

**The Long Journey of Psychoanalytic Diagnosis:
The PDM between Diagnostic Accuracy and Clinical Complexity**

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Introduction

The issue I want to address tonight concerns the effort to develop a *Psychodynamic Diagnostic Manual* that can preserve clinically meaningful diagnostic practices and bridge the gap between the categorical, “neo-Kraepelinian” diagnosis exemplified in the DSM and ICD systems, and the traditional “biopsychosocial” clinical practice of diagnostic formulation. In the PDM effort, we have tried to preserve a diagnostic sensibility that is dimensional as well as categorical and is also inferential, contextual, and integrative. Part of this talk will eventually be published in a book about overall mental health that I am currently writing for an audience that I hope will go beyond the mental health community, so you are getting an early look at the contents of that future publication.

I see our overall challenge in the area of clinically meaningful diagnosis as a piece of a larger problem endemic to large democratic societies: When decisions are made on the basis of statistical averages and mass impact, bureaucracy thrives, but thoughtful attention to individuality declines, along with sensitive consideration of the needs of distinct sub-groups. In bureaucratized cultures, people easily find themselves feeling like nameless, faceless cogs in some huge machine, or, if still animate, like Kafka’s cockroach.

This dehumanizing effect can be particularly problematic when individual and group differences matter greatly, as they do in the area of psychology and psychotherapy. Depending on someone's situation, temperament, life stage, gender, and many other factors, one person's sanity can be another person's craziness. For an overly controlled man to act impulsively might be an achievement, whereas impulsivity would be more-of-the-same psychopathology for his chronically out-of-control friend. A Hindu's respect for astrology has a different meaning from an appeal to the stars for guidance by a woman who was raised Lutheran.

Within the lifetimes of many people reading this book, the mental health field has undergone a gradual but profound shift, away from trying to understand the unique patient and toward assigning labels based on categories of psychological suffering about which certain experts concur. Several decades ago, therapists were trained to develop for each client a "case formulation" that attempted to capture what was unique to that person. Such a formulation included observations about the individual's temperament, maturational issues, defenses, emotional tendencies, identifications, relationship patterns, underlying beliefs, and sources of self-esteem, among other factors.

Currently, there are pressures from converging directions for therapists to formulate patients' suffering instead in terms of agreed-upon, nameable "disorders." Psychological problems have been grouped together in our official documents (the Diagnostic and Statistical Manual of the American Psychiatric Association and the International Classification of Diseases) based on whether they share certain observable symptoms. Rather than developing hypotheses about the specific nature of the unhappiness of the person one wants to help, one matches the patient's report to pre-formulated criteria for each category. Along with this shift have come powerful pressures to restrict the client's treatment to specific procedures that have been shown,

in research that depends on artificial conditions and statistical averages, to reduce observable symptoms that define those reified disorder categories. As with most top-down change, certain efficiencies have resulted, but at a heavy hidden price that I will explore tonight.

Psychiatry has developed differently in this respect from other areas of medicine, where we do not find illnesses categorized as, for example, “fever disorders” or “skin rash disorders,” or “tremor disorders.” Instead, illnesses are grouped by the underlying systemic causes of their symptoms, as inferred from the patient’s history, current circumstances, and description of the intensity and severity of the patient’s experience. But because psychiatry is a young science, and there are few mental health problems about which experts concur about their causes, the American Psychiatric Association has classified them by their externally observable symptoms.

The Legacy of Changes in Diagnostic Conventions

In the 1970s, there was a strong professional push to change psychiatric classification from more *inferential, dimensional, contextual* diagnosis to *descriptive* and *categorical* ways of characterizing mental suffering. Inferential diagnosis involves the assumption that a symptom is expressing something, that it has origins and meaning. Dimensional diagnosis assumes that most people have elements of any notable psychological condition, and that whether it should be considered a problem is a matter of *degree*, not *kind* (as in Harry Stack Sullivan’s famous comment, “We are all more simply human than otherwise”). Contextual diagnosis means that a person with, for example, “paranoid” tendencies who is being realistically persecuted must be understood differently from a person who has become paranoid in a benign environment. In contrast, descriptive diagnosis deliberately avoids assumptions about meaning or cause, and categorical diagnosis assumes that psychopathology is best understood in terms of distinct disorders that are either present or absent rather than in terms of a spectrum that runs from, say,

normal obsessional tendencies to obsessive-compulsive disorders, in which one infers pathology only at the extreme end of the continuum.

This “neo-Kraepelinian” movement (that is, a movement back toward the assumptions of the great 19th-century psychiatric observer, Emil Kraepelin) was the product of at least two challenges that mental health professionals were facing in the second half of the 20th century. First, they were trying to reduce the difficulties facing mental health researchers, whose work was burdened by the fact that different professional communities diagnosed somewhat differently. In the United Kingdom, for example, a certain constellation of symptoms that in the United States would be diagnosed as schizophrenia was more likely to be diagnosed as manic-depressive psychosis. Behavioral scientists were frustrated with these divergences and wanted uniformity so that their findings could be applied across societies and groups with somewhat different mental health traditions.

They were also exasperated with the assumption of many therapists that one cannot make a reliable diagnosis until a client has been in treatment for long enough that his or her psychological patterns have come to be visible *within the therapeutic relationship*. Empirical investigators wanted agreed-upon, present-versus-absent criteria that they could observe without having to undergo extensive clinical training or see patients for weeks. If they wished to study narcissistic personality disorders, for example, they wanted to be able to diagnose them without taking each possibly narcissistic individual into treatment and slowly feeling out the nature of that person’s behavior with, and emotional orientation toward, the therapist.

Second, the diagnostic syndromes described in the first two editions of the *Diagnostic and Statistical Manual* (DSM) of the American Psychiatric Association were heavily infused

with the assumptions of psychoanalysis, the prevailing explanatory psychiatric paradigm of the first half of the 20th century. By the late 1960s, there were other models explaining psychopathology, including biological psychiatry, a wide range of humanistic approaches, family systems models, behavioral analyses, and the nascent cognitive psychology movement. There was a clear need for nomenclature that did not privilege one theoretical preference over others.

With the publication in 1980 of the third edition of the DSM, these problems were addressed by a manual intended to meet the needs of researchers and nonpsychoanalytic therapists, as well as those of billing organizations, funding entities, and demographic record-keepers. It was descriptive rather than inferential and depicted discrete disorders rather than ranges and variations on themes. One of my first epiphanies about the down-side of this change occurred when in the 1990s when I was invited to speak at a well-respected psychiatric hospital. I was asked to interview, in front of the medical residents, two patients who presented confusing diagnostic pictures. Like most competent therapists, I was able to make a relationship with each individual, and in each case the patient revealed to me important information that the staff had not known. As I was leaving, I heard one resident say to another, “That’s a great line she uses. I’m going to use that line when I interview patients.” I got curious and asked him which “line” of mine he was referring to. His answer was “Can you say more about that?”!

This young doctor, a bright and highly compassionate young man, had been trained on DSM-III and was poised to ask questions such as, “Has the problem bothered you for more than two weeks or less than two weeks?” – questions for which there are yes or no answers, questions that do not invite unexpected disclosures. He knew how to fit the patient into pre-existing categories but not how to learn from the patient something about what it is like to be in that person’s shoes. Fitting an individual into a category tends to foreclose exploration of what is

unique to a patient; it especially prevents insights into unexpected aspects of a person's psychology or exploration of areas that are felt as shameful – the very areas that are of particular value in approaching psychotherapy.

Since that time, things have only gotten worse for those of us who need to understand patients in all their complexity. Students in many helping professions, not just psychiatry, are trained on the DSM as a kind of “bible” of available mental health problems and are not expected to learn how to listen with curiosity and humility to a person's idiosyncratic subjective experience. Unless one equates symptom removal with overall mental health, the DSM has no implicit concept of psychological wellness. As with any committee product, the manual is a political compromise reflecting diverse pressures and, as its authors explicitly stated until its most recent incarnation (DSM-5), was not designed to substitute for clinical inference. As many have pointed out, the current DSM is a poor reflection of the range and depth of human suffering, and its uncritical use has contributed to the over-medication and misunderstanding of many conditions.

The Unintended Consequences of Efforts at Cost Containment

There is impressive evidence that psychotherapy not only reduces psychopathology (Seligman, 1995) but also prevents or mitigates problems that come with significant long-term social costs, such as physical illness, addiction, crime, incarceration, and loss of productivity (Lazar, 2010). One would think that, in a rapidly changing world in which stress and trauma are constantly creating psychological difficulties, societies would put resources into supporting psychotherapy, that despite the expense of training and compensating skillful therapists, this investment would be seen as cost-effective in the long run.

Even if it is cost-effective over time, all the incentives to funding groups (insurance companies, governmental programs) are short-term: How much did you save us *this year*? When cost-cutters try to save taxpayers money, the first programs they tend to eliminate are those that prevent future psychopathology. The effectiveness of such programs is not immediately apparent, and long-term studies of such effectiveness are expensive to conduct. It is clearly cost-effective *in the short term* to frame mental health services as the quickest reduction of the most disabling symptoms of certain agreed-upon, isolated disorders. In the long term, we pay for this shortsightedness in immense costs for increased homelessness, crime, addiction, and other social woes.

One interesting (and, to a therapist, somewhat disconcerting) side-effect of this change toward descriptive and categorical psychiatry involves the way people in Western cultures have begun talking about themselves since the DSM-III paradigm shift. It used to be that a socially avoidant woman would come for therapy saying something like, “I’m a painfully shy person, and I need help learning how to deal better with people in social situations.” Now a person with that concern is likely to tell me that she “has” social phobia – as if an alien affliction has invaded her otherwise problem-free subjective life. People talk about themselves in acronyms oddly dissociated from their lived experience: “my OCD,” “my eating disorder,” “my bipolar.” There is an odd estrangement from one’s sense of an agentic self, including one’s own behavior, body, emotional and spiritual life, and felt suffering, and consequently one’s possibilities for solving a problem. There is a passive quality in many individuals currently seeking therapy, as if they feel that the prototype for making an important psychological change is to describe the symptoms to an expert and wait to be told what medicine to take, what exercises to do, or what self-help manual to read.

Mental health problems are listed in the DSM and similar classifications as if there is no narrative that holds together the kinds of difficulties a person reports. Experienced therapists tend to see connections between a person's "having," simultaneously, a personality disorder, a depression, an addiction, a post-traumatic symptom, and a self-harming behavior. Since we know from clinical experience that the development of a personal narrative about the connections between one's unique life experiences and one's idiosyncratic psychology is a key element of mental health – so evident in its absence from the shattered mental life of many survivors of trauma – it is not hard to view our current psychiatric nomenclature as contributing to self-fragmentation rather than providing a means to heal it.

Unintended Consequences of the Interests of Pharmaceutical Corporations

Drug companies have a natural interest in defining mental suffering in terms of discrete disorders: Once there is a "disease," a drug can be marketed to treat it. There is nothing inherently cynical about this process; this is how the pharmaceutical corporations play their role in the overall mental health scene. In their defense, it is reasonable to think of many kinds of mental suffering (e.g., bipolar disorders, PTSD) as specific illnesses. Without chemical treatment of some depressive, manic, and psychotic conditions, psychotherapy would be impossible: In the absence of medication, the patient's level of terror or self-hatred or agitation would overwhelm any other focus of attention.

Most clinicians are consequently grateful to the pharmaceutical industry for developing medications that relieve psychological suffering. Yet it is the role of therapists and patients in therapy to deal not simply with the isolated symptoms of psychological miseries but also with the larger issues that have converged in the person's presenting complaints. Whether we approach our understanding from a family systems perspective, or from a cognitive angle of

vision, or with a psychoanalytic ear, we need to make inferences that guide treatment. Why is the perspective of drug companies overtaking our capacity to look at the bigger picture?

One answer may be that viewing psychological suffering as a set of disorders that can be *fixed or improved* chemically can easily invite the obverse assumption that those painful experiences are ultimately *caused* by random or genetically based chemical differences among individuals. This is a false conclusion, of course, something like saying that because marijuana improves appetite, the cause of low appetite is lack of marijuana. But it is nevertheless a frequent leap of illogic to ascribe virtually all severe psychological suffering to a “chemical imbalance.” Such a construction tempts us to ignore all the painful other sources of psychological suffering, such as poverty, mistreatment, trauma, neglect, and the myriad ways in which human beings can injure each other psychologically.

Viewing mental struggles as afflictions that simply “happened” to us (much as one catches the flu from a random virus or develops Huntington’s disease for purely genetic reasons) exonerates ourselves and the people we love. There is no shame in an accidental or genetically explained disorder, and no one can be blamed for our suffering. There is something profoundly attractive about this, especially in the realm of problems that carry as much stigma as mental health difficulties historically have borne. But at best, it is only a partial truth to view psychological misery this way. Furthermore, it is important to note that having a more comprehensive understanding of the origins of mental distress does not equate with “blame,” as the people whose mistreatment contributed to problems in others usually did so out of the limitations of their own psychologies and not with conscious or voluntary intent to harm.

There have been many unintended negative consequences of the efforts of drug companies to define their products as “the” treatment for particular mental problems. When sweeping advertising claims were being made for the value of anti-anxiety and anti-depressant medications in the second half of the 20th century, a number of psychological researchers (see, e.g., Barlow (2011), suspecting that such conditions may respond just as quickly to psychotherapy alone, became involved in the scientific comparison of medications and talk therapies. In these investigations, despite the long tradition in academic psychology of critiquing the “medical model” of psychopathology, the researchers adopted the DSM definitions of disorders in order to test whether structured, short-term psychotherapies were as effective as the medications being touted as the treatments of choice for them.

In general, the talk therapies relieved symptoms of depression and anxiety as effectively as the medications (and without side-effects). We therapists generally appreciate these studies by our academic colleagues: They gave scientific support to what we feel we know from clinical experience (though clinical experience has also attested to wide individual differences in responsiveness to both therapy and medication; again, statistical averages show them coming out about even, but for any given individual, pharmaceutical treatments might work better, or talk therapy, or both). In the service of doing efficient, manageable research on psychotherapy versus medication, however, the scientists had to adopt the drug companies’ favored paradigm of construing mental health problems as discrete disorder categories; thus, their work inadvertently contributed to supporting an artificial, medicalized paradigm.

University professors, and then their students, began to think in those terms about the very nature of psychotherapy. Framed as discrete disorder categories, psychological suffering is much easier to research than the murkier, more vexed issues that real patients and therapists face

together, such as whether to come out as gay to one's Christian fundamentalist family, how to deal with one's infertility, or how to enjoy one's actual body in a culture that bombards us with Barbie-doll images of beauty. In other words, what is easily researchable came to define what is important to understand, in defiance of the real world of psychotherapy practice.

In the general sensibility that emerged from such interests, psychologists and social workers who had previously resisted reductionistic categories slowly joined their medical colleagues in representing mental suffering by acronyms such as BPD, OCD, ODD, and ADHD. This acceptance of the definitions favored by pharmaceutical corporations and some researchers has fostered a general assumption that individuals suffer from discrete illnesses, and that when they report more than one problematic behavior or experience, their situation is best characterized in terms of two or more "comorbid" disorders.

Sometimes, just as one could have two unrelated medical conditions such as a broken toe and colitis, one *can* have two coexisting kinds of psychological suffering that are genuinely unrelated. One might suffer, for example, from both epileptic seizures and an eating disorder. But more commonly, psychological problems are complexly interwoven and not conceptually separable. Our getting into the habit of thinking of them as distinct conditions that one "has" has distracted us from looking at what kinds of psychological capacities, integrations, and continuities characterize overall mental health, and how therapists can better help patients to develop them.

Unintended Consequences of Changes in Academia

In recent years, we have witnessed an increasing estrangement between scientific researchers and practicing therapists. Professors in universities and medical schools are

understandably frustrated when therapists do not use techniques they have studied and validated, and therapists are understandably exasperated when academics talk down to them about an art they have been practicing for years. Some amount of this tension is healthy. But contemporarily, therapists are finding the growing estrangement between practitioners and academics potentially dangerous, as our university colleagues complain to media outlets that we are not using the “best, evidence-based techniques” (approaches typically developed in laboratory settings that may or may not apply to complexly troubled individuals) or that we are ignoring “demonstrated best practices” (recommendations based on statistical averages from which any individual in the real world of clinical practice may deviate).

Such public laments have implications for practice: Insurance companies and governments have been eager to adopt the common attitude in academic psychology and medicine to the effect that there are quicker, easier, more empirically justified – and less expensive – ways of helping people than the long slog so often required in traditional therapies. They then dictate to therapists what makes sense to them to support financially, and not surprisingly, it is always the interpretation of the research that would support the least expensive outlays.

There have always been temperamental differences between academics and therapists. The motive to critique and improve existing knowledge and the motive to reduce suffering in the here and now are somewhat different callings, similar to the difference between the pure scientist and the applied scientist, the theoretical physicist or mathematician and the engineer. Researchers and therapists (as well as administrators of mental health delivery systems) also inhabit different roles, work under different pressures, and face different incentives. (Boswell, Kraus, Miller, & Lambert, 2014). But developments in academic life over the past few decades have increased the

gulf between the two groups. As someone on an academic faculty who is also in the clinical trenches, I have had more opportunity to see these changes close-up than most of my therapist colleagues.

When I was originally studying to be a therapist, most professors of clinical psychology had a fair amount of clinical experience, and often continued to maintain a therapeutic practice. These days, it would be professional suicide to do so, however, as it has become so consuming to pursue promotion and tenure in universities and medical schools that one must put all one's energy into developing research projects, pursuing grants, dealing with academic internal review boards, and writing for professional publication. A consequence of that reality is that few academics now have an experiential basis for empathy with therapists and the world of actual practice. How would they know that the problems for which most people seek treatment are not well captured by the DSM categories? How would they know that the quickest way to reduce a specific symptom is not necessarily the same thing as moving toward psychological health?

Like all of us, academic researchers generalize from the experiences that organize their lives. To do outcome studies that compare the effectiveness of different kinds of therapy or that compare talk therapy to medication, one has to take several methodological safeguards: (1) the condition to be studied must be defined as narrowly as possible so that results will not be contaminated by extrinsic factors (this is sometimes called "cherry-picking" one's research subjects); (2) there must be objective assessment of reportable symptoms at the beginning of the treatments (baseline measurements); (3) all individuals carrying out a particular approach must follow the same protocol (manualization); (4) at the end of treatment, there must be measurements of the same symptoms assessed at the beginning of the study (outcome measures). In addition, given the time limitations of grants and the pressures on professors to amass

publications, they typically need to work in the short term. These components constitute a reasonable research paradigm, and studies done this way have made valuable contributions to the practice of therapy in the world outside academia.

But when therapists find that academics expect them to (1) define problems narrowly, (2) take objective measures of symptoms at the beginning of treatment, (3) adhere closely to a manual developed for a specific disorder, and (4) judge outcome by changes in symptoms rather than improvement in overall psychological functioning, we feel we are being asked to apply a research paradigm to a clinical situation with very different properties. Taking each of these in order, let me explain why this research paradigm is a bad fit for clinical practice.

Patients may come to therapists complaining that they suffer from a particular disorder, but it rarely turns out that the initial reported disorder is the only focus of clinical attention. A man may come for “anger management,” for example, and be found to have an addiction that disinhibits his anger. The addiction is thus the most important object of intervention, not the skill-training about managing anger. A woman with an “anxiety disorder” may turn out to have a complex set of post-traumatic problems. Reducing her current anxiety symptoms will not change her ongoing maladaptive responses to her traumatic history, which must be addressed as the main issue. Objective measurement of observable symptoms is valuable in research, but in therapy, it can be the more subjective, implicit suffering that matters most. Manualization makes patients feel as if the therapist is simply following a cookbook (and in fact, there is some literature suggesting that carefully following a treatment manual is *inversely* correlated with therapeutic progress).

Finally, symptoms come and go in therapy, and sometimes get worse in the service of overall improvement. In the short term, their presence or absence, improvement or amelioration,

are often not the best indicators of important change. Individuals may test as *more* anxious when they are trying out more effective behaviors that are unfamiliar to them and feel risky; they may show *more* depressive symptoms when they are finally letting themselves grieve a loss that they have handled for years by the maladaptive defense of denial. Even within the limits of the so-called medical model, we note that no self-respecting physician would equate the removal of a fever or skin rash with the cure of the disease behind the elevated temperature or dermatitis. Nor should therapists equate symptom-reduction with overall psychological healing.

The philosopher Gilbert Ryle famously called the misapplication of the concepts of one field to the needs of another a “category mistake.” I think this has happened between academic research and the practice of therapy. I know of no clinician who thinks that psychotherapy should not be strongly influenced by research, but being *based on* research is a different matter from being *like* research.

What Price Are We Paying?

Treating all superficially similar psychological suffering as the same phenomenon, and then determining therapies for people in that category based on statistical averages, is a recipe for therapeutic malfeasance. We can define a depression by certain externally observable signs, but if we do not understand its meaning, its level of severity, or its context, we will likely fail the patient. Given differences in personality, culture, belief systems, and circumstances, one depressed person will do better with medication, another with therapy characterized by compassionate concern, another with encouragement of exercise and sleep hygiene, another with a treatment that aggressively challenges implicit self-hating beliefs. Making these distinctions is the proper role of the therapist, and research on such distinctions among patients would provide

therapists with more valuable understandings than being told what works in the psych lab with self-identified sufferers of a single disorder “not comorbid with anything else.”

A depression is an illness of sorts, but as a medical condition, it is more like a limp than a disease process. A limp is a definable, describable result of certain kinds of damage. That damage may result from many possible causes and influences, and it may have affected the leg from any one of several possible bodily directions (damage to the foot, the thigh, the hip, the shoulder). We can easily describe a limp, and we can all agree on present-versus-absent criteria for “diagnosing” it, but when we want to help someone who limps, we need to know how the distorted gait developed, how much pain is involved, what it prevents the person from doing, whether it serves some purpose we should take into account in making recommendations, whether our proper job is to help the limper to walk normally or to encourage the person’s grieving process in the face of a painful fact of life that cannot be changed.

If some of my zealous research colleagues with limited experience as therapists persuade the funders of psychotherapy treatments that all cases of DSM-diagnosed OCD should be treated by “evidence-based” exposure therapy (an excellent, effective treatment for many obsessive-compulsive problems), am I at risk of being charged with malpractice when I try to help the person who wants to work another way – even if I think this person’s individual version of obsessive symptoms warrants a different approach, and even if I have experience with reducing obsessive-compulsive symptoms through other methods? Such concerns recently prompted the American Psychological Association (see Campbell, Norcross, Vasquez, & Kaslow, 2013) to issue a press release emphasizing (1) the empirically established power of the therapy *relationship* over the type of intervention and (2) the importance of taking into consideration the patient’s values and sensibilities when planning treatment.

When we define common psychiatric symptom constellations as reified “disorders” and then assume a “one-size-fits-all” approach to psychotherapy, we are at particular risk of mistreating people in minorities. For example, some treatments developed for autistic children as a group emphasize systematically training them to make sustained eye contact. These approaches can be very helpful. But they assume a culture in which looking others in the eye is normative. In some cultures, such behavior is rude. A sensitive therapist would work in a different way with an autistic child from such a culture. If a therapist cannot adapt a treatment with respect for the patient’s context and background, the treatment is probably doomed.

It took decades for people of minority sexual orientation to persuade therapists who identified with the heterosexual majority that there is nothing pathological about their version of love. It would have taken even longer if some of the “treatments” for atypical gender expression (e.g., Rekers & Lovaas, 1974) developed by psychologists in the 1970s had been required for therapists to practice on the grounds of their being “evidence based.” They had indeed been developed via stringent methodological conditions, and they showed effectiveness at reducing “feminine” behavior in boys. But there were much bigger questions that that focus would have foreclosed, and those questions included some vision of what would make for a satisfying life for someone in a sexual minority.

There are many ways to help people. In all of them, psychotherapy is ideally a two-way process in which clinicians learn as much from their patients as vice versa. I became a therapist to try to be a healing presence for people who suffer psychologically, and to learn from them as much as I could about the immense complexity of the human condition. I like thinking of myself as a healer; I feel my patients confer upon me a sacred trust, and it is central to my self-esteem to try to justify their faith. There is nothing ennobling or sacred, however, about seeing oneself as a

technician. And for patients, there is often nothing developmentally gratifying, nothing that fosters a sense of personal growth and accomplishment, about targeting and reducing a particular symptom. It feels better not to be symptomatic, and that is no small thing. But reduction of observable, reportable symptoms such as anxiety, depression, obsession, compulsion, eating habits, and so on, is far from the whole picture of what we hope to accomplish in a meaningful therapy relationship. We hope to help people toward a more authentic, satisfying, livable life.

In the professional trenches these days, therapists are under constant pressure to redefine themselves as skillful deliverers of commodities (medications, manualized techniques) rather than as providers an overall healing relationship (within which many technical interventions may be useful). As my colleague David Mintz has noted (personal communication, August 10, 2016), we have become “mental illness professionals” rather than “mental health professionals.” The current emphasis on simply reducing the symptoms of observable, measurable suffering is obscuring the more important question of how we want to define what mental health or maturity or emotional wisdom or psychological wellness.

History of the PDM Project

Around 2003, the eminent child psychiatrist Stanley Greenspan became exasperated with what was happening in mental health treatment. He noticed, for example, that many parents who consulted him for help with their psychologically troubled children in recent years seemed uninterested in understanding the meaning of their son’s or daughter’s behavior, or in how they might help their child with the problems they were describing. They wanted simple diagnoses, such as ADD, and easily administered medications for them. He was also weary of hearing that there is no “evidence basis” for the psychoanalytic therapies. Even though very few randomized controlled trials of psychodynamic treatments had been done, he regarded the extensive literature

on personality, ego strength, defenses, attachment, neuroscience, and other areas as a strong evidence base for our work. Concluding that these disturbing changes derived partly from the impact of official taxonomies, he conceived of a new classification effort – this one directed at clinicians rather than researchers, record-keepers, and personnel involved in billing.

Dr. Greenspan then reached out to the presidents of five major psychoanalytic organizations: the International Psychoanalytical Association, the American Psychoanalytic Association, the Division of Psychoanalysis of the American Psychological Association (Division 39), the American Academy of Psychoanalysis, and the National Membership Committee on Psychoanalysis in Clinical Social Work (now the American Association for Psychoanalysis in Clinical Social Work). He solicited their support of this project, and asked them to nominate people to several task forces, including adult and child personality, mental functions, symptom syndromes, and treatment outcome research. At the same time, he prevailed upon leading psychoanalytically oriented scientists (e.g., Sidney Blatt, Reiner Dahlbender, Peter Fonagy, Falk Leichsenring, Jonathan Shedler, Howard Shevrin, Joel Weinberger, Drew Westen, Daniel Widlocher), to provide articles summarizing their research legacy so that the proposed new classification system would include incontestable empirical support.

I became involved in the original PDM project when Jaine Darwin, then the president of Division 39, suggested me for the Personality task force. Eventually, its members asked me to lead that committee and write up their section of what we began calling the “Psychodynamic Diagnostic Manual.” Once I accepted that role, I was asked to be on the overall Steering Committee as well. And as the contributions from the various task forces came in, Stanley Greenspan began to call on both me and Robert Wallerstein to copyedit the manual. So although

I was not part of the original conceptualization of the project, I got on the train at an early stop and had a good overview of the immensity of the project.

I was impressed from the beginning with the level of good will on my committee. Despite Wallerstein's private warning to me that it was comprised of some strongly opinionated individuals, and would therefore require me to operate with exquisite diplomacy, I found that all members were eager to reach common ground. One of our rare disagreements concerned whether or not to characterize personality structure as on a continuum that ran from healthy through psychotic levels of organization. Jonathan Shedler and Drew Westen felt that including a psychotic "level of severity" would confuse readers who had become accustomed to thinking of psychotic phenomena as discrete "disorder" categories, and not as a level of personality organization. In the absence of research showing that most psychoanalytic therapists endorsed the concept of a psychotic level of organization, we decided to write the continuum as going from healthy through neurotic and borderline levels to "the border with the psychoses."

As many of you know, the original PDM had sections on (1) adults, (2) children and adolescents, and (3) infancy and earliest childhood, followed by the empirical articles that Greenspan had solicited. Within the adult and child/adolescent sections, respectively, were subsections on personality structure (or emerging structure, in the case of children), mental functions (9 capacities), and symptoms (in which we took the DSM categorical diagnoses and talked about the subjective experience of each disorder in terms of cognitive, affective, somatic, and interpersonal aspects), plus some fully formulated cases. The infancy section was an extensive discussion of problems of the earliest years that included many conditions that the DSM did not address at all.

In an effort to keep costs low for students, and because his organization, the Interdisciplinary Council on Development and Learning, had the means to produce a volume in quantity, Greenspan chose self-publishing as the best means of printing and disseminating the manual. This decision did keep costs down, but an unintended negative consequence was the absence of the marketing services of an established publisher. When Greenspan became ill and died within only months of the finished product, the decision about how many volumes to produce and how to market them fell on leaders in the ICDL, who had not been part of the PDM enterprise. Fortunately, Stanley's widow, Nancy Greenspan, protected the PDM's interests, insisting, for example, that none of the profits from the volume became diverted into other areas.

Despite the limitations of the self-publishing decision and despite Greenspan's death soon after its publication, PDM-1 was quite successful. Greenspan had been able to "herd all the cats" into getting a document finished in only two years – a remarkable achievement considering the tendency of psychoanalysts to bicker among themselves and resist speaking as one community. It was reviewed positively in several major professional publications and in the New York Times. In some countries, including most notably Italy and New Zealand, the PDM became well known, while in others, such as most countries in the Far East, it was quite invisible. Overall, it sold well for a new and controversial endeavor.

Greenspan originally conceived the PDM as a preliminary document that would be improved by critiques from both clinicians and researchers. He explicitly invited commentary from the mental health community when it was first in print. Most of the feedback was positive. We did, however, get one response that embarrassed us. Daniel Plotkin, a geriatric psychiatrist in Los Angeles, wrote us to ask why, in a putatively developmentally sensitive document with sections on infancy, childhood, and adulthood, there was no attention to the elderly. The answer

was that it had never occurred to us that we needed such a section! – a comical reality and a testimony to the power of denial, given that the modal age of the members of the steering committee was in the seventies.

With Greenspan's death, I thought the PDM would die as well. Most of the PDM-1 Steering Committee members were elderly, and those under age 70 were saying they did not want to devote the rest of their career to the PDM, which had been entirely a labor of love, involving significant unpaid professional time. I was among those. I did not expect anyone to emerge who would take on the project of a revised PDM, and I was not willing to make the necessary sacrifices to do it myself. Then Vittorio Lingiardi contacted me unexpectedly, emphasizing the need for a second edition and volunteering to lead the project. I was thrilled, but not when he asked me to be the co-editor. For a while I resisted, but I was so pleased that the Greenspan's dream was not dead, and that PDM-2 would be in the hands of one of the rare researchers who is also a thoughtful psychodynamic clinician, that I let Vittorio talk me into being its "guardian angel." That meant, as you can see, that I eventually joined him in editing the document, though by far the lion's share of the vision, organization, and oversight of the volume has fallen to him (for an overview, see Lingiardi & McWilliams, 2015).

I have not regretted his talking me into that role. Overall, the project has gone smoothly. We had some conflict on a couple of the task forces, but they were eventually overcome via compromise solutions. We have not been able to get the PDM-2 out as quickly as we had hoped, but we have done reasonably well under the circumstances. Our first job was to obtain the support of all the organizations that sponsored the original PDM, which turned out to be easy. Our second was to obtain support from some additional organizations, including the International

Association for Relational Psychoanalysis and Psychotherapy, and the Italian Group for the Advancement of Psychoanalytic Diagnosis. This was also easy.

Then we sought people to head task forces. Not so easy, but again, we found many energetic, highly qualified, and hard-working individuals willing to donate their time and effort most generously. Our chapter editors include Robert Bornstein, Franco Del Corno, Francesco Gazzillo, Robert Gordon, Norka Malberg, Johanna Malone, Linda Mayes, Nick Midgley, Emanuela Mundo, John O'Neil, Daniel Plotkin, Larry Rosenberg, Jonathan Shedler, Anna Maria Speranza, Mario Speranza, and Sherwood Waldron. Although PDM-1 involved scholars from enough geographical areas to justify our description of the manual as an international effort, PDM-2 includes a greater diversity of contributors, including experts from countries as different from Western Europe and North America as Turkey and Iran.

Then we approached Guilford Publications in the United States and Raffaello Cortina Publishers in Italy. Both were immediately enthusiastic about publishing PDM-2. Although it was complicated to handle the transfer of copyright from the ICDL, and very much dependent on the good will and support of Nancy Greenspan, it was worth the difficulties: The involvement of major publishing houses guarantees that the PDM-2 will be well publicized and will not depend, as PDM-1 did, solely on word of mouth. It has been a pleasure working with Jim Nageotte of Guilford Press, whose suggestions have improved the final manuscript. Thus, perhaps the most important change from PDM-1 to PDM-2 is that it will be issued by a well-respected publishing house. In addition, there are other notable changes.

First, there are some additions. There will be five rather than three developmental phases considered in PDM-2. We have separated adolescence from childhood, on the commonsensical

grounds that the psychology of a 7-year-old child is considerably different from that of a 17-year-old. Our task force on adolescence has done a fine job with that section. Perhaps more consequentially, we accepted Daniel Plotkin's recommendation for a section on the elderly, and asked him to oversee it, along with Franco Del Corno. As a consequence of his vision and his involvement, PDM-2 will be the first major diagnostic system that explicates the special needs of the geriatric population. Thus, we will have chapters on infancy, childhood, adolescence, adulthood, and old age. In these sections we considered homotypic as well as heterotypic developmental patterns. And on the basis of both research and clinical report, we added three capacities to the sections on mental functioning, for a total of twelve, under the rubrics of cognitive and affective processes, identity and relationships, defense mechanisms and coping, and self-awareness and self-direction.

Second, in response to feedback that learning the PDM-1 system of biopsychosocial diagnosis can be intimidating to learn for those unfamiliar with psychodynamic diagnosis and case formulation, we added a section on assessment of overall personality and mental functioning. In addition to comprehensive reviews of empirically sound instruments, PDM-2 includes the Psychodiagnostic Chart, a user-friendly tool for summarizing personality organization. Originally developed by Robert Bornstein and Robert Gordon for adults, the PDC has versions for children, adolescents, and the elderly. The cases in the second edition include forms on which the patient under consideration is profiled according to the PDC. Therapists will be able to download these forms from the Guilford web site.

Third, in the Personality section, consistent with Kernberg's structural personality diagnosis as well as with longstanding clinical observation, we officially embraced a psychotic level of personality organization. In PDM-1, as I have noted, we did not include such a level on

the continuum of personality functioning because there was insufficient empirical data attesting to the clinical prevalence and usefulness of the concept of a psychotic level, and some task members felt that the term “psychotic” had become hopelessly narrowed to the symptoms of the DSM and ICD disorders of schizophrenia, psychotic bipolar illness, and schizoaffective psychosis. By the time we were developing PDM-2, however, research by Robert Gordon (e.g., 2009) had demonstrated that most practitioners – not only psychoanalytic therapists but also those of other orientations – feel that the concept of a psychotic level is supported by their clinical experience.

Fourth, we included specific comparisons and contrasts among the ICD and DSM systems, relating relevant controversies to our decisions in PDM-2. These conversations are contextualized historically and shed considerable light on what are otherwise confusing shifts and inconsistencies in the use of professional terminology. I think we all learned a lot in the context of this part of the enterprise, and we expect our readers to appreciate this element of PDM-2. The evolution of diagnostic nomenclature, and the cycles of sensitivity to some mental health issues and not to others, is an area of intellectual history that deserves more attention.

Finally, we made an omission and an integration. We eliminated the large section of articles by cutting-edge researchers, preferring instead to incorporate citations to their relevant research throughout the PDM-2. That decision allowed us to devote more pages to issues of diagnosis while continuing to honor the empirical legacy of our most important researchers.

One other note of interest about a proposed change that we eventually did *not* make: We have had informal feedback from many directions, including therapists trained in humanistic, biological, and cognitive-behavioral approaches, that the PDM has been helpful to them. At the

same time, in PDM-2, there is more attention to nonpsychoanalytic approaches, which the manual recommends for particular kinds of mental suffering. With a view to expanding the reach of the PDM classification system, we considered renaming the manual the “Psychological Diagnostic Manual,” or perhaps the “Practitioner’s Diagnostic Manual,” thus explicitly welcoming its use by nonpsychoanalytic professionals. I was enthusiastic about this proposal, but Guilford Press was not. They argued that the *Psychodynamic Diagnostic Manual* has become a “brand,” and therefore should not be retitled. We deferred to their expertise.

Concluding Comments

No diagnostic system can capture the complexity of anyone’s psychology nor the uniqueness of an individual person. But therapists, especially less seasoned clinicians, need some overall “map” of relevant psychological territory, or else they may feel helpless in the face of this infinite human variety. We hope we have provided a good-enough map of the territory of human strengths and weaknesses, health and suffering, to be more useful to the coming generation of clinicians than many of our official taxonomies of “disorders” have been.

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