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Diagnosis and Its Discontents: Reflections on Our Current Dilemma

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ABSTRACT
The author reflects on contemporary diagnostic conventions in mental health and their impact on clinical practice, framing the "neo-Kraepelinian" (descriptive and categorical) diagnostic shift that began with *DSM-III* as unintentionally destructive to effective psychotherapy. She describes the international project of creating and improving the *Psychodynamic Diagnostic Manual*, an effort to restore a clinically useful diagnostic sensibility that values inference, dimensionality, context, and meaning.

KEYWORDS
Diagnosis; nosology; personality; symptoms; PDM

Introduction

In this essay I explore the social construction of psychopathology and the unintended consequences of our currently dominant psychiatric taxonomies. In that context I review the development of both editions of the *Psychodynamic Diagnostic Manual* (PDM; Lingiardi & McWilliams, 2017; PDM Task Force, 2006), an endeavor intended to preserve clinically meaningful diagnostic practices and to bridge the gap between, on one hand, the categorical, "neo-Kraepelinian" diagnosis exemplified in the classifications of the *Diagnostic and Statistical Manual* (DSM) of the American Psychiatric Association and the *International Classification of Diseases* (ICD) and, on the other, the traditional "biopsychosocial" clinical practice of diagnostic formulation. Contributors to the PDM effort tried to preserve a diagnostic sensibility that is not simply categorical but also dimensional, inferential, contextual, and integrative.

The term "diagnosis" comes from Greek root words meaning "understanding" and "through," a combination that implies an in-depth, focused meaning-making. In that original sense, all clinicians who struggle to understand the suffering of their patients engage in a diagnostic process. Currently, putatively in the service of such efforts, therapists are pressed from many directions to match patient data to preexisting categories that have been arrived at by expert consensus. This process has some value, but as Ludwig Wittgenstein famously observed, the language in which we speak about something determines our ability to think about it. Although there are ample and erudite critiques of the categorical, descriptive, symptom-based taxonomies that predominate in our era (e.g., Frances, 2013; Sadler, 2005), clinicians all nevertheless feel the effects of being required to speak in a language that constricts how we can think and what we can imagine.

Our overall challenge in the area of clinically meaningful diagnosis seems to me to be 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 psychology and psychotherapy. Depending on someone's current situation, personality, life stage, gender experience, economic position, ethnic and racial identity, and many other factors, one person's sanity can be another person's craziness. For an overly controlled woman, to act impulsively may be an achievement,

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whereas impulsivity would be more-of-the-same pathology for her chronically out-of-control friend. A Hindu man's respect for astrology has a different meaning from an appeal to the stars for guidance by a woman who was raised as a Lutheran.

Within the lifetimes of many of us, 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 academic experts concur. Within recent memory, 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 conflicts, and sources of self-esteem, among other factors. Contemporary therapists who value case formulation also consider attachment styles, experiences of trauma, and issues of privilege and diversity when conceptualizing individuality.

Currently, therapists are expected instead to formulate patients' suffering in terms of agreed-upon, nameable "disorders." In the DSM and ICD, psychological problems have been grouped together based on whether they share certain externally observable symptoms. Rather than developing and testing hypotheses about the ultimate nature of each person's problem (the "local clinical scientist" model long advocated by George Stricker (Stricker & Trierwelier, 1995), 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 measurable symptoms that define those reified disorder categories. As with most top-down change, certain efficiencies have resulted, but at a heavy hidden price.

Psychiatry has developed differently in this respect from other areas of medicine, in which we do not find problems categorized as, for example, "fever disorders" or "skin rash disorders," or "tremor disorders." Instead, physical 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 person's experience. But because psychiatry is a young science, and there are few mental health problems for which experts agree about causes, the American Psychiatric Association has classified them by their manifest symptoms. And that poses a big problem for therapists.

The legacy of changes in diagnostic conventions

In the 1970s, there was a concerted 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" – Sullivan, 1973). Contextual diagnosis means that a person with, for example, observable "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 diagnosable obsessive-compulsive disorders, in which one infers pathology only toward the end point of a long continuum.

This "neo-Kraepelinian" movement (that is, a movement back toward the assumptions of the great 19th-century psychiatric observer, Emil Kraepelin – see, e.g., Clegg, 2012; Klerman, 1978; Spitzer, 1991) 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 observed different diagnostic practices. In the United Kingdom, for example, a 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 conventions.

Researchers were stymied also by the belief of many therapists that one cannot make a reliable diagnosis until a client has been in treatment long enough for his or her psychological patterns to have become visible *within the therapeutic relationship*. Empirical investigators wanted agreed-upon, present-versus-absent criteria that they could employ without having to undergo extensive clinical training or see patients for weeks before choosing research subjects. Some researchers reported, for example, having asked clinicians for their criteria for diagnosing narcissistic personality disorder and having been told something like "You take the person into treatment, and if a self-object transference develops, that's evidence of a narcissistic organization."

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 accounting for 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 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 both patients revealed to me vital information that the staff had not known. As I was leaving, I heard one resident remark, "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 preexisting categories but not how to learn from the patient something about what it is like to be in that person's shoes. A friend who trains psychiatric residents reports asking them questions like, "How do you understand the patient's anxiety?" and sometimes receiving puzzled looks and the "explanation" that "The patient has an anxiety disorder." The idea that one is anxious (or depressed or obsessive) *about* something that has meaning is being lost. 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 planning and carrying out psychotherapy.

Since that time, things have only gotten worse for those of us who want to understand individuals 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 asked 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 influences (see Pilecki et al., 2011) 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, long-standing evidence that traditional psychotherapy not only reduces psychopathology (e.g., Lambert et al., 2001; Morrison et al., 2003; Seligman, 1995) but also prevents or mitigates problems that create significant long-term social costs, such as physical illness, addiction, crime, incarceration, and loss of productivity (Lazar, 2010). On a sheerly rational basis, 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, however, all the incentives to funding organizations (insurance companies, governments) are short-term: How much did you save us *this year*? When cost-cutters try to save taxpayers money, among the first programs they tend to eliminate are those that prevent future psychopathology. The value of such programs is not immediately obvious, and long-term studies of effectiveness are expensive to conduct. Politically, it can be hard to sell prevention to taxpayers, especially in the United States. Even with respect to predicted physical illness, we have witnessed Donald Trump's elimination, two years before the COVID-19 outbreak, of the part of the US Center for Disease Control that concerned itself with future pandemics. We tend to spend money only on what is manifestly present rather than what is preventable – at an enormous ultimate price. 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 short-sightedness in high costs for increased homelessness, crime, addiction, and other social woes.

One interesting (and, to a therapist, somewhat disconcerting) side-effect of the 1980 change toward descriptive and categorical psychiatric diagnosis involves the ways 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 internal psychological change is to describe their 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 someone'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 and research on self-reflective function (e.g., Fonagy et al., 1991; Gabbard, 2005; Jurist & Slade, 2008; Müller et al., 2006) 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.

The assumption that complex human miseries are describable by *measurable, free-standing symptomatic entities* has been critiqued by some clinicians as reflecting a primitive, either-or mentation that one sees in young children, before they have emotionally integrated the complexity of life, or in people under stress who want to attribute blame unambivalently to a one-dimensional other. One of my crankier colleagues, a psychoanalytic theorist who is fond of Melanie Klein's (1946) differentiation between the paranoid-schizoid idea of categorically good or bad entities and the depressive appreciation of nuance and continuity, quipped that the DSM "IS the paranoid-schizoid position."

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 some kinds of mental suffering (e.g., bipolar disorders, autism) as specific conditions or illnesses. Without chemical treatment of some depressive, manic, and psychotic states, 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 reduce 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 – in the thinking of nonprofessionals and of some professionals as well – to ascribe much 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, neglect, trauma, and the myriad ways in which human beings can injure each other psychologically.

I remember when that construction began. In the 1960s, when the new neuroleptics began to be widely prescribed for psychotic symptoms, their effects could be so remarkable in reducing the psychic hell in which many patients had lived for months or even years, that mental health professionals felt the best thing they could do for individuals diagnosed with schizophrenia was to convince them that they needed to take this medication – and keep taking it interminably. A false but convenient analogy was made between antipsychotic medicines for people with schizophrenic illnesses and insulin for people with diabetes. Patients were told that they simply had a chemical imbalance that Thorazine would correct, and that if they took it regularly, their problems would be drastically mitigated, now and in the long term, just as they were for medication-compliant diabetics.

It is not surprising that this formulation became popular. Viewing mental struggles as afflictions that simply "happen" 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 appealing about this relief from culpability, 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”; most of those whose behavior has contributed to problems in others usually acted 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 Big Pharma 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 for them as the medications being touted as the treatments of choice.

In general, the talk therapies relieved symptoms of depression and anxiety as effectively as the medications (and without side-effects). Most therapists 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, whether and when to retire if our personality is defined by work, or how to accept and 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 their having 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, for example, suffer from a longstanding addiction to gambling and an acute “adjustment disorder” related to a loss. But much more commonly, a person’s 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 – the average of 2 and 10 is 6, but both numbers are quite far from 6). Some have even argued that if we are not applying a laboratory-derived treatment to a disorder for which such a treatment has been studied, then we are "practicing unethically."

Such public laments by researchers have painful 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 and prevent mental suffering are somewhat disparate 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 (see Boswell et al., 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 psychotherapists and the world of actual clinical 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; (2) there must be objective assessment of reportable symptoms when a treatment begins; (3) all individuals carrying out a particular approach must follow the same protocol; (4) at the end of treatment, one must evaluate the status of the symptoms assessed at the beginning of the study. In addition, given the time limitations of grants and the pressures on professors to amass publications, these studies typically need to be done in limited time periods. These components – cherry-picking and elimination of comorbidities, baseline measurements, manualization, outcome measures, and required termination of treatment – 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, (4) judge outcome by changes in symptoms rather than improvement in overall psychological functioning, and (5) offer only short-term psychotherapies, 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. A transgender adolescent with “depression” needs a lot more in the way of therapeutic conversation than a preformulated approach to symptoms. 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, some empirical literature suggests that carefully following a treatment manual correlates *inversely* with therapeutic progress – see Truijens et al., 2019).

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 psychological change. Individuals may test as *more* anxious when they are trying out new, more effective but unfamiliar behaviors; they may show *more* depressive symptoms when they are finally letting themselves grieve a loss that they have handled for years by maladaptive defenses such as dissociation and 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 *informed by* 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 explicitly 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 “with no comorbidities.”

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 limping person to walk normally or to encourage the individual'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?

For example, consider patients at the extreme end of the obsessive-compulsive continuum, whose obsessions border on delusional beliefs, who suffer profound annihilation anxiety, who wholeheartedly believe they will die if they fail to carry out their rituals, and who regard the therapist with suspicion for not sharing their conviction – in other words, the subgroup of obsessive patients that Kernberg (1984) would consider as psychologically organized at the low borderline or psychotic level. My experience suggests that with this group exposure therapy not only fails, it demoralizes the patients, makes them feel like failures personally, and kills any hope they may have that psychotherapy can help. It also demoralizes therapists, who have been told again and again that exposure therapy is the treatment of choice for OCD. If they believe their teachers, such clinicians can easily conclude they are simply not good enough therapists.

Laboratory studies of treatments for specific disorders typically eliminate patients at the most disturbed end of any spectrum, who almost always have "comorbidities" such as personality disorders and addictions. An influential study by Edna Foa and her colleagues, for example, excluded more than half of potential patients with PTSD because they had comorbid problems (see Huppert et al., 2002). (Patients with real-world complex trauma also tend to be less reliable and cooperative research subjects than those with mild, ego-dystonic symptoms, making them more attractive to researchers but not at all representative of the traumatized patients regularly seen by therapists.) Concerns with such disconnects between what can be easily researched and what is prescribed prompted the American Psychological Association (see Campbell et al., 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. Unfortunately, APA's position is often honored, even by APA itself, only in the breach, as exemplified by what Norcross and Wampold (2019) have understandably depicted as the "tragedy" of the recently adopted APA guidelines on the treatment of trauma.

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 minority populations. For example, some treatments developed for autistic children emphasize systematically training them to make sustained eye contact. These approaches can be very helpful. But they assume a milieu in which looking others in the eye is normative. In some cultures, such behavior is rude. A sensitive therapist would not insist on eye contact with an autistic child from a culture that construes a direct gaze as aggressive. 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 developed by psychologists in the 1970s (e.g., Rekers & Lovaas, 1974) had been required for therapists to practice on the grounds of their being "evidence-based." They had indeed been developed via stringent methodologies, 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 influence 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 does feel better not to be symptomatic, and that is no small thing. But reduction of observable, reportable symptoms such as anxiety, depression, phobia, 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 of an overall healing relationship (within which many technical interventions may be useful). As 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 mental health or maturity or emotional wisdom or psychological wellness.

The PDM project: An effort to address unintended effects of current taxonomies

The following account describes one effort to counteract some of the forces I summarized in the previous section. In case readers worry that Vittorio Lingiardi and I have a conflict of interest, let me mention at the outset that royalties from the *Psychodynamic Diagnostic Manual* do not go to editors and authors; instead, they go into a fund that supports research that clinicians may find more relevant to their work than short-term laboratory trials of treatments for DSM-defined disorders. In 2019, we set up a committee to evaluate research proposals, created a process for doing so, and sent out a call. We were able to award four grants that year, three for $15,000 and one for $8000, for PDM-related empirical studies.

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 had consulted him in the previous decade for help with their psychologically troubled children 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 witnessing. 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 by the early 2000s, he reasonably regarded the extensive empirical literature on personality, ego strength, defenses, attachment, development, neuroscience, general psychotherapy, and other areas as a strong evidence base for psychodynamic clinical work. Concluding that the social changes that were affecting his practice derived partly from the impact of official taxonomies, he spearheaded a new classification effort – this one directed at clinicians rather than researchers, record-keepers, and billing personnel.

Greenspan contacted 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, now the Society for Psychoanalysis and Psychoanalytic Psychology), 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 and asked them to nominate people for several task forces, including adult and child personality, mental functions, symptom syndromes, and 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 classification system would include incontestable empirical support.

I became involved in the original PDM project when Jaine Darwin, then President of Division 39, proposed me for its Personality task force. Eventually, I was asked to head that committee and to write up our section of what we began calling the "Psychodynamic Diagnostic Manual." Once in that role, I was asked to be on the overall Steering Committee as well. As the contributions from task forces came in, Greenspan began to call on both me and Robert Wallerstein to copyedit the submissions. So while I was not part of the original conceptualization of the project, I got on the train at an early station and had a good overview of the immensity of the project.

I was impressed from the outset with the level of good will among participants. Despite Wallerstein's private warning that my committee was comprised of strongly opinionated individuals who would require my most exquisite diplomatic skills, I found that all members were eager to reach common ground. Interestingly, one of our rare disagreements concerned whether or not to characterize personality structure as on a continuum running 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 in terms of discrete "disorder" categories rather than as a level of personality organization; Otto Kernberg, Eve Caligor, and I wanted to include a psychotic range of severity of personality disorder. 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 psychotic conditions" (PDM Task Force, 2006, p. 21).

The original PDM had sections on (1) adults, (2) children and adolescents, and (3) infancy and earliest childhood, followed by the empirical articles Greenspan had solicited. Within the adult and child/adolescent sections, respectively, were subsections on personality structure (or, in the case of children, emerging structure), mental functions (9 capacities), and symptoms. In the symptoms part, Greenspan asked us to take the DSM categorical diagnoses *as is* and note the subjective experience of each disorder in terms of cognitive, affective, somatic, and interpersonal aspects. Finally, we included in each section a few fully formulated cases, often individuals who met criteria for the same DSM diagnosis but whose problems had discrepant meanings and different treatments of choice. 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 the books in quantity, Greenspan chose self-publishing for printing and disseminating the manual. An unintended negative consequence of this decision was the loss of marketing services that would have been done by an established publisher. When Greenspan became ill and died within only months of the finished product, decisions about how many volumes to produce and how to distribute them fell on leaders of the ICDL, who had not been part of the PDM enterprise. Fortunately, Greenspan's widow, Nancy Greenspan, protected the PDM's interests, insisting, for example, that none of the profits from the volume became diverted from the intended fund for research to benefit clinicians.

Despite the downsides 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 psychoanalytic cats into getting a document finished in only two years – a remarkable accomplishment considering the tendency of analysts to bicker among themselves and resist speaking as one community. The manual 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 mostly invisible. Overall, for a new and controversial endeavor, it sold well.

Greenspan had conceived the PDM as a preliminary document to be improved by critiques from both clinicians and researchers; upon publication, he invited comments from the mental health community. 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 consideration of 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 members of the steering committee was in the seventies.

When Greenspan died, I thought the PDM would die as well. Most of the PDM-1 Steering Committee members were elderly, and those of us under age 70 did not want to devote the rest of our careers to the PDM, which had been entirely a labor of love, involving significant unpaid professional time. I did not anticipate the appearance of anyone 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, a clinician and a professor at Sapienza University in Rome, contacted me, emphasizing the need for a second edition, volunteering to lead the project, and urging me to co-edit it. Despite my misgivings, I eventually acceded to his plea. Greenspan's dream was not dead; the PDM-2 would be in the hands of one of the rare researchers who is also a seasoned clinician. The lion's share of the vision, organization, and oversight of the volume has fallen to Dr. Lingiardi (for an overview, see Lingiardi & McWilliams, 2018).

I have not regretted his talking me into that role. Overall, the project went smoothly notwithstanding some conflict on a couple of the task forces. Our first job was to obtain the support of all 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. Sponsors of PDM-2 include ten international organizations, including the original five.

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 interested in publishing PDM-2. Although it was complicated to handle the transfer of copyright from the ICDL, the good will and support of Nancy Greenspan made it possible. Jim Nageotte of Guilford supervised the manuscript with unflagging enthusiasm. The involvement of major publishing houses has meant that the PDM-2 has been well publicized and has not depended, as PDM-1 did, on word-of-mouth advertisement. This was perhaps the most consequential change from the first to the second edition. There were some other notable changes as well.

First, there are some additions. PDM-2 includes five rather than three developmental phases. We 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. We accepted Daniel Plotkin's recommendation for a section on the elderly and asked him and Franco Del Corno to oversee it. As a consequence, PDM-2 is the first major diagnostic system that explicates the special needs of the geriatric population. In the chapters on infancy, childhood, adolescence, adulthood, and old age, 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 four 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 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 (PDC), 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 can be profiled via the PDC. Therapists can 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 included a psychotic level of personality organization. As I have noted, we did not include such a level on the continuum of personality functioning in PDM-1 because there was a difference of opinion among task force members and we lacked empirical studies supporting the concept. By the time we were developing PDM-2, however, research by Robert Gordon (2009) (e.g.), had demonstrated that most practitioners – not only psychoanalytic therapists but also those of other orientations – endorse and find clinically valuable the concept of a psychotic level of functioning.

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. They shed considerable light on what are otherwise confusing shifts and inconsistences in the use of professional terminology, and they illuminate many aspects of the social construction of psychopathology. The evolution of diagnostic nomenclature, with its cycles of sensitivity to some mental health issues and not to others, is an area of intellectual history that deserves more attention. Because clinicians had responded positively to the section in PDM-1 on the patient's subjective experience of DSM diagnoses, we expanded the comments on cognitive, affective, somatic and interpersonal implications, contextualizing those issues similarly.

Finally, we made an omission and an integration. We eliminated the large section of articles by cutting-edge researchers at the end of the first edition of the PDM, preferring instead to incorporate citations to 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 clinically relevant scientists.

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, unlike the first edition, there is more attention to nonpsychoanalytic approaches, which the manual recommends for certain 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 risk feeling helpless in the face of infinite human variety. We need to be careful that the maps we are given describe the most important elements of the clinical terrain, and not just those that are useful to pharmaceutical corporations, administrators of health benefits, and a narrow group of researchers. We hope that in PDM-2, the clinical community will find a good-enough map of the territory of human strengths and weaknesses, health and suffering, that will be more useful to the coming generation of therapists and scholars than many of our official taxonomies of “disorders” have been.

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