

Is the Neo-Kraepelinian Paradigm in a Phase of Crisis?



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SUMMARY

The neo-Kraepelinian paradigm has been the dominant paradigm in psychiatry since the introduction of DSM-III in 1980. Though successful in achieving reliability and in some other respects, it also has limitations. Lately, it has been argued that a paradigm shift is needed in psychiatric diagnosis. The aim of this paper is to review evidence of the limitations of DSM and to determine whether our classification system is in a phase of crisis in the Kuhnian sense.

The most important criticisms raised since the early eighties include: the psychodynamic objection, the descriptive approach criticism, the criticism of poor diagnostic validity, and Öztürk's critique. The author concludes that these old objections are not a real threat. In particular, problems stemming from the descriptive approach and low diagnostic validity are not attributable to DSM but are due to under-achievements in psychiatry.

On the other hand, the new problems seem more serious and may be considered signs of crisis. These include high rates of false positives, high levels of pseudo-comorbidity, and other deficiencies attributed to the categorical approach. However, because of the advantages of this approach and the lack of a serious candidate available to replace it, a paradigm shift seems highly unlikely. The real source of the problem is not the categorical model but the low validity of some of our categories. If our etiopathological understanding of all the categories were to reach that of medical disorders, no paradigm shift would seem necessary.

Key words: Diagnostic and Statistical Manual of Mental Disorders, mental disorders / classification, mental disorders / diagnosis, comorbidity

INTRODUCTION

In the field of psychiatry, the eighties will be remembered as a period during which an important paradigm shift occurred. A movement (termed neo-Kraepelinian), originating in America and spreading throughout the world, altered the science and practice of psychiatry profoundly. The change was induced by the publication of the third edition of the official American Psychiatric Association classification in 1980. Diagnosis, which had been overshadowed by psychoanalysis, suddenly became the primary focus of interest. The real significance of the revolution was its emphasis of the medical model, which defined mental disorders as medical diseases (American Psychiatric Association 1980, First 2010, Sorias 2011b).

As in all revolutions, scientific revolutions are bound to create disputes. DSM-III, the symbol of the neo-Kraepelinian approach, was confronted with serious criticism and challenges from the beginning (Chodoff 1986, Faust and Miner 1986, Livesley et al. 1985, Vaillant 1984, Widiger et al. 1984). It was criticized so much that, immediately upon its publication, a committee was again established to revise and correct its suggested faults. Thus, the name of the version published only seven years after, is not DSM-IV, but DSM-III-R (DSM-III-Revised; American Psychiatric Association 1987).

Despite on-going criticism and controversy throughout the three decades that followed, the neo-Kraepelinian approach remained the dominant paradigm. DSM-III-R, published in 1987, and DSM-IV, issued in 1994, followed the same attitude (American Psychiatric Association 1994). However, over time this approach, which was welcomed with excitement

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and obtained important successes, demonstrated faults and deficiencies. The number of those suggesting that it should be modified increased with time. That the group preparing DSM-5 includes several of those who have adopted critical attitudes towards the neo-Kraepelinian approach only makes the controversy more interesting (Regier 2007).

The aim of this paper is to review the criticisms of DSM, and the responses to those criticisms, and to attempt to answer the following two questions:

- 1) Does the neo-Kraepelinian paradigm, symbolized by DSM, show symptoms of crisis in the Kuhnian sense (Kuhn 1962)?
- 2) Is there a paradigm change on the horizon?

Though it is not possible to find definitive answers to these questions, I will try to present my personal interpretation by evaluating diverse opinions and criticisms. I previously considered the emergence of this diagnostic paradigm and the DSM-III revolution in another article (Sorias 2011b). In the present paper, I will dwell on the later chapters of the story.

As stated in my previous article, although DSM is regarded as the symbol of the neo-Kraepelinian movement, ICD may be considered part of the same approach as well. Therefore, anything said for DSM also holds for ICD to a large extent.

Criticisms may be divided into old opinions expressed since 1980 and new problems arising during the use of the system.

PREVIOUS CRITICISMS DIRECTED TOWARDS DSM

1. The Psychodynamic objection

“DSM is a superficial system which tries to label, rather than understand, humans and attempts to fit the variety and richness of the human mind into a strait jacket.”

Briefly, *“DSM excludes psychodynamic understanding.”* (Chodoff 1986, Reiser 1988).

This was the criticism most commonly expressed at the outset because, until the advent of DSM-III, psychoanalytic theory was the approach most widely used to explain the etiology and symptomatology of mental disorders. The standard reply given to this objection by those preparing DSM-III was that the validity of psychoanalytic etiological explanations was not proven and not commonly accepted (Bayer and Spitzer 1985). Therefore, they decided to exclude all etiological explanations, other than those whose correctness had been proven, and to develop a simple system that addressed mental disorders merely at the descriptive level. The widespread acceptance of DSM-III in time justified this response.

The psychodynamic objection, though raised loudly at first, lost its power with time. For one, DSM was adopted

as a disease classification representing the medical model. Accordingly, the aim of DSM is to diagnose and differentiate mental illnesses, as in other branches of medicine. Psychodynamic evaluation is something beyond diagnosis. Patients with the same diagnosis may have very different psychodynamic formulations, and a person without a diagnosed illness may be considered worth studying and administering therapy from the psychoanalytic perspective. Diagnosis is based on general similarities—that which the individual patient has in common with other patients. On the other hand, psychoanalysis attempts to understand the problems unique to that individual.

As these are unrelated modes of evaluation, they can be carried out concomitantly (Frances and Cooper 1981). Making a diagnosis does not prevent us from examining the patient from a psychodynamic or other perspective. DSM-IV actually proposed an axis where those who wish to do so may write the defense mechanisms (American Psychiatric Association 1994 p.751). According to DSM, trying to understand the patient is a respectable effort. Still, a descriptive diagnosis should be made first.

The psychoanalytic approach is quite satisfactory in explaining the private side of an individual. However, a psychodynamic formulation valid for all patients with the same descriptive diagnosis could not be demonstrated. To give an oversimplified example, presenting the data of an analyzed individual as follows, *“The patient is uncomfortable around authority, because, as a child, he was afraid that his father would do something bad to him”*, may be reasonable. However, it is difficult to state that this psychodynamic mechanism plays a part in the etiology of all patients with the same diagnosis. As DSM does not claim to make a dynamic evaluation, and permits anyone who wishes to do so, this criticism does not identify a weakness.

Some of those who criticize DSM as excluding the psychodynamic approach are, in fact, criticizing the medical model. These investigators are openly against considering mental disorders as medical diseases (Wampold et al. 2001, Lerner and Lerner 2007). However, they are currently a minority in the *psychiatric* community, and the medical model is widely accepted. Indeed, broad evidence discovered by biological psychiatry in the last two decades has increasingly supported the assumption that mental disorders are medical diseases.

The psychodynamic objection lost its relevance largely with the publication of the Psychodynamic Diagnostic Manual (PDM) in 2006, prepared through the joint efforts of five large psychoanalytic associations. PDM is not a classification, but a comprehensive guide to performing a psychodynamic evaluation. The authors of PDM state that the guide aims to complement DSM and ICD (PDM Task Force 2006).

According to them, DSM is concerned with disorders while PDM is concerned with individuals (McWilliams 2011).

2. The descriptive approach criticism

“DSM diagnoses are based only on clinical symptoms. Biochemical, radiological or electrophysiological examinations used commonly in other medical branches are not present in DSM. It does not take psychological or biological etiology into consideration.” (Clark et al. 1995, Möller 2009).

This, in my opinion, is a much more serious criticism than the psychodynamic objection. Admittedly, it points to an important deficiency of our field compared to the other branches of medicine. According to Möller (2009), DSM is not a nosological classification, but a classification of syndromes. Actually, ours is the only medical specialty which does not use auxiliary tests or laboratory methods for diagnosis. Charney et al. (2002) state that: *“the field of psychiatry has thus far failed to identify a single neurobiological phenotypic marker or gene that is useful in making a diagnosis of a major psychiatric disorder or for predicting response to psychopharmacologic treatment.”*

However, this failure is not a fault of DSM, but of psychiatry itself (Adamson 1989). It is psychiatry and its branches that should develop specific diagnostic tests for each diagnostic category. In fact, psychiatry has found many biological markers for several diagnostic categories within the last three decades: Neuro-imaging techniques, biochemical tests, evoked potentials, and many others. When applied to certain diagnostic groups, a host of psychological tests also yielded results which were found to differ from those of healthy subjects. Nevertheless, specificity and sensitivity were not high enough in any of them to be used for diagnostic purposes. Therefore, none could become diagnostic criteria.

Why do we not refer patients suspected of schizophrenia to radiological examination, for example, to see whether they have ventricular dilation or “hypofrontality”? The reason is that such findings have very low specificity for schizophrenia. What neuro-radiological studies have shown is a small, albeit statistically significant, difference between schizophrenics and controls. Neither ventricular dilation nor hypofrontality is seen in all schizophrenic patients, and both also occur in many other disorders. Let's suppose that ventricular dilation was used in the diagnosis of schizophrenia. This finding would have a very low diagnostic threshold and a high variance among schizophrenic patients. Furthermore, other diseases (such as hydrocephaly or degenerative dementias) yield much higher values.

Unless biological psychiatry makes important advances and identifies tests with higher specificity and sensitivity, the rate of correct diagnoses cannot be increased regardless of the system used. If such a biological indicator is found, DSM will

add it to its criteria without waiting for the publication of a new edition.

In my opinion, those who voice this criticism confound the classification with the science. The duty of a classification is to put into order the findings of the science to which it belongs. It cannot be a substitute for the science.

3. The criticism of poor diagnostic validity

“The diagnostic validity of DSM categories is low.”
(Vaillant 1984, Malt 1986, Hyman 2010).

This is another form of the descriptive approach criticism. Diagnostic validity, put forward by Robins and Guze (1973), is perhaps the most important diagnostic concept. It signifies how close a diagnostic category is to its real/natural counterpart. It must be remembered that a diagnostic category and a mental disorder are not the same thing. A mental disorder is a natural phenomenon which we try to understand and cure, whereas a diagnostic category is our current understanding and definitions of that particular disorder.

As mental disorders are not well understood, our definitions may be deficient or, sometimes, even mistaken. The more mistakes and deficiencies there are, the less the similarity between the diagnostic category and the real form of the disorder. Diagnostic validity is the degree of similarity between the two. The higher the similarity between a diagnostic category and its corresponding natural disorder the higher its validity (Sorias 2000). This criticism suggests that DSM's definitions of mental disorders do not closely resemble their natural counterparts.

This is a correct and important assertion. For example, it is well-known that DSM's diagnostic categories do not correspond to the real phenotypes required in genetic studies (Robert and Plantikow 2005). This is the most important obstacle for genetic research in psychiatry. But, can this criticism lead to the collapse of the paradigm? The DSM revolution solved problems related to low reliability and a lack of standards. However, it did not increase validity significantly. Although some categories without validity (such as latent schizophrenia) were eliminated, and more cautious definitions of the disorders increased descriptive validity to some extent, structural and predictive validity, which are more important, remained low. Though some categories have high diagnostic validity (for instance, mental retardation related to Down syndrome or cocaine dependence), mental disorders remain the medical diseases with lowest validity.

However, again, the problem is with psychiatry, not DSM. To increase diagnostic validity, the causes and physiopathological mechanisms of mental disorders must be determined and their courses predicted; these are the tasks of psychiatry and its branches. Critics expect the classification to do what

psychiatry itself could not accomplish. The strong effect of the revolution seems to have created unrealistic expectations regarding DSM.

Therefore, poor diagnostic validity does not necessitate that the classical DSM approach be abandoned. This drawback cannot be eliminated by adopting a different classification or diagnostic system (such as the dimensional approach discussed later).

4. M. Orhan Öztürk's criticism

"[DSM] has turned psychiatry into a science of diagnosis. ...the meticulous task of recognition, definition and classification of symptoms prevails in all areas of psychiatry" (Öztürk 2001).

This is a criticism frequently voiced by Professor Öztürk and its correctness should be acknowledged. After 1980, the number of publications related to diagnostic issues and about DSM increased explosively. Kirk and Kutchins (1992) stated that, "...by 1990 over 2300 scientific articles explicitly referred to [DSM] in the title or abstract." Psychiatrists seem to have neglected issues apart from diagnosis.

I believe that psychiatry as a science has not completed the definition and classification phase yet, thus explaining the ongoing focus on diagnosis. The first step in the development of a natural science is to describe and classify its subject. Only after classification can an understanding of its nature be attempted. Mayr and Bock (2002) state that: "...classifications are needed to reduce this chaotic diversity into understandable, manageable arrangements before scientific explanations are possible." From astronomy to botany, all natural sciences have passed through this phase. But unlike the other sciences, psychiatry remains in the first stages of its development. (I do not think this is the fault of psychiatrists. The problems which psychiatry tries to solve are much more difficult and complex than those of the other sciences.) According to Frances and Egger (1999) *"We are at the epicycle stage of psychiatry where astronomy was before Copernicus and biology before Darwin. Our current descriptive system will undoubtedly be replaced by explanatory knowledge..."* Although the definition and explanation stages frequently overlap during the development of a science, definition is always the initial step. Efforts to explain without making a good definition first may yield faulty results due to the likelihood of considering the wrong object.

Although efforts to understand mental disorders have been on-going for more than a century, not much success has been obtained compared to, say, internal diseases. The lack of a single biological marker that can be used in diagnosis indicates that psychiatry has not yet passed to the stage of understanding and explanation. The deficiencies of the DSM system, the main topic of this paper, are proof that we have not even completed the definition phase.

The greatest defect of the diagnostic approach prevailing before 1980—the lack of standardized definitions for mental disorders—is proof that psychiatry had not even entered the definition phase at that time. Prior to 1980, though psychiatry knew its object of study, it had failed to define it. The neo-Kraepelinian approach accomplished this; it initiated the definition phase in psychiatry, but it could not complete it.

Öztürk will probably reply as follows:

Doesn't revealing the causes of mental disorders and elucidating their physiopathological mechanisms increase diagnostic validity? Unless, diagnostic validity is improved, even the most standardized and universally accepted definition will be nothing but an empty syndrome. A definition of the clinical picture does not suffice to define our object of interest. Characteristics other than clinical findings should be added to this definition. Therefore, the two stages should be carried out together and other issues should not be neglected on the grounds that the definition phase has not been completed yet.

Of course this is also true and is what DSM, itself, wants to accomplish. Although the definition stage is not over for the science of psychiatry, thanks to the criteria developed by DSM, it has become possible to conduct etiological investigations. On the other hand, it is difficult to trust the etiological studies conducted before 1980.

5. Other criticisms

In addition to the above, based on the existence of culture specific disorders, it has also been suggested that, because DSM is a classification developed for the American society, it cannot be applied to other cultures (Vaillant 1984). The worldwide acceptance of this classification suggests that this is not the case.

DSM acknowledges that the presentation of psychopathology may vary from culture to culture. Furthermore, DSM-IV tried to incorporate the cultural traits which influence the clinical picture of a mental disorder in its text, if not in the criteria. But, cultural characteristics are considered not the essence but factors affecting external appearance only. In other words, the effect of culture is "pathoplastic" (Jablensky and Kendell 2002).

Indeed, if mental disorders are medical-biological diseases, it is more likely that culture will influence only its external expression, not its physiopathological mechanism. Therefore, focusing on common universal traits, rather than the variations, will provide insights into the biological essence of the disorder. The aim of science is to search for universal laws rather than special cases. If each culture were to develop its own classification, a wide range of different classifications would appear. Although these classifications would faithfully

mirror the local characteristics of the clinical presentation, international communication and cooperation would be adversely influenced.

However, some behaviors described in the criteria may imply different things in different cultures. It is clear that this will render the application of DSM in other cultures more difficult. As an international classification, ICD is more careful in this respect (Üstün 2002).

Another justified criticism is that DSM is too big and complex. In my opinion, DSM-IV, which is quite large and detailed, is not suitable for use in outpatient settings (especially in Turkey) and in hospitals with heavy routines. ICD-10, developed for use by members of the WHO which includes countries whose health systems are under-developed, is shorter and simpler than DSM-IV. Therefore, we may say that ICD-10's usability is better than DSM-IV. I believe it is best to use ICD in routine patient assessment and DSM in research. Shortening and simplifying a classification may improve its usability but would decrease its scope and validity; finding a balance is hard but important.

At this point, I will stop examining the old criticisms directed towards DSM. In spite of all these objections, the neo-Kraepelinian approach was not particularly shaken and has maintained its dominance for the past three decades. However, the real faults in the system arose in unexpected places. As DSM-III and DSM-IV were used, new problems started to emerge.

THE NEW PROBLEMS

1. High prevalence and false positives

Starting in 1980, epidemiological studies using structured interviews and DSM criteria found the prevalence of psychiatric disorders in the general population to be much higher than expected (Regier et al. 1993, Kessler et al. 1994, Kessler et al. 2005). Annual prevalence rates in these studies were as high as 28% for any psychiatric disorder and 10% for major depressive episode. It was generally accepted that these rates were artificially high and did not reflect the truth (Kendell 2002). This was the first blow inflicted on the new system.

It is obvious that a sizable proportion of the people considered patients by DSM criteria are false positive cases, i.e. normal. Although the existence of this problem has been acknowledged, no complete agreement on its cause has been reached. It is possible that the rigorous interview schedules used in epidemiological studies or the interviewers (specifically, questionnaire administrators), who are not clinicians themselves, are behind the problem. But, the false positives have usually been ascribed to a low diagnostic threshold, and it has been proposed that a criterion of clinical significance be added to all categories (Regier and Narrow 2002). Upon these

recommendations, a clinical significance criterion was added to more than half of the categories in DSM-IV. However, the addition was not effective in reducing the number of false positives (Wakefield et al. 2010).

Indeed, increasing the diagnostic threshold will decrease false positives and prevalence rates. The stricter the conditions a patient must meet to get a certain diagnosis, the higher the threshold. To this end, a clinical significance criterion may be added to existing criteria, or the number of criteria required to make a diagnosis may be increased. For instance, positing six symptoms instead of five to make a diagnosis of depression would raise the threshold in the same manner. It is possible to completely eliminate false positives by setting a very high threshold.

Unfortunately, increasing the threshold has a serious side effect: false negative cases would rise (Kendler 1999). That is, the number of those inaccurately determined to be normal would increase. We must set a threshold value that minimizes both false positives and false negatives. Such an optimal threshold value can be found with a method called ROC analysis. However, this requires a gold standard—another diagnostic method which is a hundred percent accurate. Because no such method exists, those who develop criteria set the threshold value based on a combination of clinical experience, intuition, and consensus. But, the real problem that these findings indicate is not only the high rate of false positives, but also that the distribution of psychopathological symptoms in the population is not as expected (Kendell and Jablensky 2003).

No matter how we determine the diagnostic threshold, we continue to encounter high rates of false positive and false negatives. *The existence of a threshold value which minimizes both false negatives and false positives is possible only if cases and non-cases constitute two discrete groups which exclude each other to a certain extent.*

2. Multiple diagnoses

The second problem with the new paradigm regards comorbidity and is more serious. Epidemiological studies using DSM criteria found that a significant proportion of patients receive more than one diagnosis. For example, in the National Comorbidity Replication Survey (NCS-R), 45% of the patients received two or more diagnoses (Kessler et al. 2005). Patients with personality disorders most often received multiple diagnoses. In some patient groups, a single patient met the criteria for four or five personality disorders on average. In other cases, the number of personality disorders increased to six or seven.

Such a high rate of comorbidity may be considered normal to a certain degree. For example, two disorders may coexist in the same patient by chance alone. However, chance may

explain only a small proportion of multiple diagnoses, since the likelihood of the coincidental coexistence of two independent disorders is determined by multiplying their respective prevalences. This can only account for about two or three cases out of every 1000 patients.

Multiple diagnoses can also be considered normal in cases in which one disorder leads to the other. For instance, alcohol dependence may give rise to alcohol withdrawal, alcohol withdrawal delirium, or many other alcohol-related disorders.

However, the other co-morbidity cases we see in practice are disturbing. For example, the frequent co-occurrence of three anxiety disorders, such as generalized anxiety disorder, OCD, and panic disorder (Brown and Barlow 1992), suggests that these three are not distinct disorders. Similarly, when a patient is given five personality disorder diagnoses, asserting that this person has five distinct diseases is not reasonable. It is evident that this pseudo-comorbidity problem is the fault of the classification (Sorias 2011a). Pseudo-comorbidity indicates that the splitting of the categories has been carried to an extreme in DSM, and many invalid subcategories have been created (Regier 2007). It makes more sense that these frequently co-occurring syndromes are actually a single disorder. Since we do not know the etiopathological mechanisms of mental disorders, it is very likely that those syndromes, which we define as distinct disorders, are different symptoms of the same disorder (Clark et al. 1995).

There are two pieces of evidence indicating that these categories are different manifestations of the same disease:

- a) *The monoamine hypothesis of “depression” also holds true for other disorders.*

The monoamine hypothesis was originally put forward to explain the physiopathological mechanism of depression, but it turned out that it was also valid for some anxiety and eating disorders as well (Hirschfeld 2000). Both depression and some anxiety disorders benefit from medications that increase monoamine transmission. The improvement in the clinical picture with the use of serotonergic drugs indicates that the decrease in serotonin transmission is not a consequence or by product of the disease but its cause (or, at least, one part of the etiological mechanism). If the same neurochemical mechanism plays a part in the etiology of two or more disorders, it is highly probable that they are a single disorder or subtypes of the same disorder.

- b) *The number of multiple diagnoses is directly correlated to the severity of the clinical picture.*

For example, the NCS-R study found that more severe disorders had a higher number of comorbid diagnoses. On the other hand, mild depression is more likely to appear on its own (Kessler et al. 2005), suggesting that comorbid disorders are actually symptoms of depression. Since depression presents

with few symptoms when mild and with many symptoms when severe, comorbid diagnoses should be considered as the increased symptoms of severe depression (Sorias 2011a).

The same logic most likely also applies to personality disorders. When many are diagnosed together, it is more logical to view them as multiple symptoms of a single severe personality pathology, than to view them as five distinct coexisting disorders. Psychoanalytic theory has long dealt with personality disorders in this way. Just as different obsessions or hallucinations are not different disorders but different symptoms of the same disorder, the presence of many personality disorders in the same patient indicates that the person's personality pathology is severe, not that he/she has multiple disorders.

There is something dissatisfying in explaining the clinical presentation of a patient with more than one disorder. (Think of a patient with five symptoms, each of which I attribute to a different disease.) A time honored rule of medicine says:

“The principle of medical diagnosis is that all the disease-phenomena should be characterized within a single diagnosis...in any one person.” (Regier et al. 2009 quoting Jaspers).

A more general form of this principle holds in the natural sciences as well:

“The simpler the explanation of the complexity we observe, the higher the probability of its being true.”

This principle is referred to as “Occam's razor” and is attributed to William of Ockham, a 14th century philosopher. The razor cuts out unnecessary explanations (or in our case, unnecessary diagnoses).

All this evidence reveals that categories have been unnecessarily divided in the most recent DSMs. Many unnatural and invalid categories have been created based on small differences in clinical presentations. The number of categories, which was 106 in DSM-I, increased to 357 in DSM-IV. Although this increase created the impression of a more accurate understanding of mental disorders, the divisions have proven artificial.

3. Categorical approach

The third new problem with the neo-Kraepelinian paradigm is related to the categorical approach. The DSM and ICD classification systems have long considered mental disorders to be discrete categories. This classification principle, termed the categorical approach, seemed natural: If mental disorders are medical diseases, they must constitute discrete categories as in the other branches of medicine. Although initially adopted as the natural choice, the categorical approach received serious criticism over time (Kendell and Jablensky

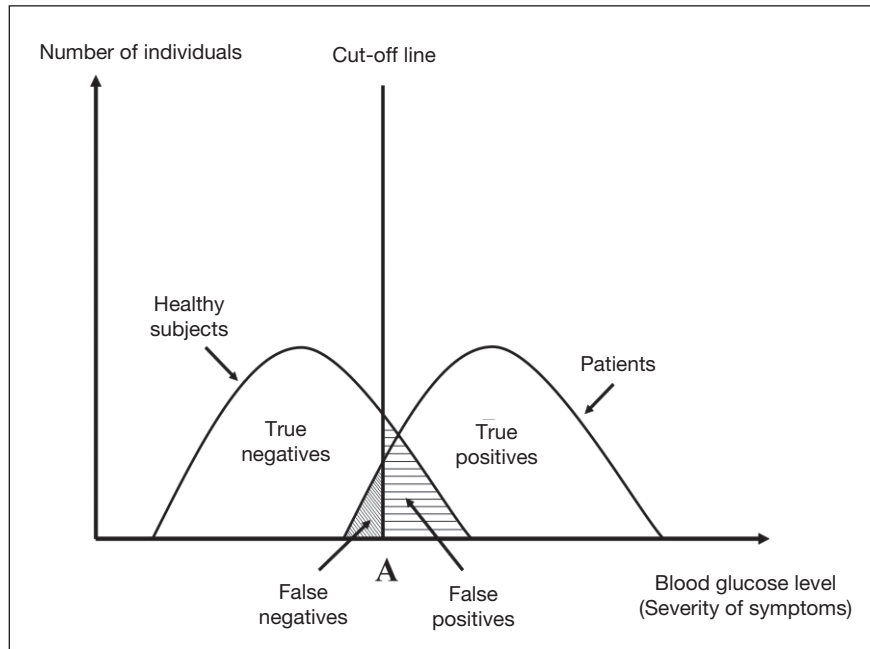


Figure 1. The distribution of the severity of symptoms for patients and healthy individuals for a given disease. For example, the distribution could represent the blood glucose levels of diabetic and non-diabetic subjects.

2003, Widiger and Samuel 2005, Luyten and Blatt 2007, Mellso et al. 2007).

3.1. Diagnostic categories have been defined as distinct entities. However, the clinical pictures we observe in practice are not. In real life, category borders are vague and blurred (Kendler 1999). The distribution of symptoms across cases and non-cases does not constitute discrete aggregations, rather a notably regular continuum (Allardyce et al. 2007). The evidence indicating that DSM diagnostic categories are not distinct entities is as follows:

- If the categorical approach were correct, the large majority of patients would be assigned to one of the existing categories with few remaining cases. However, in practice, we encounter many people who are undoubtedly ill but do not fit the criteria of any category.
- Many patients partly exhibit the characteristics of two or more categories but do not meet the full criteria of any of them (Clark et al. 1995). DSM rules require us to assign these patients to the “Not Otherwise Specified” (NOS) category, but NOS is not a natural disorder, only a “waste basket” category.
- The high rate of pseudo-comorbidity is also a sign of the extensive overlap between categories.
- Regardless of how we set the diagnostic threshold of the categories, criteria always yield high false positive and false negative rates, suggesting that the distribution curves of symptoms in patients and healthy individuals largely overlap. I will try to explain this idea with a simplified example:

Assume that the graph in Figure 1 shows the distribution of the number (or severity) of symptoms for a patient and a healthy group. We further assume that the symptoms are normally distributed. Think of the graph as the distribution of fasting blood glucose levels among diabetic and healthy individuals. First, we need a method for diagnosing diabetes apart from the blood sugar level. After splitting the subjects into healthy and diabetic groups, we can draw the graph using each group’s glucose levels. Examining the figure, we see that, although the two curves partially overlap, they are distinct, and the patient group is markedly shifted to the right. That is, the severity of the symptoms (in our example, the blood glucose levels) is markedly higher in the patient group.

We can see that the threshold value for the diagnosis of diabetes is around point A. If we draw the cut-off line near this point, we can use blood glucose levels in establishing a correct diagnosis. Although a few false negative and false positive cases will occur (the small triangular areas on the graph), these rates will be acceptable given the high rates of true positives and negatives.

By shifting the cut-off line to the right or left (i.e. by changing the threshold value), the rates of false negatives and false positives may be brought to the desired level.

However, this is possible only if the *natural distribution* of the symptoms of the disease we consider is as in the figure. Since the distribution of blood glucose levels among diabetic and normal individuals is similar to that shown in the figure, we may use glucose levels in the diagnosis of diabetes. The distribution pattern of symptoms is a natural characteristic of the disease, not something we can change.

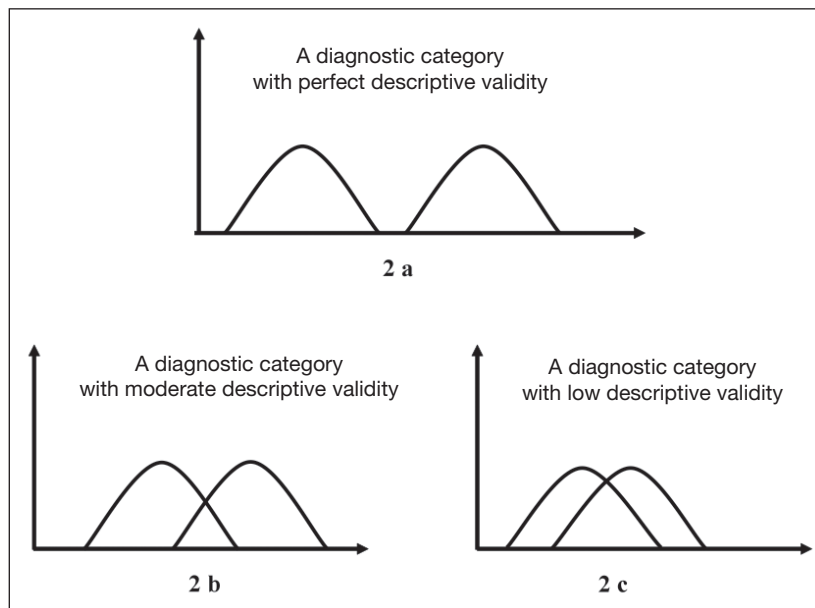


Figure 2. The descriptive validity of a diagnostic category can be defined as the degree of separation between the distribution curves of its clinical features for cases and non-cases. The distribution curves for three diagnostic categories with perfect, moderate, and poor descriptive validity are shown above.

Now we can use the example above to define descriptive validity:

The descriptive validity of a diagnostic category is the degree of separation between the distribution curves of its clinical features for cases and non-cases.

For example, in Figure 2a, the distribution curves for a diagnostic category with perfect descriptive validity are shown. Here, the two curves are completely separated, and if the cut-off line is drawn through the gap between them, specificity and sensitivity become 1.0, while false positive and false negative rates become 0.0. Distribution curves for a category with moderate descriptive validity and for a category with low descriptive validity are shown in Figure 2b and Figure 2c, respectively.

Warning: In order to draw these curves, we must be able to make a definitive diagnosis using a method that does not use the symptoms in question. Only afterwards can we see the distribution of those symptoms. For example, it is possible to make a definitive diagnosis of diabetes without looking at blood glucose level. However, for mental disorders, we only have the descriptive method, and so, these curves can only be drawn hypothetically. The actual distribution can be estimated through indirect means alone.

The high false positive rates and the inability to determine an optimal threshold value suggest that the distribution of psychopathology is continuous, i.e. does not constitute discrete groups in the general population as we expect. Consider schizophrenia, for example. In practice, we observe that the general population is not separated into one markedly ill group, displaying most symptoms of schizophrenia, and one markedly healthy group, with no symptoms. Most people display few or more psychopathological symptoms, and it is

difficult to tell for certain the point at which the disorder starts (Allardyce et al. 2007). Most probably, the distribution of psychopathological symptoms among patients and normal individuals is even worse than that shown in Figure 2c.

Some investigators have suggested that, if two categories are even slightly separated, the number of cases displaying symptoms of both disorders should be less than the number showing symptoms of only one. Such “zones of rarity” were sought using discriminant analysis but could not be found (Kendell 2002, Kendell and Jablensky 2003).

The consideration above indicates an important problem. The descriptive approach is based upon the very assumption that disease symptoms are not randomly distributed throughout the population but are instead clustered significantly among patients. However, the findings above suggest that this assumption is not correct. In other words, we rely on descriptive validity alone to establish diagnoses for mental disorders, but most of our categories do not actually have descriptive validity.

However, a point should be stressed here: the overlapping categories problem occurs mostly in subcategories and in categories with relatively low diagnostic validity (those which ranked low in the old DSM-III hierarchical order). Most of the main classes, including, organic disorders, substance use disorders, psychotic disorders, and mood disorders, stand well apart from each other. Alcohol dependence and OCD, or mental retardation and anorexia nervosa, are not even neighbors, let alone overlapping. However, the boundaries between anxiety, dissociative, eating, and personality disorders, which were formerly within the neuroses class, are more blurred (Brown and Barlow 1992).

3.2. With the categorical approach, everything is either black or white. There are no gray zones. A patient is assigned to a category only when he displays enough symptoms to reach or exceed the diagnostic threshold. Symptoms which do not reach the threshold value are not assessed. Consider a hypothetical patient who exhibits only two symptoms of manic syndrome (for instance, euphoria and insomnia) and none of the others. We would determine that the patient does not have mania. But, can this patient be considered identical to someone who has no symptoms at all? According to DSM, yes, but our diagnostic opinion is that the probability of mania in this patient is greater than zero. Even if the full syndrome is not present, we opine that the patient has “subthreshold manic syndrome”.

3.3. In the categorical approach, all patients exceeding the threshold are considered equal. After a patient receives a diagnosis, the number of criteria met is not taken into consideration. For example, two cases of depression, one which meets five criteria and another which meets nine, are attributed the same importance. Nevertheless, it is obvious that the depression of the latter patient is more severe than that of the former. This depression may even be qualitatively different.

3.4. DSM does not consider the sensitivity and specificity of the symptoms. A few exceptions apart, all diagnostic criteria for a category are considered equally relevant, but, in reality, the diagnostic significance is not the same for all symptoms. For example, DSM-IV regards fatigue and delusions of guilt as equally important symptoms of depression. However, fatigue is a symptom with very low specificity for depression and is observed in many other disorders. On the other hand, delusions of guilt are highly specific to depression. It is evident that these two symptoms do not equally contribute to the diagnosis of depression. However, the system does not take this difference into consideration.

A NEW PARADIGM?

The issues we have considered so far reveal that the neo-Kraepelinian paradigm, embodied in DSM, has some very serious flaws and deficiencies. These may be viewed as signs of crisis in the Kuhnian sense (Kuhn 1962, Soria 2011b), but a paradigm's signs of crisis are uncorrectable problems which can be solved only through the adoption of a new system. For a scientific revolution to occur, a new candidate paradigm, which could replace the old one but does not have its drawbacks, must appear. Is there such a candidate?

In the literature, a concept termed “*the dimensional approach*” is proposed as a candidate to replace the categorical approach (Widiger and Samuel 2005, Mellisop et al. 2007). The

dimensional approach attempts to make a gradual evaluation of mental disorders. It envisions psychopathology as a continuum, not as discrete categories, and places each patient on a different point on this continuum. This approach has long been advocated by psychologists.

The best way to gradually evaluate pathology is to use rating scales. In the most extreme case, if we were to completely abandon diagnostic criteria and use the Hamilton Rating Scale for Depression or the Yale-Brown Obsessive Compulsive Scale for diagnostic purposes, we would get a dimensional diagnostic evaluation. Instead of splitting subjects into two groups, those who have or do not have depression, we state that one patient's depression score is 29 and another patient's is 6. This approach does not have some of the drawbacks of the categorical approach.

Another example is the Minnesota Multiphasic Personality Inventory (MMPI). The MMPI is a typical dimensional evaluation tool. It evaluates patients using 10 different “clinical” dimensions (scales) and provides a different score for each dimension. The MMPI was developed 70 years ago to help clinical evaluation according to the Kraepelinian approach. Although the MMPI contains scales with names, such as schizophrenia, hysteria, paranoia, and hypochondriasis, they do not much relate to the diagnostic categories with the same names. Therefore, the MMPI could not become one of our diagnostic tools.

It has been suggested that the dimensional diagnostic approach is most suitable for personality disorders. Personality disorders are characterized by heterogeneous categories, significant category overlap, and a high rate of multiple diagnoses, all of which have led to the widespread belief that the categorical approach is not appropriate for their classification. Hence, the most developed proposals for dimensional diagnosis have been put forth for personality disorders (Widiger 2007). It is more sensible to view different personality disorders as different symptoms or dimensions of one personality pathology, as opposed to, as ten different diseases.

The most favorably received proposal for the dimensional evaluation of personality disorders is the five factor model (Costa and McCrae 1992). In this model, a person is evaluated on five main dimensions (termed *extraversion*, *agreeableness*, *conscientiousness*, *neuroticism*, and *openness*) and their corresponding sub dimensions. Thereby, a measure of the subject's extraversion or conscientiousness is calculated, and a personality profile is derived. As a high number of diverse profiles may be obtained, the personality of every individual may be revealed by a different test profile. Other important models include the psychobiological personality model of Cloninger, consisting of four temperament and three personality dimensions (Cloninger et al. 1993), and Millon's (1999) personality model with six dimensions.

On the other hand, dimensional approaches proposed for diagnostic categories other than personality disorders are mostly at the conceptual level. For example, van Os (2009) proposed that psychotic patients be evaluated in six dimensions, which he termed *positive*, *negative*, *manic*, *depressive*, *disorganization*, and *developmental cognitive deficit*. According to van Os, if psychotic patients, including affective and schizoaffective cases, are evaluated in this way, in-between cases (such as those between schizophrenia and bipolar disorder) can be represented.

Now to attempt an answer to our main question: Can the dimensional approach replace the categorical approach in the diagnosis of mental disorders?

In my opinion, there is no such possibility in the near future. In spite of all the problems and drawbacks to the categorical approach, it seems almost impossible to completely abandon and replace it with the dimensional, or some other, approach, for the following reasons:

1. The most important advantage of the categorical approach is that it's effortless for humans. Separating phenomena into different categories makes their recognition more facile. Eleanor Rosch (1978) called this "cognitive economy". According to Rosch, in our minds, we have a mental category for every entity we know (e.g. "*penguin*", "*Chinese*", "*tuberculosis*", or "*small*"). Mental categories allow us to quickly recall memories, compress information, and rapidly recognize objects. Our mind categorizes almost everything. Determining the category to which an object belongs is called recognition ("*this is a crow*"). Similarly, making a diagnosis is recognizing a disease. Diagnostic categories in disease classifications are very appropriate to this cognitive mechanism. Hence, the categorical approach is easier for users. The same mechanism is also responsible for automatic, or intuitive, diagnoses.

Abolishing categories is, in a sense, abolishing disease names or labels. How will we count individuals then? How will we determine the frequency or prevalence of disorders? Counting is finding the number of members belonging to a certain set. If an individual is only a partial member of a given set (for example, if a patient is considered partly manic and partly schizophrenic) counting becomes difficult, even if it is mathematically possible. While, scientifically, this is not an obstacle, we lose the intuitive simplicity with which we are familiar. It is also difficult to compare different groups without categories. Primarily for these reasons, physicians prefer the categorical approach and do not want to give it up (Luyten and Blatt 2007).

2. Categories are compatible with the rest of medicine. Thus, all medical diagnosis occurs in the same way, providing uniformity and simplicity. Classifying psychiatric

disorders differently will render our classification system, which is already quite complex, even more obscure.

3. The categorical system is familiar for clinicians. Mental disorder categories have been used for at least three decades, and users have become accustomed to them. They have even permeated into popular culture.
4. The categories are extensively investigated typologies on which a great deal of information has been collected by different schools of psychology and psychiatry. Abandoning mental disorder categories would mean abandoning all related historical research and data as well.
5. A classification change will give rise to "backward compatibility" problems. A new system should be able to execute all the functions of its predecessor and, preferably, should contain its predecessor completely. A new system should allow us to use old products and should not ask us to reproduce them. Suppose for a moment that categories are completely abolished. Set aside the difficulties of getting used to a new system. What will happen to the patients diagnosed under the old system? What about the old files, the old reports? Updating them would be an enormous task. What would be the results of the studies conforming to the old diagnostic system?
6. However useful dimensional/graded evaluation may be, it is inadequate on its own. In practice, we are frequently forced to make a categorical decision. For example,

Should this patient undergo ECT?

Should this patient be hospitalized?

Can this patient be held responsible for the crime he committed?

These questions can only be answered with "yes" or "no". Therefore, graded evaluation may be used only as an aid to categorical diagnoses.

7. Last but not least, there is no ready candidate to replace the categorical approach. The only practical models based on the dimensional approach are those developed for personality disorders. These models are quite useful in exploring personality traits and differences among normal people. For example, they are commonly used in hiring people for jobs that require certain personality characteristics. However, the clinical utility of these models has not been demonstrated yet (First 2005), and how they will be integrated into the psychiatric interview and examination is still unknown. Even the five factor model, which has been studied extensively and applied to different fields, ranging from sports psychology to counseling psychology, could not be adapted to the practice of psychiatry (Rottman et al. 2009).

For categories other than personality disorders, no tried dimensional model exists to replace the existing approach. Current proposals are all at the conceptual, or even speculative, level (Clark et al. 1995, Vieta and Phillips 2007, van Os 2009). It is doubtful whether they can be implemented. Will the reliability of dimensional diagnoses be better than that of standardized categorical diagnoses? Will the diagnostic validity be higher? *Dimensional models are also descriptive. They use a different approach to describe the clinical picture, but they are not etiological.* We don't yet know whether dimensional models will reflect the underlying pathology more accurately than categories. For example, a diagnosis stating that a certain patient is partly manic and partly schizophrenic may better represent the observed clinical picture. Still, it remains uncertain whether this model, which has yet to be tried, will correspond more closely to the etiopathological truth.

CONCLUSION

We generally think that, in using the descriptive approach, we base our diagnoses on observable clinical symptoms, without making use of laboratory tests. In fact, the situation is worse in psychiatry. The problem with the descriptive method is that, in psychiatry, the relationship between symptoms of mental disorders and the underlying (but unknown) physiopathological mechanisms is not straightforward. For example, in neurology, a clear and direct relationship exists between the clinical symptoms of a hemiplegic patient and the pathology causing those symptoms. The locus and mechanism responsible for power loss and reflex changes are well known. Therefore, in neurology, we can pinpoint the underlying pathology much more precisely by looking at small changes in the clinical picture. On the other hand, the symptoms of mental disorders are indirect consequences of the underlying pathology which their shape has changed many times along the way. Let's stop for a moment and assume that there is no auxiliary diagnostic method in neurology and that neurologists base their diagnoses on clinical symptoms only. The validity of the diagnostic categories defined by such a "descriptive neurology" would be much higher than that of the categories of DSM-IV. In other words, the specificity and sensitivity of symptoms is much lower in psychiatry than in the rest of medicine. There is no reason to accept that dimensional diagnoses, which would be based on the same symptoms, would more closely reflect reality.

It is my contention that descriptive psychiatry has reached the limits of what it can accomplish. I do not believe that more natural disease definitions can be obtained by redistributing psychopathological symptoms to discrete categories or by spreading them over a few continuous dimensions. A similar belief was expressed by Hyman (2010). Furthermore, we are still far from achieving an etiological approach based

on cause, instead of clinical appearance (Luyten and Blatt 2007, First 2010). That same lack of etiological insight forced DSM-III to be atheoretical.

It seems dubious that a dimensional model will ever completely replace categories. For that reason, even proponents of the dimensional approach advocate a graded categorical approach (Allardyce et al. 2007, Mellsop et al. 2007, Vieta and Phillips 2007). In this approach, classical categories are preserved, but in addition, the severity of the pathology is integrated into the diagnosis using rating scales. Mental retardation has long been an example of such a graded category. Proponents argue that other diagnostic categories, especially anxiety, sleep, substance abuse, and mood disorders, should be evaluated similarly to mental retardation (Widiger and Samuel 2005). By using graded categories, the above mentioned advantages of the categorical approach can be maintained, while some of its unfavorable characteristics may be eliminated. For example, no longer will every patient who exceeds (or remains below) the threshold have to be placed in the same basket. DSM-5 will supposedly adopt this approach (Regier et al. 2009).

In my opinion, the problem with the present system is not the categorical approach but the low diagnostic validity of some categories. Again, validity varies for different categories (Sorias 2000, Sorias 2011a). The natures of many disorders are well understood, and hence, have high diagnostic validity. "Dementia of the Alzheimer's type", "Alcohol Withdrawal Delirium", and "Mental Retardation due to Down Syndrome" are mental disorder categories which we use without complaint. Because the natures of these disorders are understood relatively well, their differential diagnosis is easy, and they are not associated with false comorbidities. In other words, their diagnostic validity is high. Similarly the other branches of medicine do not perceive any problems with the categorical approach.

The problem arises only in categories with low diagnostic validity. Those occupying the lower ranks of the old DSM-III hierarchical order seem to reflect all the aforementioned faults of the categorical approach. This is especially true for personality disorders and for the categories and sub categories previously classified as part of neuroses. The natures of these disorders are not well understood, and it seems unlikely that each of them is a different natural disease. Discarding those with low validity and decreasing the number of categories by combining them, as proposed recently by some authors (Löwe et al. 2008, Andrews et al. 2009), may alleviate some of the problems with the categorical approach.

However, the real solution lies in augmenting the validity of all the categories. What we have now is all which is possible with the descriptive method. We must stop playing with clinical symptoms and place emphasis on etiopathological

research. If our etiopathological understanding of all the categories were to reach a level similar to that of medical disorders, no paradigm shift would seem necessary.

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