

Commentary on *Logical Operations in Theory-Building Case Studies* by William Stiles

**Lurching Toward Theory:
The Case of Case Study Research in Conflict Mediation**

KENNETH KRESSEL^{a, b}

^a Department of Psychology, Rutgers University, Campus at Newark, NJ

^b Correspondence concerning this article should be addressed to Kenneth Kressel, Department of Psychology, Smith Hall, Rutgers University, Newark, New Jersey 07102
E-mail: kkressel@psychology.rutgers.edu

ABSTRACT

William Stiles (2009) makes a cogent argument for the value of case study methods for building practice relevant theory in psychotherapy. My use of case study methods in the domain of conflict mediation, an area of professional practice in which more traditional empirical methods have largely failed to answer questions of most interest to practitioners, well illustrates many of Stiles' points. Although mediation has more delimited objectives than psychotherapy and is practiced by many individuals with little if any mental health training, its practitioners face the same challenge of exerting influence in situations of considerable tension, ambivalence, and unpredictability. Stiles' argument for the case study as a theory building device resonates most strongly with my own experience in three major ways: (1) The value of the case study for connecting theory to practice; (2) the capacity of the case study to capitalize on problematic or unique experiences; and (3) the ability of case studies to generate the kind of rich observations that allow theories to be productively modified and adapted. I illustrate these points by describing my gradual movement towards a theory of expert mediation practice in a series of case study investigations in three diverse domains: divorce mediation in a family court; the management of conflict in a university medical center; and the mediation of conflict among scientists by ombudsmen- mediators at The National Institutes of Health.

Key words: conflict mediation; case study methods; theory of expert practice

William Stiles (2009) makes a cogent argument for the value of case study methods for building practice relevant theory in psychotherapy. Dan Fishman, the editor of this journal, asked me to comment on Stiles' paper because, in Fishman's view, my use of the case study illustrates some of Stiles' major themes. This is a provocative (and flattering) invitation because my research area is not psychotherapy but the study of conflict mediation. In addition, unlike Stiles, my use of the case study has been less a carefully planned research strategy and more an improvisational activity with a decidedly pragmatic bent. At first I was simply interested in developing a mediation approach that would be effective in a particular time and place (the family court in Essex County, New Jersey); later, the work was shaped by conceptual concerns, but the primary emphasis was in helping a team of expert ombudsmen-mediators at the National

Institutes of Health. In short, I have worked less in the spirit of the researcher looking to systematically build theory and more in the spirit Mark Twain had in mind when he remarked, “by picking up a cat by the tail you learn things you can learn in no other way.”

Despite the “cat grabbing” spirit of my case study research, it well-illustrates many of Stiles’ arguments in favor of case study methods. In the commentary that follows I will illustrate the correspondence by tracing my gradual movement towards a theoretical perspective on expert performance in mediation. The confirmatory value of Stiles’ paper is heightened for me because the field of conflict mediation, because of its relative youth and multidisciplinary nature, is very much in need of unifying theoretical perspectives.

To understand the fit between Stiles’ argument and my own experience it will be helpful to describe the history of mediation as an area of professional practice and the research that grew up around it.

CONFLICT MEDIATION AS AN AREA OF PROFESSIONAL PRACTICE AND RESEARCH

Mediation is a process in which disputants attempt to resolve their differences with the assistance of an acceptable third party. The mediator’s objectives are typically to facilitate a search for mutually acceptable solutions to the conflict and to counter tendencies toward competitive strategies and objectives. Although mediation has more delimited objectives than psychotherapy and is practiced by many individuals with little if any mental health training, its practitioners face the same challenge of exerting influence in situations of considerable tension, ambivalence, and unpredictability.

Because I am both a social psychologist and a clinician, I was drawn to mediation as an area of scholarship. When I began my research career as a social psychologist in the 1970s, conflict mediation as a professional activity had begun to explode beyond the boundaries of labor-management conflict where it had originated 40 years earlier. The civil rights struggle, the women’s liberation movement, and the protests against the Vietnam War made social conflict and its “resolution” a subject of major national concern. Mediators of every stripe and sponsored at every level of government began appearing to assist disputing individuals and groups resolve their differences “constructively”.

In those early days, practice far outstripped theory. A movement, more than a profession was underway. The growing cadre of divorce, neighborhood, and environmental mediators who began plying their new trade drew for theoretical justification (if they drew on anything at all) on Morton Deutsch’s influential work in social psychology on constructive vs. destructive conflict (Deutsch, 1973). Deutsch’s key insight was that in constructive conflict resolution, motivation was key: if the parties defined, or could be helped to define, their dispute as a search for mutually satisfying outcomes the results would be solutions to the conflict which were more creative, more durable, and accompanied by greater feelings of mutual regard compared to conflicts in which the parties defined the goal as winning a competitive victory. Deutsch and others provided considerable evidence that this theoretical proposition was true, but much of it came from laboratory studies using the proverbial college sophomore and made use of gaming simulations

(Rubin & Brown, 1975). Research in real-world settings was an occasional afterthought and the theory provided little guidance to the practitioner.

Of course, practitioners did have ideas about what they should be doing and began to codify these in training programs, articles, and books. The most common idea, consistent with Deutsch's theory, was that mediators should be scrupulously neutral facilitators of the communication between the parties. This facilitation should enforce norms of respectful dialogue and exert tactful pressure on the parties to move away from positional bargaining and coercive strategies and towards more collaborative efforts to build a "win-win" agreement around mutual interests.

The growing popularity of mediation soon began to generate a research enterprise of small, but significant proportions. Researchers began to address two major questions. The first was evaluative — does mediation provide the superior outcomes claimed for it by its proponents compared to the outcomes which feuding parties generally obtained thru the adversarial "win-lose" climate provided by the courts? The second was about process: What do mediators actually *do*?

Nearly all the research in both categories was highly quantitative and relied on statistical comparisons between groups. By my count, only two investigations of the 2-3 dozen in that period were case studies (Kolb, 1983; Silbey & Merry, 1986). We learned from the evaluative studies that, by and large, mediation worked. Two-thirds or more of the people who used it reached higher levels of agreement and were more satisfied than people who used lawyers or the courts. There were methodological problems with these investigations (e.g. few studies using randomized assignment) and mediation was not a panacea, but overall, it was clearly a viable realm of professional activity (Beck & Sales, 2001; Kressel, 2006).

On the process side there were a spate of studies in which mediators or the disputing parties were asked to describe what occurred in mediation. These studies too relied heavily on quantification and the summarizing of responses to check lists of mediator activity. There were also occasional laboratory studies in the hypothetico-deductive mode in which theoretical ideas about the causes of particular mediator behaviors were tested (Carnevale, 1986; Pruitt & Johnson, 1970). We learned from such studies that the idea of the mediator as neutral facilitator was frequently violated by the use of strong-arm tactics; the circumstances in which these and other tactics were more or less likely to be used, and something about the relationship between mediator behavior and outcomes. That was the good news.

The bad news was that, with the exception of the evidence on mediation's workability, practitioners appeared largely uninterested and unaffected. Practice grew and practitioners began arguing about "models" of mediation, but none of this was influenced in any discernible way by empirical research.

Around this time I began to experiment with divorce mediation as part of my own clinical practice. I soon reached conclusions very similar to Stiles':

Research literature on mediation has been growing incrementally now for more than three decades. Unfortunately, much of this research has little bearing on the concerns of practicing mediators . . . The empirical literature is . . . based on research models that assign priority to precision, control, and the aggregation of data. This reduces the chances of developing results that have clear meaning to practitioners whose concerns run to the concrete. There have been calls to develop research that is more germane to mediation practice (Kressel & Pruitt, 1989; Pruitt, 1986). However, we still lack clear models by which to conduct empirical studies of mediation that practitioners would find relevant (Kressel, 1997, pp. 143-144).

It was at this point that I turned decidedly in the direction of case study methodology. My colleagues and I have now done nearly 100 in-depth analyses of mediators working in such diverse circumstances as a family court, a university affiliated hospital, and in the Office of the Ombudsman at the National Institutes of Health. Most recently we have invited experienced practitioners into the laboratory and observed them at work on the same simulated, carefully constructed conflict. All of this research has been case based. Throughout, the primary goal has been to understand what makes for good practice and to do so with case study methods of which I believe Stiles would approve.

Stiles' (2009) argument for the case study as a theory building device resonates most strongly with my own experience in three major ways: *The value of the case study for connecting theory to practice; the capacity of the case study to capitalize on problematic or unique experiences; and the ability of case studies to generate the kind of "rich observations" that allow theories to be productively modified and adapted.* I will consider each in turn. Conflict mediation is an activity with enough in common with psychotherapy to make me believe that the correspondence between my experience and Stiles' argument is of more than coincidental value.

THE VALUE OF THE CASE STUDY FOR CONNECTING THEORY TO PRACTICE

A core idea in Stiles' paper is the central importance of theory for practice.

The quality of the theories therapists use can powerfully affect the quality of the treatments clients receive. Inaccurate, confused, or internally contradictory theories can lead to inappropriate, inefficient, or damaging treatment" (Stiles, 2009, p. 11).

But by what methods are such pragmatic theories to be developed? Stiles argues that the case study method provides the concrete details and the empathic attunement necessary for a truly useful clinical theory.

In contrast to statistical hypothesis-testing research, case studies characteristically . . . use empathy and personal understanding rather than detached observation, place observations in context rather than isolation, focus on good examples rather than representative samples, and sometimes seek to empower participants rather than merely observe them. (Stiles, 2009, Appendix A, p.7).

It would be difficult to find a more cogent justification for my decision to shift my efforts from traditional experimental and quantitative methods or a more apt description of the case

study methods that have resulted. That undertaking began in the family court of Essex County, New Jersey. I will describe it in some detail because it established the approach and spirit of much that has followed.

Responding to a request for proposals from the New Jersey Administrative office of the Courts to implement and evaluate a divorce mediation program, I was “married” to the probation staff of the Essex County family court. For three years my two probation office colleagues, Fran Butler and Sam Forlenza, and a lawyer colleague, Linda Fish, worked together to mediate conflicts between divorcing parents and to understand and improve our practice.

Over the course of the project, 50 divorcing or divorced couples participated in mediation with us. All of the cases represented parties whose lawyer-assisted efforts at resolving the co-parenting issues had broken down and who were now approaching the court for judicial assistance. More than half of these cases involved extremely polarized conflicts (e.g. physical threats; extensive nonpayment of child support; complete ruptures in child-parent contact).

As the researcher on this team it was my job to organize our collaboration. It was intuitively clear to me that systematic case study debriefing was the relevant vehicle for our purposes. Although I subsequently dubbed the approach I conceived there the “Reflective Case Study Method” and refined the method in my work with the Ombudsmen at NIH, at the time I was simply motivated to find a format that would allow us to make pragmatic sense of our collective experience. From the perspective of traditional research design, self-study is a highly controversial, not to say unacceptable choice. In the traditional view, objectivity becomes impossible from such a vantage point. Objective or not, it was a vantage point that proved instructive for practice.

The details of our case study method and what we learned from it may be found elsewhere (Kressel, Frontera, Forlenza, Butler, & Fish, 1994; Kressel, 1997). The core of the method was the use of reflective debriefing protocols focused around the mediator’s experience during the conduct of a case. The protocols required us to consider four major questions at our weekly case conferences: 1) what were the biggest headaches/obstacles you had to deal with during the session? 2) What did you do and why did you do it? 3) Which of your interventions seemed to work? Which didn’t? Why? 4) What lessons, if any, did you learn for next time?

Our case conferences revealed that our intervention decisions and choices were not primarily conscious responses to the immediate circumstances in the room — that is, they were *not* explicitly derived tactical responses — but were driven instead by strong, and largely *implicit* notions of the role of the mediator. I referred to these overarching notions as cognitive “schema.” Two such schemas dominated our collective practice: The *settlement-oriented style* (SOS) and the *problem-solving style* (PSS). Each of us tended more toward one or the other style in the conduct of our cases and for some of us this inclination, albeit implicit, was strong.

The SOS mediators were primarily concerned with getting a settlement and staying neutral and non-directive. This was the conventional view of the mediator then extant in the practitioner community (and still enshrined in many mediator training programs). PSS mediators followed a different script. They were focused on understanding the cause of the conflict thru

intensive and highly structured question-asking (what we came to refer to as the *interrogatory style*), were highly directive, and were willing to depart from strict neutrality, especially in cases where the conflict was being fueled by particularly destructive behavior in one of the parents. While both styles could produce agreement making, PSS produced a more structured and vigorous approach to conflict resolution, more frequent and durable settlements, and a generally more favorable attitude toward the mediation experience (Kressel, et. al., 1994). SOS was not necessarily bad, but PSS was better.

The Essex project also taught me something about what Stiles refers to as the *empathic* value of case study research. It was in a team context that the four of us attempted to make sense of the phenomenon with which we were confronted and in which we debated better and worse ways to handle difficult mediation situations. To do this constructively and honestly it was essential to maintain an attitude simultaneously challenging and supportive of the person whose case was being discussed. This was not easy and not always successful, but, from a personal point of view, the team meetings were the richest and most rewarding aspect of the research experience.

A primary mode of the team meetings was persistent questioning of each other's tactical choices. A typical series of queries might include questions such as: "Why did you do (or not do) x?" "What were you thinking when you did x?" "Can you say more about the cues in the situation that led you to do that?" "Why didn't you do y or z?" Toward the midway point in the project, as we began to realize that our individual tactical choices appeared to rest on two implicit and contrasting schema of practice, team questions tended to mirror this awareness. ("Did you do that because . . .?" "Is *this* model you are using?")

This sketch should make clear the general concordance of my initial foray into case study research with the spirit of Stiles' argument: Case study research was enormously useful for connecting to practice in a highly relevant way. It grounded me in the concrete detail and the close-up perspective of the struggling human beings —mediators as well as disputants — whom I was trying to understand and help.

I hasten to point out, however, that whereas Stiles appears to have begun his research program on traumatic coping with theory building explicitly in mind, when the Essex project began and even after it was completed, theory building was far from my thoughts. The most I would claim is that shortly after the active phase of the project ended a theoretical impulse slowly began exerting itself. I began to search for concepts and theoretical perspectives by which to communicate our findings and deepen my research program. This search led me to Donald Schön's qualitative work on reflective professional practice (Schön, 1983, 1987) and Gary Klein's more systematic research on expert decision-making in real world settings (Klein, 1998).

Schön's work was important because it opened my eyes to the distinct possibility that studying practitioners could have genuine theoretical relevance. Schön argues that the solution to improving professional performance (he did not write specifically of mediation) must involve research that takes as its starting point the vexing and complex realities of professional activity. The focus of such research, he claims, should be the skillful behavior and thinking of expert practitioners. In Schön's view, important questions of practice can be more readily answered by

helping practitioners articulate their implicit knowledge—their “theories in action”—than by trying to infer practice-relevant lessons from research of the more traditional kind. Schön’s framework has become popular in the practitioner community, including the psychotherapy community (Peterson, 1995), because it takes clinical expertise seriously as a key to systematic (theoretical) understanding rather than as a mysterious and second-class form of knowledge.

Gary Klein’s research on naturalistic decision-making among experts in domains very different from dispute mediation (e.g. intensive care nurses, firefighters) made me realize that my methods and interests were widely shared by researchers beyond the relatively insular world of alternative dispute resolution. Klein’s work also depended very heavily on systematic, case-based interviewing methods. My reflective case study method had much in common with his procedures and I found his arguments for the importance of such case-based methods convincing. The substantive thrust of his findings also resonated with my own: Experts in naturalistic settings of all kinds rely heavily on the recognition of familiar patterns and top down, schema driven thinking. This made conceptual and theoretical goals exciting for me to contemplate and began to shape the studies I subsequently undertook.

THE IMPORTANCE OF UNIQUE EXPERIENCE, ESPECIALLY PROBLEMATIC EXPERIENCE

Stiles argues that case studies are particularly useful for building practice relevant theory because of their capacity to make use of unique, unexpected or vexing moments.

unexpected distinctive features of cases can show where theories need to grow. Unlike statistical hypothesis testing, where rare or unique features maybe regarded as error, case studies allow researchers to incorporate them into research and theory. (Stiles, 2009, p. 17).

Stiles is not alone in stressing the value of the unique or perplexing moment for getting at implicit theories of practice. The idea has been a centerpiece of Schön’s work (Schön, 1983) as well as that of Klein and his associates (Crandall, Klein, & Hoffman, 2006) While I was familiar with neither Schön or Klein at the time of the Essex project, problematic moments turned out to be among the most instructive means for improving our practice and pointed me towards the importance of schema driven thinking (the PSS/SOS distinction).

For example, in one of my early cases, a mother remained silent while the father monopolized the floor, bombarding both of us with a persistent, hostile, and narcissistic set of interpretations and demands about the co-parenting schedule. I set out on what seemed like familiar territory from my family therapy training and tried, in a number of ways, to suggest to mom that her passivity was an important cause of dad’s nonstop domination of the session. To my dismay, what I had conceptualized as a promising tack to rejuvenate the problem-solving dialogue was an utter failure. Mom withdrew increasingly into herself with a look that said plainly, “You are clueless about what it is like to deal with this man.”

The team reflections occasioned by this frustrating episode was the first glimmerings we had about the disproportionate obstacles to constructive problem solving often presented by one of the parents. We came to refer to these individuals with the inelegant but descriptive label of

“Interpersonally Dysfunctional Parent (IDP). IDPs were characterized by a narcissistic preoccupation with their own needs and a corresponding inability to acknowledge the legitimate needs of the other parent or the children. IDPism was not invariably fatal to mediation, but much depended on the mediator’s receptivity to the concept. Those of us who did not have a cognitive category that included IDPism tended to waste an inordinate amount of energy trying to cajole the IDP into more reasonable postures or in futile efforts to remain “neutral.” On the other hand, when team members had (or developed) a view of parental conflict which include the IDP category—a hallmark component of what I eventually labeled PSS—they were better able to promote agreements that placed limits on the disruptions caused by IDPism and were able to help at least some IDP parents achieve legitimate goals (by skillful management of their narcissistic sense of importance).

Another illustration of the ability of case studies to turn the perplexing moment to good use came when I was auditing a tape recording of an opening session conducted by one of my project colleagues, Fran Butler. I realized with surprise that Fran had not offered the parties that most enshrined of mediator procedures—an initial opportunity to “tell their story.” The session in question seemed much the better for it. This discovery led to a reflective dialogue in the research team in which I questioned Fran carefully to elicit the implicit thinking behind her approach. The essence of her response was that by the time the parties arrive in mediation they are typically so angry and polarized in their views that an unstructured invitation to give their side of the story can easily blow the mediation up before it has begun. A more effective procedure, Butler thought, is for the mediator to elicit relevant information through a closely structured series of questions, based in part on a careful reading of the court file. This episode was one of the first to lead us to appreciate the crucial importance of structure of question asking—another central component of the PSS approach (Kressel, 1994).

The lessons I learned in the Essex project about the value of the problematic experience for increasing conceptual insight about effective mediation practice led me to bring such experiences front and center in my next two studies. I believe that this focus helped my colleagues and I solidify our grasp on the “basic units” of mediator cognition around which a theory of mediation practice began to emerge. This is a matter to which I now turn.

THE ABILITY OF CASE STUDIES TO FACILITATE THEORY ALTERATION AND DEVELOPMENT THRU “RICH OBSERVATIONS” (ABDUCTION)

After the Essex study I conducted two more case-based investigations of mediator practitioners; one at a major medical center involving health care professionals with a reputation for being particularly skilled at managing conflicts within their institution; the other with the ombudsmen at the National Institutes of Health. Both studies were practice-oriented, but each also contributed to my growing theoretical interest in the nature of expert mediation practice. The process by which this movement toward theory has occurred is well-captured by Stiles’ discussion of abduction:

Abduction . . . starts with a meaningful account and modifies it. The modification may include correction of previous errors, elaboration of previously unappreciated aspects, or extension to domains not previously expressed. Theory building can be continual, in response to each new observation, and logical consistency is assessed continuously. New tenets may require adjustments in other parts of the prior theory (sometimes major adjustments) in order to maintain logical consistency and consistency with previous observations . . . Of course, labeling this process as abduction does not explain *how* researchers creatively construct new theoretical tenets. (Stiles, 2009, p. 19).

Stiles' account suggests a highly premeditated effort at building theory from cases. As I have noted, my journey from cases to theory has followed a considerably more improvisational path. Nonetheless, that path follows closely Stiles' description of how new observations and exposure to new domains of practice can lead to increasing theoretical clarity.

The Study of Informal Experts at Managing Health Care Conflict

Shortly after the divorce mediation study I became interested in health care conflict. I was directing a graduate certificate program in conflict management at that time and I wanted to be able to design a training seminar that would be useful to health care professionals. The investigation that resulted was a case based interview study in which we asked 17 health care professionals in a major medical center to discuss a "vexing" conflict (Kressel, Kennedy, Lev, Taylor, & Hyman, 2002). All 17 had been nominated by at least two colleagues as particularly skilled at conflict management within the institution. We defined a vexing conflict as one that had been very difficult to handle but from which they felt they had learned, regardless of outcome. The respondents were helped to describe the thinking behind their intervention by means of the *critical decision interview method* developed by Klein and his colleagues (Klein 1998) to study expertise in domains far removed from conflict mediation but sharing certain underlying characteristics with it (e.g. ill-structure problems, shifting, ill-defined or competing goals, action-feedback loops, multiple players).

We learned much from this study about the nature of dysfunctional conflict in health care and were able to develop a well-received training seminar for health care professionals. From a theoretical point of view the findings resonated with certain key themes from the divorce mediation study.

First, our respondents' accounts confirmed the theoretical importance of top down schema for driving intervention strategies, as well as the broad dichotomy between superficial, tactical schema (like SOS) and "deeper" strategic schema (like PSS). There was also evidence that, as was the case in the divorce investigation, these schemas were largely implicit, in so far as certain situational cues rapidly triggered a well-instantiated course of action with little or no conscious reflection on the part of the intervener. While only four of our respondents gave signs of having a PSS-like schema, all four had had prior training in latent cause thinking (e.g. from organizational development courses) and relied on their significant knowledge of institutional culture.

We ended our report of the health care study with these words: "The concept of conflict schema is . . . in need of more systematic work. Can such schema be reliably identified with more

objective empirical methods? If so, how important are they in distinguishing between the skilled conflict manager and the novice or less skilled?" (Kressel, et. al., 2002). Those questions were very much on my mind at the time the Director of the NIH Office of the Ombudsman, Howard Gadlin, invited me to work with him and his colleagues.

The NIH Study.

In this investigation I did no mediation, but served entirely as the facilitator of the reflective case study process. The NIH team I worked with came from diverse professional backgrounds. In addition to Howard Gadlin, the Director of the Ombuds Office, who is a psychologist, the other team members were Doris Campos-Infantino, who has a background in organizational development; Kathleen Moore, a psychologist; Kevin Jessar, a lawyer; and Andre Smith, with experience in Equal Employment Opportunity conflicts.

By and large, we followed the same case debriefing protocols and procedures I had used in the Essex project, but with more explicit attention to using vexing moments as a lever to productive reflection. Eighteen cases provided the data for this study. All involved ombuds mediators working with scientists fighting about aspects of their scientific collaboration, including conflicts between post-docs and their mentors, disagreements about authorship or intellectual property rights, and conflicts within large research groups. The relatively small number of cases was a function of the amount of time consumed by each reflective case debriefing (4-6 hours was typical) and the need to accommodate to the work demands of a very busy office. Because of my now explicit interest in exploring theoretical ideas, I paid a good deal of attention to maintaining as much objectivity as possible in the data analysis process, going thru an increasingly detailed, iterative analysis of the tape-recordings of our reflective case discussions and involving my NIH teammates as much as possible at each stage of the analysis (Kressel & Gadlin, 2009).

The results confirmed the importance of schema-driven thinking in expert mediator performance and provided valuable detail about the elements out of which such schema were built. We captured our key findings under the heading of the ombuds team's *working mental model* of mediation practice.

The working mental model of practice that characterized the ombuds team consisted of two strongly contrasting intervention scripts: a *deep problem-solving* script (DPS) focused on identifying and addressing latent issues of an interpersonal or systemic kind; and a *tactical script*, (TPS) focusing instead on the issues as presented by the parties. This finding was a reaffirmation of the "Deep" and more tactical schemas identified in the two earlier studies. However, in the NIH setting the "deep" script was the preferred intervention mode of all team members, not the predilection of only a few. Every case began with at least a preliminary effort on the part of the ombudsman to search for and address latent causes and team members expressed dissatisfaction if they could not apply DPS in cases where they were persuaded that latent problems were fueling the conflict.

The ombudsmen's choice of which script to follow in a given case appeared to be determined by *first order decision rules* concerning the existence and nature of any latent

problems which might be present (e.g. dysfunctional communication patterns, absentee leadership) and *second order decision rules* (e.g. level of conflict, motivation) concerning the parties' capacity to engage in "deep" problem-solving. Despite their very different foci, both DPS and TPS appear to follow the same metascript of problem-solving stages and ombudsmen used the scripts flexibly and switched to TPS if DPS was unnecessary or not feasible. Both scripts produced agreements that were useful to the parties and to the NIH's scientific purposes, particularly the fostering of scientific competence.

As in the two prior studies, a good deal of the mental model of practice was, in some important sense, hidden from the ombuds-mediators' conscious awareness until reflective case study made it explicit. Initially they preferred to think, "every case is unique" and that their skill resided in being able to "intuitively" grasp its special needs. "We have no "model" had been their motto.

The mental model also appeared to be very heavily shaped by the social context in which the ombudsmen function. For example, in perhaps one-third of the cases the ombudsman's intervention seemed to be aligned with the actor in the dispute whose agenda was most consistent with the furthering of scientific excellence. Thus, ombudsmen often threw their considerable mental energies into finding creative, mutually acceptable ways of freeing promising young scientists from the icy grip of unnurturing mentors.

The most compelling example of the impact of social context, however, was the preferred status of the DPS approach in the culture of the office. The primacy of the DPS script was supported, it seemed to me, by certain social realities that are relatively uncommon in the world of alternative dispute resolution. In that world, mediators typically work alone, are expected to resolve disputes in one or two sessions, and are not part of an institutional setting whose patterned contributions to conflict they get to see on a regular basis. In addition, many mediators come from disciplines with little if any tradition of latent cause thinking (e.g. law), have few opportunities to consult closely with colleagues about the conduct of cases, and in many instances deal with conflicts over a few well-circumscribed tangible issues, frequently money. Such conditions do not provide ripe soil for the development of mental models of practice built on identifying and addressing latent interpersonal or systemic causes of conflict.

The NIH ombudsmen occupy a very different world of practice. They are repeat players in the life of the NIH and therefore in a position to become adept at recognizing the latent sources of its dysfunctional conflicts. As ombudsmen, they are also under a strong role mandate to pay attention to covert patterns of organizational dysfunction (Gadlin, 2000). In addition, three members of the team have training in disciplines that stressed the importance of latent dynamics (clinical psychology and organizational consulting). The culture of the ombuds office also provides frequent opportunities to reflect with other team members about case management. Finally, the parties with whom they work, because of their scientific training, are well suited to the search for latent causes of their difficulties and are highly motivated to address them for the sake of their research. No surprise then that the ombudsmen's mental model of practice favors "deeper" intervention scripts. The Essex divorce mediation study, where a "deep" problem-solving schema was also prominent, involved a social context with similar social parameters—mediators with institutional knowledge, training in latent cause disciplines, opportunities to

consult and reflect, and disputants with good reason (although not always the ability) to work together for an important purpose (the welfare of their children).

The Explicit Move Toward a Theory of Expert Mediation Practice

Spurred by these thoughts, I began to reread the handful of ethnographic, case based studies of mediation with which I was familiar (Kolb, 1983; Brett, Drieghe, & Shapiro, 1986; Wall & Chan-Serafin, 2006; Silbey & Merry, 1986). I rediscovered a fact I had known, but forgotten: Social context can indeed be a powerful shaper of how mediators defined their role and what they feel is good practice — except that for the most part these studies were of mediators working in contexts that promoted relatively narrow, tactical models of intervention.

I have now arrived at an avowedly theoretical framework: Expert mediation practice is embodied in working mental models that are only partly conscious to mediators but that are the driving force behind their tactical and strategic moves. The basic units of these models are encompassing schema and the diagnostic decision rules that inform schema activation. A mediator's model is heavily determined by prior training, the parameters of the conflicts with which the mediator typically deals, and the social context in which the mediator practices. In other words, *all (good) mediation is local* (Kressel, 2007; Kressel & Gadlin, 2009). Some of the key variables that shape the mental model can be specified and predictions about the nature of the mental model of practice which are likely to result can be made, permitting the theoretical framework to be tested and modified. This is perhaps not earth shaking news, but it strikes me as a significant advance over the arguments among practitioners about which “model” of mediation is “best” which completely ignore mediator background and social context, and the hodge podge of explanations for mediator behavior that characterize the quantitative research literature.

CODA: REALITY OR SELF-SERVING BIAS?

There is, of course, a more vexing side to the story of case study research and theory building, and it is one that Stiles also discusses. It goes under the heading of validity. I close with a few comments on that subject.

Stiles (2009) notes that to build theory all the “signs” [terms] of the theory must have a “fixed” meaning (e.g. we cannot have variable meanings attached to terms like “transference”) and that there is a “natural tendency of sign meanings to shift” (p. 17). The inductive, theory building process, he writes,

has to contend with the capacity of human perception to be shaped by the perceiver's frame of reference — a capacity that often favors confirmation of observers' preferred theories. Scientists need to be able to understand clearly what others have observed as well as what the theory says, so there is a great need for signs that are unambiguous, in the sense that their meaning is the same for everyone, to the degree that is feasible. (p. 17).

In Stiles' case, the “signs” in his theory of traumatic coping are captured concretely and very specifically in his APES scale. The “signs” in my developing theoretical framework of

expert mediation practice—schema and decision rules—seem to me to be much broader. I am concerned therefore that it may be much more difficult to define them consistently from one study to the next. I also worry about the degree to which the series of case studies I have described, relying as heavily as they do on highly qualitative and subjective methods, may reflect my own biases.

One way to address these problems is to engage other methods which are either more rigorous or which make use of alternative means of capturing the basic units of the theory. I have begun to try this. The early returns are interesting but mixed.

After the NIH study, my colleagues and I invited professional mediators into the laboratory and had each of them mediate the “same” simulated dispute between two college roommates. Two carefully prepared undergraduates played the roles of the roommates. The study was motivated by the desire to test whether intervention schemas could be reliably identified in a more or less controlled setting using more objective approaches to measurement. The dispute between the roommates was about surface issues (messiness, noise, and disrespect) but was carefully constructed to include certain latent sources of tension (e.g. the negative impact on their relationship of the roommates increasing workloads as the semester progressed). We videotaped the mediation sessions and immediately after the session asked each mediator to view the tape and report any thoughts or feelings they remembered having during the session. The videotapes of the mediators’ performance were rated by trained observers for evidence of any overarching schemas as well as for specific tactical behaviors. The mediators post-mediation recall statements were also analyzed. The results have yet to appear in print, but they have been presented at professional conferences.

Using the statistical method of multidimensional scaling we were able to divide our mediators into those who viewed the session primarily as an occasion for negotiating an agreement on the surface issues and those who focused more on encouraging dialogue and mutual understanding. In this latter group was a small subset of mediators who gave evidence of something akin to the “deep” problem-solving script of the earlier studies. This was comforting to a degree, but the greatly simplified situation and our more constrained approach to data collection did not allow us to capture anything like the complex decisions rules and alternative schemas that the NIH study reported.

There are other ways to test the theoretical framework as well. One study, currently underway, is to evaluate the impact of social context on mediators’ schemas using survey and scaling methods on a large national sample of professional mediators working in very different circumstances and with varied professional backgrounds. Still another tack is the comparative case study design. In such a design cases are carefully sampled from settings in which dispute characteristics, mediator professional background and the parameters of the social context differ in ways specified by the theory and the impact of these differences on mediator mental models carefully documented. I have not yet mounted such a study but it has been noted as a useful way to extend the theoretical power of the case study method (Druckman, 2005; Yin, 2003).

SUMMARY

As a result of reading Stiles' paper, it has been tonic to discover that, like the lady who realized to her delight that she had been speaking prose all her life, I have been talking "theory" all along. I am grateful to Stiles for making this clear to me (and to Dan Fishman, for affording me the chance to read and comment on Stiles' article). Stiles' paper makes a cogent argument for the case study as a vehicle for building and testing theory, especially theory about practice. As I have indicated, his account describes quite well my own research trajectory. I did not start with theory in mind, but it seems to me that, for many of the reasons that Stiles advances, the case study approaches I intuitively reached for on utterly pragmatic grounds fostered in me a gradual but decided movement toward theoretical possibilities. It strikes me as well, that in a field like conflict mediation, which is so much younger and much more lacking in theories of practice than psychotherapy, the case study is, if not the method of choice for developing a theory of expert practice, then certainly on an equal footing with other methods. Having read Stiles I feel more inclined than I was before to make this point and to make it without apology.

REFERENCES

- Beck, C.J. and Sales, B.D. (2001). *Family mediation: Facts, myths, and future prospects*. Washington, D.C. American Psychological Association.
- Brett, J.M., Drieghe, R. & Shapiro, D.L. (1986). Mediator style and mediation effectiveness. *Negotiation Journal*, 2, 277-285.
- Carnevale, P.J.D. (1986). Strategic choice in negotiation. *Negotiation Journal*, 2, 41-56.
- Crandall, B., Klein, G. & Hoffman, R.R. (2006). *Working minds: A practitioner's guide to cognitive task analysis*. Cambridge, MA: MIT Press.
- Deutsch, M. (1973). *The resolution of conflict*. New Haven: Yale University Press.
- Druckman, D. (2005). *Doing research: Methods of inquiry for conflict analysis*. Thousand Oaks, CA.: Sage Publications.
- Gadlin, H. (2000). The Ombudsman: What's in a name? *Negotiation Journal*, 16, 37-46.
- Klein, G. (1998). *Sources of Power: How People Make Decisions*. Cambridge, Mass.:MIT Press.
- Kolb, D. (1983). *The mediators*. Cambridge, MA.: MIT Press.
- Kressel, K. (1994). Francis Butler: Questions that lead to answers in child custody mediation. In D. Kolb (ed.), *When talk works: Profiles of mediators*. San Francisco: Jossey Bass.
- Kressel, K. (1997). Practice-relevant research in mediation: Toward a reflective research paradigm. *Negotiation Journal*, 13, 143-160.
- Kressel, K. (2006). Mediation revisited. In M. Deutsch, P.T. Coleman, & E.C. Marcus (Eds.), *The handbook of conflict resolution: Theory and practice* (pp. 726-756). San Francisco: Jossey Bass.
- Kressel, K. (2007). The strategic style in mediation. *Conflict Resolution Quarterly*, 24, 251-283.
- Kressel, K., Frontera, E., Forlenza, S., Butler, F., & Fish, L. (1994). The settlement orientation vs. the problem-solving style in custody mediation. *The Journal of Social Issues*, 50, 67-84.
- Kressel, K. & Gadlin, H. (2009). Mediating among scientists: A mental model of expert practice. *Negotiation and Conflict Management Research*, 2, 308-343.

- Kressel, K., Kennedy, C. A., Lev, E., Taylor, L., & Hyman, J. (2002). Managing conflict in an urban health care setting: What do “experts” know? *Journal of Health Care Law and Policy*, 5, 364-446.
- Kressel, K., & Pruitt, D.G. (Eds.). (1989). *Mediation research: The process and effectiveness of third-party intervention*. San Francisco: Jossey-Bass.
- Peterson, D.R. 1995. The reflective educator. *American Psychologist* 50: 975-983.
- Pruitt, D. G. 1986. Trends in the scientific study of negotiation and mediation. *Negotiation Journal*, 2, 237-244.
- Pruitt, D.G. and Johnson, D.F. (1970). Mediation as an aid to face-saving in negotiation. *Journal of Personality and Social Psychology*, 14, 239-46.
- Rubin, J.Z. and Brown, B.R. (1975). *The social psychology of bargaining and negotiation*. Orlando Fla.: Academic Press.
- Schön, D.A. (1983). *The Reflective Practitioner: How professionals think in action*. New York: Basic Books.
- Schön, D.A. (1987). *Educating the reflective practitioner*. San Francisco: Jossey-Bass.
- Silbey, S.S. & Merry, S.E. (1986). Mediator settlement strategies. *Law & Policy*, 8, 7-32.
- Stiles, W. B. (2009). Logical operations in theory-building case studies. *Pragmatic Case Studies in Psychotherapy*, Vol. 5(3), Article 2, 9-22. Available:
http://hdl.rutgers.edu/1782.1/pcsp_journal
- Wall, J.A. & Chan-Serafin, S. 2006. *Inside the black box: Processes and precursors of agreements in civil case mediations*. Paper presented at the meeting of the International Association for Conflict Management Montreal, Canada.
- Yin, R. K. (2003). *Case study research: Design and methods*. 3rd Edition. Thousand Oaks, Ca.: Sage.