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The American Academy of Forensic Psychology annually presents a Distinguished Contribution in Forensic Psychology Award to an individual whose work has significantly moved the field forward. Dr. Follingstad received this award for decades of productive research, testimony, and public education regarding family, adolescent, and women's issues, all areas in which she has received numerous research grants over the years. Following is her acceptance speech, presented at the American Psychology-Law Association convention in Vancouver, B.C. in March 2010.

Diane Follingstad, Ph.D., A.B.P.P., is a Diplomate in Forensic Psychology. She earned her Ph.D. from the University of Colorado in 1974. Dr. Follingstad specializes in the areas of:

- Psychology and law
- Physical violence in adult relationships
- Psychological aggression
- Victimology
- Adult therapy and diagnostics
- Psychology of women

Since retiring from the University of South Carolina, where she was a Distinguished Professor in Clinical and Forensic Psychology, Dr. Follingstad has moved to an Endowed Chair at the Center for Research on Violence Against Women at the University of Kentucky; she is currently a Professor in the Department of Psychiatry at the University of Kentucky Medical School.

Increasing Complexity: Resisting Simplification in Forensic Psychology

Diane Follingstad, Ph.D., Department of Psychiatry, University of Kentucky Medical School, Lexington, KY, USA. Email: Follingstad@uky.edu

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Thank you very much, Dr. Witt. That was a generous and gracious introduction. To express adequately how honored I feel to receive AAFP's Distinguished Contribution Award would take up all of my time here, I'm afraid.

For almost thirty years now, forensic psychology, and in particular ABFB and APLS, have been my professional home. My teaching, clinical practice, and research have come to center largely around forensic concerns, and people in this room and on that impressive list of past Distinguished Contribution award winners have been my professional models, my professional peers, and my friends. To have been in your company these years has enriched my professional and intellectual life.

With the academic world too often tainted by petty politics, and with the larger field of clinical psychology increasingly reduced, it seems, to a simple-mindedness, you, my forensic psychology compatriots, have been a refuge—an oasis.

That you now place me in the company of the likes of past award winners, as I have said, is beyond an honor. I accept your award gratefully, happily, and, I hope, gracefully.

Now it occurs to me that a person vastly increase her chances of garnering an award such as this simply by being around a long time. You throw enough darts at the target and you're bound to hit the bulls-eye now and then. I would hope that I've hit the target a bit more than "now and then," but there do seem to be some advantages to growing older.

Nice things like getting awards can happen. And you also get to complain.

I would like to say that age brings the privilege of *kvetching*, which is a word that I really like and wish I could claim. But I come from a long line of plain and dour Norwegians—and we don't get to *kvetch*. We just complain. And so tonight I'm going to do a bit of Norwegian complaining.

The title of my talk—which isn't very subtle—is "Increasing Complexity: Resisting Simplification in Forensic Psychology," and I realized as I set about trying to figure out what

I wanted to say, that, at the basis of my complaining, is that I do care about psychology, and I am bothered by the direction it is going.

Probably my frustrations are directed mostly at clinical psychology as a field as well as how clinical psychology has been applied to the forensic arena, and I worry that its changes parallel general trends in the United States, where Americans seem preoccupied with the most mind-numbing things (Survivor, for example), where the capacity for depth and thought has become limited, where there is a feeling of anti-intellectualism, or maybe more simply, as Bob Herbert (not a psychologist) says, we “live in a nitwit age.”

But here we are in 2010.

The raw material that is the basic concern of clinical psychology research and practice, that is, human behavior—broadly defined—and its vicissitudes, is inherently ambiguous, as anyone who has ever done evaluations for court or ever given courtroom testimony should know. It’s the awareness of that ambiguity that produces a bit of queasiness before we’re called to the stand, a little tightening in the stomach.

I remember after having *given* a workshop regarding battered women and self-defense issues, that I felt some consternation because I realized there was no real definition of a “true” battered woman and I figured that would likely be a question directed toward me in court as well as when I conducted such a workshop again, and I briefly believed that there should be a defensible answer. But, if we’re honest with ourselves (which is not always useful by the way), we know that the story that we’re about to tell to the court has been fashioned this way: We’ve used imperfect methods (interviews and psychological tests) to assess variables that are difficult to define, let alone measure, in any kind of reliable way.

Our stories often involve conclusions about state of mind, motivation, and intent, as if the intricacies of unconscious experience, the multiplicity of functions served by behavior, and the ambivalent nature of human motivation can be coherently organized and neatly summarized.

If we are wise, we know that this is not so. We take the oath, we take a deep breath, and do the best that we can.

It is the complexity, ambiguity, and elusiveness of human experience that can make clinical and forensic psychology seem, to borrow Janet Malcolm’s famous term applied to psychoanalysis, “impossible professions.”

But, in my view, the very complexity that sometimes makes clinical and forensic work seem so difficult, is ultimately the thing that makes them disciplines to which it is worth devoting one’s intellectual life. Human behavior *is* contradictory, mysterious, conflictual, layered, intricate, and ironic, and it is this complex aspect of people, I will contend, that has led bright minds to professional psychology and still does.

So what's my complaint, you ask?

Wherever I look—clinical training, clinical practice, and clinical research—the prevailing trend is in the direction of developing psychological understandings that are rigidly categorical, reductionistic, and excessively simplified. For some time now, I have had this creeping sensation that clinical psychology was simplifying itself into potential meaninglessness.

You are all familiar with the most glaring examples of this trend. Within many universities, clinical training has given way to clinical *research* training where students *intentionally* are given only a cursory exposure to practice. An unnamed clinical program recently reduced assessment courses from three down to one, only requires one psychotherapy practicum, and no course on personality is even offered—their rationale, they say, is to give clinical students more of a chance to take other courses “more pertinent to their training such as epidemiology.” EPIDEMIOLOGY???

Psychotherapy and psychodiagnostics, the backbone of clinical psychology practice, have both fallen quite far from the lofty perch that they occupied in the 1960s and 70s. The energy behind psychotherapy theory back then is something our current students can never relate to—Fritz Perls, Carl Rogers, Albert Ellis—these guys were like rock stars; Virginia Satir, Sal Minuchin, and Carl Whitaker were writing about family therapy and a new way of looking at pathology; and there were real advances in psychoanalytic theory, especially applied to the understanding of patients with severe character pathology and to hospital treatment. You look around in 2010 and all of that is just a distant memory.

It's hard to believe today that we were once excited about psychotherapy and assessment, that these clinical skills were what used to bring almost everyone to clinical psychology. But now, the reduced emphasis on clinical skills is fostered by professors with little clinical experience themselves (who ironically also do the “clinical” supervision), and who actually are, too frequently, even anti-practice. Then, these graduate students, who are untrained in the applied skills, go on to do research on aspects of human behavior about which they cannot formulate pertinent research questions.

Clinical training, both in the applied and research ends, is more and more a sad example of the blind leading the blind. No wonder manualized treatments have become so popular within the university (they must feel like a veritable life raft) and no wonder that clinical research questions seem increasingly superficial. No one knows enough to know what to ask. The curiosity that propelled many of our graduate students to psychology in the first place frequently is crushed by this kind of training regimen.

The practice sector—and again, I know I am preaching to the choir here—has fared little better and there are many reasons for this. The pernicious influence of the insurance industry has been analyzed to death, but to little avail. At the policy level, mental-health treatment is dictated by MBAs while untrained staff run the enforcement and insurance claims process. Consequently, we get psychological “treatments” aimed at symptom

reduction with little regard for the *persons* who are seeking treatment. A giant shift in treatment modalities has turned individuals into diagnostic categories, so that people no longer have personalities—especially because insurance companies won't pay for a person to have a problematic personality.

Increasingly, psychologists, and the culture at large, view psychological problems as *afflictions*, things *separate* from the persons who experience them. This is, in my view, a childlike view of people and behavior. It can be satisfying to lay this sorry state of affairs at the feet of the insurance industry, but while it can be satisfying, it does seem incomplete. Because, the fact is, our own have been at the forefront in peddling simplistic treatment packages aimed at symptom clusters, under the guise of “empirically supported treatments.” It is not uncommon to hear colleagues assert that using anything other than the approved list of treatments should be considered *malpractice*.

So we live in a world where the easiest treatments to study make the approved list, and this is a world where students master manualized treatments for depression without the foggiest idea of the nature of depression, and without the basic understanding that knowing that a person is “depressed” tells us very little, if anything, useful about him.

Psychotherapy, as *taught today* within the university, could just as well be provided in a one- to two-year program at the community college. It has become a set of procedures that seem divorced from complex thought and theory.

Simplistic views of human life are also encouraged by our culture in the form of the media pushing us to reduce our extremely complex field to sound bites. I know many psychologists who are willing to try to do this, under the rationale that it's important to get psychology “to the people.”

After a Virginia Tech student shot many students and faculty on that campus a couple of years ago, I received a call from a reporter for Newsweek. She asked me to explain, for her article on that tragedy, how a person “just snaps.” I quickly told her that I was *not* someone she would want to speak with, because, in generalities and in specifics regarding that young man as the shooter, an analysis would take much time, along with significant amounts of personal knowledge of the man's history, thought processes, relationships, etc., and even then, the question would not likely be answered to an acceptable level of clarity and accuracy.

Just think back to the last time you spoke with a reporter for a half hour and your nuanced and complex answer was badly summarized in one sentence (making you sound just a little smarter than Elmer Fudd). But we also add to the problem by simplifying psychological concepts through cooperating with media to produce, for example, the five-point strategy to handle stress over the holidays.

I will not leave this rant about simplification without commenting on the caving in to financial pressures of university administrations which in turn has reduced scholarship to a business venture in which the value of one's research is gauged by funding dollars.

Also, down the line, many department chairs moved quickly to adopt this focus, and even faculty, your peers and friends, now determine quality, tenure and promotion of colleagues on the basis of whether they have gotten grant dollars.

How is *this* related to simplification? Well, if you listen carefully, you don't hear many conversations about "are we studying the really important questions?" Rather, they revolve around, "So what's the hot topic now?" or "What are *they* saying will be the new focus for funding?" or "Which areas are the ones on which our university has decided to focus?"

As I left the university where I had spent over 30 years, the designated centers of excellence were announced as: public health (because we know grant funding is still alive and well there), nanotechnology (which no one seemed to be able to define), and automotive technology. If I had stayed on, I planned on moving my research area into the psychology of front-wheel drive.

I have witnessed colleagues with superb records of research treated as second-class citizens because they conducted research in an area that did not require funding, and they were invested in the research questions that guided their program of research. I have seen other faculty tell them that they should buy their own pencils and notepads because they were not bringing money into the department.

All this, and I have never seen a research study which demonstrated that research done through *funded* grants, compared with unfunded research, resulted in higher quality products, or more thoughtful investigations, or more inspiring notions.

Now, I am aware that some of the forces of simplifications I have trotted out may seem less relevant for forensic psychology except as they touch on the clinical aspects of the field. Forensic psychology, during its existence, has seemed to strive for better educated practitioners, better instrumentation, better psychological explication of legal issues. Even a definition of cognitive complexity by two authors, Elder & Paul, sounded to me like the preamble of the forensic psychology guidelines: "Cognitive complexity, broadly defined, is the ability to absorb, integrate, and make use of multiple perspectives. Individuals using cognitively complex perspectives ask questions, admit uncertainty, examine their own beliefs, tolerate ambiguity, listen carefully, suspend judgments, look for evidence, and adjust opinions when new information becomes available."

But, we must continue to ask: Do our research and instrumentation continue to reflect the inherent complexity of our field?

How many of you find it imperative to "keep up with the literature" in order to conduct your practice effectively? How many of the research presentations to which we listen are tackling intricate issues in ways that reflect how complex they actually are?

Truly significant knowledge in applied psychology seems to accrue at a glacial pace, which is expected. We don't study molecules, we study people, human lives, human suffering.

But have you noticed that we seem to be experiencing a vortex or black-hole phenomenon or some other astrophysical spectacle that is fueled by a need to demonstrate that we are onto the "new" with a corresponding rapid dump of past knowledge?

The rich wisdom of the past is rarely read or consulted (remember you don't want citations before 1990) and it seems that all we can do is live in hope that it will be rediscovered someday. Once in a while, some piece reemerges, attachment and relationship schemas, for example, both psychoanalytic concepts, but, even so, I'm not convinced the current approach to them would be recognized by the original thinkers. Most graduate students are shocked to discover that the origins of these lie within psychoanalytic history, not cognitive behavioral theory. [It has been many years since the average clinical student has read even one original article from the vast reservoir of psychoanalytic literature.]

The study of human lives is difficult to do well, and even when done well, our research is only too often, only suggestive. This is simply our cross to bear.

In the vast array of variables that affect human behavior, no one thing is going to account for much of the variance. We live with ambiguity. The problem is, many of us don't live well with ambiguity. We can't tolerate it. We wish for the psychological world to be easily measurable, for there to be linear causality, for phenomena to be reduced to two or three categories. In so doing, we have often become promoters of simplicity.

I think, for example, of the raging popularity of trauma theory over the past twenty years or more. Now, I am not downplaying the impact of physical and psychological trauma, and I've spent much of my own career around the edges of trauma theory, but we have become a culture that serves up trauma as an *explanation* with an almost sickening frequency.

The simple idea that external event A leads to pathology B has had a remarkable appeal to clinicians and forensic psychologists. It clears away a hell of a lot of brush. It also, by the way, does away with the need to understand the client in depth as an active, constructing, conflicted, defending, adapting person, who will be adequately understood only after a long time, if at all.

It gives the diagnostician and the therapist the illusion that he or she knows what is wrong and what to do. And what mischief this has led to—the great multiple-personality/satanic-ritual hysteria of the 80s and 90s, the false-memory fiasco—all of which, I believe, was at least partially because we were seduced by the prospect of simplicity.

There is also a new kid on the block these days. Have you noticed that everybody is now a neuroscientist? Or at least a clinical scientist? Many won't even use the term "psychologist" anymore.

How did this happen? Well, I think, it happened partly because of a technological advance—functional magnetic resonance imaging or, as those in the know like to say it, fMRI—that allowed for the examination of the relationship between brain activity and “fill in the blank.”

Now much of this *is* quite remarkable from a pure science point of view. The problem, and I think there is a problem, is in application. Increasingly we are hearing the encroachment of neuroscience language (the language of brains and brain function) into the clinical realm and into our understanding of people and pathology.

We grew up hearing how psychologists were incredibly advanced for moving beyond the old “medical model” that viewed those with psychological difficulties as having some sort of disease process. We were so proud in our new approach. But do you feel as though we’ve gone full circle?

Patients with depression or anxiety or anorexia are now told very straightforwardly and simply that they have a brain disease. And how did they get that disease? Well, genes, of course—genes and neurons. A leads to B leads to C, and so on.

But, of course, ***we do not know this.***

Brain research is in its infancy and what we have, basically, are some interesting correlations between lit-up brain areas and behavior. It is the eagerness of clinicians (and granting agencies and the culture at large) to grab hold of this, however, that points to the problem. And the problem is a flight into oversimplification because of an intolerance of uncertainty and ambiguity.

Now that I am in a research position at the University of Kentucky, hired through the Center for Research on Violence Against Women, and with an academic home in psychiatry, I am aware that other disciplines are not exempt from the scourge of simplification either.

I am currently working on a grant proposal regarding outcomes from psychological maltreatment. Because of the particular slant of the proposal, I needed to include a psychiatrist who specializes in opiates, an epidemiologist who has already done research in Appalachia, and a statistician familiar with the kinds of statistical models that make the rest of our heads spin.

So when I got them all together after having them read some introductory material and speaking to them about the complexity of the area and my desire for an in-depth study, they encouraged me to make the design simpler, to try not to accomplish too much, and to pick one variable so that a clearer path model can be generated and the granting agency will understand better what is being studied. (Actually I have heard this last comment so many times, that I am starting to wonder who NIH has chosen to read these grants!)

There are less overt manifestations of this simplification phenomenon as well. For many years now I have been interested in various aspects of intimate partner violence, both physical and psychological. The latter area has been, unfortunately, summarized under the heading “psychological *abuse*.”

The term “abuse,” I believe, is by now loaded down to the breaking point with emotional, moral, and socio-political meanings, all having as one aim to reduce, dichotomize, and oversimplify a complex interpersonal phenomenon.

I came across an interesting article on the NY Times sports page a few weeks back that relates to this. The Times story reported that Dr. Harold Seymour, apparently a well-known baseball historian, was helped a great deal in his writing and editing by his wife, Dorothy Mills, but failed to credit her or share authorship. Now, since Dr. Seymour’s death in 1992, Ms. Mills has worked up, according to the Times, a fairly sizeable resentment about her exclusion, and happily, the Society for American Baseball Research has now given her proper due.

But what caught my eye was this. The Times suggested that Dr. Seymour’s failure to share credit with his wife was an example of “what can best be described as intellectual spousal abuse.”

“Intellectual spousal abuse.” I must admit, that’s a new one to me and I’ve been reading this literature for a long time. It got me to thinking, why did the author put it precisely this way? Why did he not say, for example, that:

Seymour and Mills, having adopted roles in relation to one another that were reflective of the time, colluded to position Mills as an invisible helper, the strong but silent wife behind the professional man and that, by virtue of changing times and her own psychological growth, Ms Mills, at age 81, chose to be something other than silent. She chose to give public voice to apparently longstanding but unexpressed resentment.

Or,

Dr. Seymour, a cantankerous, bull-headed, loveable, but more-than-a-touch narcissistic history professor, adored Jane Mills, his one time undergraduate student and typist and woman 17 years his junior, but never managed to detach himself from a view of her, mostly unconscious, as a daughterly clerical assistant—an innocent, adoring, young helper.

Ms. Mills idealized professor Seymour, took refuge in a sense of him as a wise and benevolent father, and unconsciously adopted a submissive, self-sacrificing role in relation to him to complete their complementary and largely happy relationship.

Since his death, friends have described her as having become livelier, more outspoken, and more assertive. She has told friends that she has been rethinking aspects of her long marriage.

You get the idea. I've made up almost all of that except the fact that Mills was Seymour's undergraduate student and typist and 17 years his junior.

Whatever the reality of Mills' long marriage to Seymour, and the bargain, both conscious and unconscious, that they struck around their collaboration and his failure to credit her great help, it surely must be more complicated than the idea that "Seymour abused Mills."

But the construction "Seymour abused Mills" has obvious allure, which is no doubt why the author chose it. It adopts a term, "abuse," that is all too common in the popular culture, and so it's a shorthand. But shorthand for what? In my view, it's shorthand that translates the great and varied complexity of intimate relationships, with their unconscious contracts around roles and mutual-need gratification, into a simple, morally tinged, and frankly primitive dichotomy of the good one and the bad one, the passive victim and the powerful "abuser."

With one word it says, in effect, "we really know what's going on here," and all the detail, and nuance, and subjectivity that ordinarily would inform an accurate understanding of any relationship *are just not necessary*. The term "abuse," and its implied meaning, reduces, dichotomizes, and simplifies. What a relief!

[I personally bemoan the fact that I cannot persist in believing this type of dichotomy, because I would love to convince those around me that I am obviously the "good" person in my marriage.]

Now reducing, dichotomizing, and simplifying might be just what the NY Times wants out of its sports page writers—though somehow I doubt it. But when the same processes occur in psychologists in their clinical and forensic work and research, then we are in trouble.

I believe that my most current research area—the area of psychological abuse—is in trouble, and how could it not be when it's demarcated by the term "abuse." How is it remotely possible to study psychological cruelty and mistreatment in intimate relationships, a complex transactional phenomenon, when we close down nuanced thought with a weighted and judgmental term like *abuse*?

Well the answer, unfortunately, is that we have not been able to do it very well. One problem that plagues any new research area is measurement, and the area of psychological aggression (that's what I'll call it here instead of abuse) is thus plagued.

How do we determine through measurement whether relationship X is characterized by psychological aggression, whether this aggression is one-sided, or reciprocal? An arti-

measuring psychological aggression, but the first approach in this area has been the construction of rating scales which requires one member of a couple to say, in effect, whether such and such ever happened to him or her, and *much less frequently*, whether he or she *personally* has done such and such.

Now “such and such,” based on current measurement devices, has unfortunately included such things as “said something spiteful,” or “sulked and/or refused to talk about it,” or “yelled at you” alongside much more egregious items.

According to many of these scales, if you endorse any of these conflict items occurring even once, you are put in the category of having been psychologically abused and of being in a psychologically abusive relationship, and of course, by implication, the partner is an abuser. This could almost be comical, if it did not have the potential to create so much mischief.

If our current sensibility regarding couples’ interactions is that of hypersensitivity, such that we assign to the mildest conflict or misbehavior or ineptness a label of “abuse,” we have lost a view of human beings as a complex mix of altruism and selfishness, empathy and meanness.

All expressions of psychological aggression in couples should not be labeled pathological. I recommend Steven Mitchell’s book, *Can Love Last?* Which eloquently discusses how intimate relationships contain and absorb the passion of the drives.

If *any* act of psychological coercion, and *any* act designed, wittingly or unwittingly, to induce some negative emotional state in the other—say guilt or anxiety or simple upset—is to be regarded as psychological *abuse*, this leaves very little room, I would say *no* room, for studying psychological aggression in couples as a part of everyday life.

The plot thickens. When we apply the term *abuse* to any instance of psychological mistreatment, as these measurement scales do, we have placed upon that instance, a priori, a boat-load of meaning. We have judged the behavior in moral terms—it is “bad” of course—but we have also arranged the actors according to rigid, and again morally tinged, roles. The actor (the “abuser”) is bad, and the receiver (the “victim”) is passive, innocent, and to that extent good.

I believe that this dichotomy *sometimes* applies in real life, in real relationships. Sometimes there *is* a powerful, controlling, cruel, sadistic person who assaults a weaker, fearful, intimidated, relatively helpless person using a variety of maltreatment strategies. But if we take a step back, we once again see the psychological world as more complex across couples.

The dichotomy of bad and good, abuser and victim, as appealing as it may be for political and emotional reasons, is terribly, terribly limited—and more to the point, often very wrong. Like physical force, and especially at lower levels of severity, psychological mistreatment in intimate relationships is often reciprocal and done in the service of need

gratification, role maintenance, the re-enactment of unconscious object relations paradigms, and anxiety and intimacy regulation. Some research I have conducted in a national sample shows much reciprocity, even to type, although nearly everyone says the partner did more and was more harmful.

Psychological aggression, as a concept, does not fit neatly into the categories of abuser and victim. To suggest that psychological aggression in relationships may arise out of complex interpersonal mechanisms has led to resistance, outcries that, to go beyond the victim/abuser dichotomy, is to blame the victim and deprive her/him of protection.

Regarding violence toward women, in the world of women's advocacy and women's rights, this challenge has been an instructive and powerful concept that has opened eyes and changed a culture—and for that we are all grateful. But, applied to the pursuit of knowledge by clinical and forensic psychologists, the phrase “blaming the victim” is a show stopper.

Researchers investigating *physical* abuse in intimate relationships by women have experienced a firestorm. But, as psychologists, we must continue to ask the tough questions, look squarely at the data, and follow scientific inquiry where it leads us. We should ask questions such as “Why do couples harm one another?” without preconception, other than the idea that human behavior is complex and not reducible to polarized, simplistic categories and roles.

At this point, an unspoken rule of talks such as these is that, while you might get to complain, especially if you've just won an award and the audience is sort of forced to be polite, you've got to suggest some solutions—kind of like the “directions for future research section” of a paper. I must confess, I'm a little short on solutions.

But, in the interest of not leaving you stuck with only the memory of my despair, let me focus briefly on clinical training and clinical research. What I do believe is this—we can't afford to continue pretending that we're providing adequate clinical training in most university programs these days when we no longer even come close.

The dismissiveness and contemptuous attitudes toward psychotherapy and diagnostic training suggest that moving clinical training out of academic settings may actually allow for restoration of the craft to the better place it deserves. Because I hear faculty say “Oh, they can learn the practical end on their internship,” maybe the whole applied-training experience could be ceded to hospitals and other clinical settings where we still have some (old, I might add) psychologists who could run clinical psychology training programs.

Regarding the research end, entering the area of psychological aggression made me realize that, in that specific area, but also in many aspects of clinical research, we need to back up rather than speed ahead, spend more time understanding complex phenomena, write about problems and inadequacies of research design, and work with profes-

sionals across different fields (*if warranted*) to wrestle with definitional and conceptual intricacies.

My husband, who I consider to be a wise clinical psychologist, told me that I was old enough—I don't think he really used those words—to write a *position paper* about this problematic area of research, psychological abuse, rather than to just contribute another study that would have to be considered in the mix of articles already out there. That made sense to me—put these problems out in the literature to have others consider them and perhaps change the approach to defining and measuring psychological maltreatment through *increasing* the complexity of it.

Probably, of anything I have done in my career, I am glad I wrote that article because it was meant to stop the rush of articles based on flawed, deficient, and primitive measurement, to critique the methods to date, and to raise clinical issues that needed to be considered when asking individuals to give their self-report to this extremely face valid inquiry.

In my own small way, I am trying to chip away at the measurement problems by designing studies that try to address each identified criticism. For example, I am proposing a grant that will for the first time ever, as far as I know, have both members of couples who report more serious psychological aggression interviewed, with ratings of response style, relationship characteristics, personality characteristics, etc. generated by clinicians.

But what would I want you to take away from my harangue, which has been my first and only venue for venting of, like Dr. Seymour's wife, my slowly gathering resentment over many years? [Actually, those of you who know me, know that I have always loved this profession.] I think it is this—that I wish for our profession to step back, process more, protest more, develop controversies, demand time to develop the big picture, refuse to simplify major ideas, force people to listen to long, involved explanations, challenge goals impinged upon us that seem counter to the development of psychological realities, and try to stay connected with our rich history. Bring in the thinkers and let them orate, and let's see if we can rescue the amazing complexities of our most fascinating field.

Thank you again for this most valued award.