sought to reduce the over-representation of children of color in foster care by using Family Group Decision Making (FGDM) to divert children from foster care into informal kinship care. The focus of FGDM is convening a meeting of the child's extended family in order to make a plan for the child's care and protection in cases of child abuse and neglect. One day, I did a simple calculation that showed how the program reduced admissions of children of color into foster care by 23 percent. I wrote up a brief memo explaining how I did the calculations but also carefully establishing some context and caution for the findings. I reminded my community partners that diverting children was not enough; we also wanted children to be better off. The goal is family empowerment, not saving money. But before I knew it that 23 percent figure was bouncing all over the state and I heard was even discussed in Washington, D.C. (probably shared with naïve Capitol Hill staffers who had no idea what it meant).

The Governor's budget included a proposal to spend 6 million TANF dollars piloting FGDM in Michigan. Why? Because it can empower families? No. The rationale listed in the Governor's budget included only the information that a pilot program had reduced foster care by 23 percent. I learned from that experience that community-based research can have great impact, but not always in the ways we intend. To make sure research helps improve practice, we have to spend a lot of time with our community partners to help them articulate what they hope to accomplish, how their program can accomplish those things, and how we will demonstrate their success. We also have to consider how research results might be used within larger political and social contexts.

As much as I enjoyed doing community-based research as part of my graduate studies, it wasn't all easy. Many of my professors noted that I had little patience for knowledge for knowledge's sake, which was true. Always thinking about how a new theory or method might improve my community work helped me understand many of the things I learned, but I also missed many valuable lessons in graduate school because I did not see an immediate value to them. Part of the problem was working so much while going to school; I had little time to think big thoughts.

The most stressful thing was the money. We received very little financial support from the university. Every semester we had to pay for our tuition and salaries through our community-based research or by teaching an M.S.W. course. We also hired a part-time research assistant who understandably wanted to know if her job was going to end any time soon. We wrote a lot of proposals that were rejected, but just enough were awarded to keep us afloat. Over a seven year period, we raised over half a million dollars, but it was never easy or certain. We spent a lot of time pouring over spreadsheets-not the ones with program data but the ones we used to track our expenses and grants. In the end, we managed to pay our bills, get our degrees, and, I think, help improve services for children and their families.

As great as this experience was, I could not see how to replicate it in the next stage of my career. We benefited from a unique convergence of a great research team that worked well together, a community that wanted our help, and the resources to do the work. As our grants ran out, it was time to find something else. My research partner chose a full-time research position at a community-based agency. She spends her days helping the staff improve their practice, but she can teach only an occasional course as an adjunct. After reading this essay, she reminded me that agency-based work is no bed of roses. She has to deal with the bureaucracy, the funding needs, and all the other things that go into working for an agency. I suppose I avoided the agency route because I was tired of always worrying about money (of course, academics do a lot of grant writing as well). I decided to try an academic career, and that presented a new set of challenges.

Learning How to Obtain an Academic Position Doing Research On The Ground

I went on the faculty job market and considered a wide range of schools. I applied to the teaching-intensive schools, the research-intensive schools, and those that wanted to be both. I even applied to a well-known school of public administration. That school invited me to a campus interview. After a full day of interviews and my job talk, during which I was peppered with questions designed to throw me off my feet, the search committee chair informed me that I was not a good fit for the school. After my trip to campus, the faculty realized that I wanted to do community work and at their school, untenured faculty are not supposed to do community work. I agreed that was a terrible fit for me, but I learned from that mistake. During the rest of my job search, I asked every school about community service and also asked to meet with community members as part of every campus interview (if the school can't find a community member willing to recruit new faculty, that is a bad sign).

I spent a considerable amount of time researching the schools that I applied to so I could write persuasive cover letters and so I could make sure they were a potential fit for me. I did a lot of informal interviewing at conferences before I signed up for those exhausting campus visits. For example, in a conference interview I met with a small group of faculty with excellent ties to their community and who were excited about my work, but upon hearing about their experiences with high teaching loads and service demands, I realized I would probably not have time to write for publication. Some schools were annoyed by my questions about teaching "loads" and some were reluctant to name out loud how many publications they expected new faculty to have in order to gain tenure, but I found that these crude numbers helped me gauge how much support I would receive as a new faculty member. The elusive fit between the school and the new faculty member is very important for both parties, and straightforward questions with open communication are the only ways to determine if the fit is right.

In the end, I chose to accept a position with a research-intensive school that has excellent ties to the community. As part of my campus interview, I met with a public, child-welfare, deputy director, and we mapped out a specific research agenda based on a match between my expertise and their community needs. Using that blueprint, I wrote to the Dean and told him exactly what research I would do if they hired me. They did hire me, and I can honestly say that three years later, I am following through with that work.

Although this community work is valued at my school, a strong publication record is the key to tenure and promotion. Through writing this *Reflections* essay, I can now see that I have two community partners: the community I try to help through my child welfare research; and the academic community that hired me with tenure expectations of grants and publishing. The challenge is to meet the demands of both communities, which I will address next.

Learning How to Live and Work in The Academy While Doing Research On The Ground

In my current job, I have two community partners: the community that hired me; and the community I help with my research. The local public child welfare agency will take as much of my time as they can get. I am on three standing committees, and whenever they start a taskforce, I know I will receive an invitation to join. My other community, the one that pays my salary, wants me in my office writing articles for publication. Of course, there are only so many hours in a day. As my planner fills up with child welfare meetings, I find fewer blocks of time to write. One well-meaning colleague suggested that I just stop going downtown to the public child welfare agency for a while. Although the public child welfare folks would be jealous if I did that, I admit they would understand. The real barrier to isolating myself like that is I would feel lost. The reason I do this work is to change practice on the ground. Take away those long frustrating meetings in which we try to improve a big social service bureaucracy, and I would forget why I do what I do.

My initial concern was how I was going to balance learning my way around my new child welfare community with writing articles and grants. Fortunately, my school community has been generous in giving me time to do both. My real challenge is to write articles that are accepted and grants that are funded. In three years, I have submitted a dozen manuscripts, but only four have been accepted. I have written six grants, but only two small university-sponsored grants have been funded. I have also presented over a dozen conference papers and posters, but those are not so important to my promotion. At the rate I am going, I may not be able to keep my job unless I learn how to negotiate yet another "community," the world of peer-reviewed journals.

At first, my window into the peer-review world was a stream of rejection letters. My impression was that article reviewers don't like things they don't recognize, and many of them think community-based work is messy and foreign. Reading rejections of your work is painful, but it gets easier over time. I later decided to become a reviewer myself, which certainly has helped me learn the process. One journal I review for sends the reviewers a copy of all the reviews for the article they read. It is amazing to see how uneven and incongruous a set of reviews can be. Of course, that is what we all think when they are rejecting one of our own articles, but when it is someone else's article you can really see how random the process is. On the other hand, journal editors are sympathetic to our 'publish or perish' existence and are often willing to offer some advice on where to send manuscripts and how to pitch them.

Striving to Succeed in the Academy as a Community-based Researcher

I just had a major performance review with the not surprising outcome that I need to "publish, publish, and publish," as one colleague succinctly put it. If I want journal editors to publish my work, I have to understand their constraints and rewards while I remain true to my calling as a community-based researcher. My child-welfare partners always want data they can use, but I have to challenge them to think of how to obtain data that will persuade the newspaper reporter or the taxpayer that a program works. On the other hand, research journals want to publish research that is rigorous, but, for me, it must also be relevant to social work practice. What I find helps me in my own research is a constant feedback loop of sharing my results with practitioners and getting their input and then sharing it with fellow researchers and getting their input. This is why I think all community-based researchers can benefit from publishing our work .My community work is stronger when I subject it to peer review. All of our work improves when we share our experiences through publication. A strong publication record earns the respect of our peers and, hopefully, helps us secure grants and tenure.

But how do I "publish, publish, publish" – with a ticking tenure clock – when my work is often not understood by the editors of academic journals and those who review for them? Here are a few things I have tried.

First, write about methods. While sample sizes and research designs in community work are often not readily accepted by research journals, community-based researchers can sometimes write a publishable manuscript about the methods developed for a particular study. For example, much of my work concerns Family Group Decision Making (FGDM). In several articles, I describe how I help community members articulate why they want to try FGDM and how they will evaluate their efforts. Because FGDM is focused on forming partnerships with all the people who are concerned about a child's welfare, I demonstrate how FGDM evaluation and research should similarly engage all stakeholders in designing the evaluation, interpreting the results, and making changes to the program based on the research findings.

Second, know the literature. Generalizability is usually a concern with community-based work, but an extensive knowledge of the literature in a specific substantive area helps community-based researchers draw out interesting points related to differences between the conventional wisdom and findings drawn from community-based research. For example, a lot of the research on kinship care compares relatives and foster parents as caregivers and then tends to highlight the deficiencies of relative caregivers. However, focusing on this comparison misses information gained by studying FGDM as an alternative way of working with relatives. Kinship care providers' concerns, needs, and strengths are better understood when social workers use FGDM to work with relatives rather than assume that relative caregivers should behave like foster parents.

Third, do large data set analysis with local implications. Given my love of community work, I have been reluctant to follow the path of downloading a secondary data set, regressing A on B, and cranking out a quick article. I have discovered an alternative that I like. I download secondary data that is drawn from my community, analyze it, and then ask my community partners to help me interpret the results. I can then write articles based on a large sample, but the implications of the work have more meaning to me. One database that is helpful to me links information from the birth certificates of all children born in the county to the children's contact with social services. My colleagues and I have used this database and life table analysis to show that 49 percent of all African-American children in our community will be investigated for child maltreatment some time between their birth and their tenth birthday. I have presented these data in community forums throughout the county and in an op-ed I wrote for the main newspaper in order to increase awareness about the over-representation of African American children in the child welfare system. Now I can write an article about the value of using life table analysis in community advocacy work that describes both the statistical methods and community-organizing methods.

Finally, collaborate with community and faculty colleagues. If community-based researchers have picked their institutional home carefully, faculty colleagues should be supportive of community-based work. Writing with supportive colleagues should help community-based researchers get more published. For example, I am writing a literature review on the relationship between neighborhood characteristics and child maltreatment with two of my senior colleagues who are experts on this topic. Similarly, community-based researchers can write with their community partners. When I am honest about my publish-or-perish existence, I find that my community colleagues are more than willing to review a manuscript for me or even participate in writing it. I recently wrote an article about how principles of social work with groups can apply to FGDM practice. I asked several practitioners to review the manuscript for me and help me clarify when group work applies and when it does not. As a result, my practitioner colleagues had to think critically about why they do, what they do and the practical relevance of my academic work improved.

Coda

As I reflect back on my journey, there are at least three major challenges to doing research on the ground: finding an institutional home that supports this work; balancing the demands of my institutional home with the needs of my community research partners; and publishing community-based research. A response to these challenges begins with recognizing that finding a career that supports community-based research is not unlike doing community-based research itself. Just as I would never jump into a community and start researching without talking with community members about their needs, I should not accept a job with an agency or university without first understanding what they want from me. When I am doing research on the ground, I have to understand the different needs of different stakeholders and figure out how to produce research that will be useful to the whole community. In the same way, I have to learn how to reconcile the needs of my employer with those of the community members we are helping. Successful community-based research requires favorable reception of my work by the community, which means I have to understand their needs. In order to get my work published, I have to appreciate the needs of journal editors. My ultimate goal is to understand and to meet the needs of these various constituencies while remaining true to my calling.

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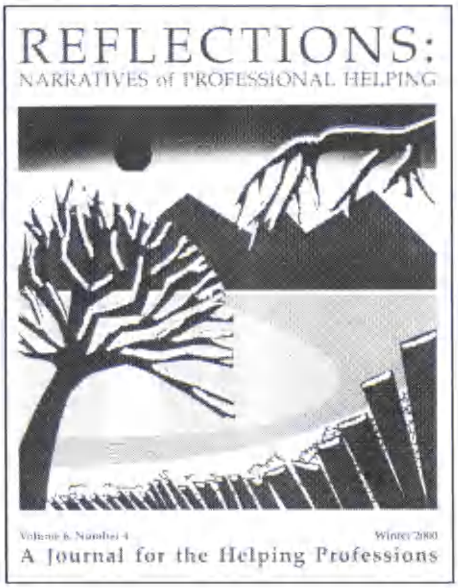

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