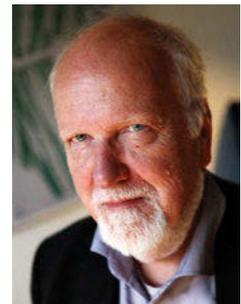


The Paradox of Atheoretical Classification

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Abstract: A distinction can be made between "artificial classifications" and "natural classifications," where artificial classifications may adequately serve some limited purposes, but natural classifications are overall most fruitful by allowing inference and thus many different purposes. There is strong support for the view that a natural classification should be based on a theory (and, of course, that the most fruitful theory provides the most fruitful classification). Nevertheless, atheoretical (or "descriptive") classifications are often produced. Paradoxically, atheoretical classifications may be very successful. The best example of a successful "atheoretical" classification is probably the prestigious *Diagnostic and Statistical Manual of Mental Disorders (DSM)* since its third edition from 1980. Based on such successes one may ask: Should the claim that classifications ideally are natural and theory-based be reconsidered? This paper argues that the seemingly success of atheoretical classifications hides deeper problems and that the ideal of theory-based classification should be maintained.

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1.0 Introduction

Hope Olson realized that information science and knowledge organization should be based on theory. Two examples from her rich production are: 1) Olson (2001) describes how the dichotomous principles of western philosophical heritage create classification systems, which privilege the mainstream, majority view; and 2) Olson (2002) brings a critical feminist perspective to key issues in knowledge organization. The title of her book, *The Power to Name*, is in itself a powerful expression of an extremely important theoretical principle: the assignment of a subject to a document is not a neutral act but is a policy act contributing to facilitate certain uses of that document at the expense of other uses. The importance of this theoretical principle—and of theory in general—cannot be overestimated. (See more about Olson's production and its relation to theory in Fox 2015). Many philosophers have recognized the importance of theory for classification; David Hull (1998), for example, wrote: "The fundamental elements of any classification are its

theoretical commitments, basic units and the criteria for ordering these basic units into a classification."

Also in my own writings, this view has often been highlighted (e.g., Hjørland 2008 and 2013). For example, I wrote (2008, 335):

In every domain different theories, approaches, interests and "paradigms" exist, which also tend to describe and classify the objects according to their views and goals. For example, psychoanalysis and biological psychiatry disagree on how mental illness should be classified and they disagree about the value of a particular classification scheme such as the DSMIV.

The claim here is that we have many approaches and theories of knowledge organization and among them some claim that they are not theories: they claim that classification can be or should be atheoretical. This is, of course, a view that should be examined as should any other view or theory. If we want to defend any theory (and the teaching

of theories), we have first of all to defend ourselves against atheoretical research and atheoretical classification. In this paper, the focus is atheoretical classification. This is however, related to the broader issue of atheoretical research (and atheoretical practice, including atheoretical library and information practices—the generic concept being atheoretical activities). Our task can also be understood as “to uncover the hidden theoretical assumptions in activities, which claim to be atheoretical” (cf., Slife and Williams 1995). Some researchers do recommend atheoretical research (e.g. Cole 1996), while others consider it a false category. Economist Michael P. Keane (2010), for example, wrote: “All econometric work relies heavily on a priori assumptions. The main difference between structural and experimental (or ‘atheoretic’) approaches is not in the number of assumptions but the extent to which they are made explicit.”

Windschitl (2004) considered atheoretical research as a kind of “folk theories of ‘inquiry.’” In this paper, I shall not go into this broader discussion but will focus on atheoretical classification, while the broader issues concerning other forms of atheoretical activity are only superfluously covered and reserved for a future paper.

2.0 The paradox of atheoretical positions

In Hjørland (2015) I defined the concept “theory” as a statement or a conception, which is considered open to questioning and which is connected with background assumptions (i.e., theory is the way something is considered). Theories form interconnected systems of grand, middle rank and micro theories and actions; concepts, observations, practices and artifacts are theory-laden.

The idea of atheoretical research, practice and classification is related to the positivist dichotomy between observational and theoretical statements: What we can see (e.g., a hammer or a blackbird) are here considered “given,” not theory or theory-laden. Wilfrid Sellars (1956), among many others, criticized the view that perceptions of sense data give immediate knowledge that can serve as the foundation of empirical knowledge. He called this “the myth of the given.” The opposite of this myth is known from many philosophical positions such as historicism, pragmatism, paradigm theory, hermeneutics and activity theory, which all claim that human perception is “mediated” by language and other culturally developed sign systems. The positivist dichotomy between observational and theoretical statements came into crisis (see Putnam 1962) as did the logical positivist program in general in the aftermath of Kuhn’s (1962) *The Structure of Scientific Revolutions*. When a clear distinction between observations and theories cannot be obtained, it follows that all human actions are theoretically laden and the very idea of atheoretical action becomes an

oxymoron (as does the concept of “raw data,” cf., Gitelman 2013). According to Novick (1988), historical science considered itself objective from its positivist foundation about 1880 until, for example, black people and women entered scholarship and put forward alternative views on history. This example may demonstrate a general principle: the only way something can be revealed as theoretically laden is by developing an alternative view or theory (e.g., by introducing people with other cultural perspectives or other disciplinary backgrounds, or engaged in broader philosophical studies). Very often, things look for a very long time as “given” or “atheoretical” until a new understanding reveals their theoretical nature.

There is, however, an important difference between an abstract claim that something is theoretically loaded and a specific analysis of “how” this thing is theoretically loaded. It is problematic, for example, to claim that all research done by men is wrong just because it is done by men. Some men have contributed to feminist theory or to knowledge acknowledged by feminist epistemology. Therefore, claims of theoretical “bias” should be specified, not just held as a generalized principle.

The methodological implications should be obvious: researchers who possess broad background knowledge and know alternative views should be better equipped to evaluate those theories and to do better research and to better classify things. They are less vulnerable to the mistake of “naïve realism” or choosing a problematic theoretical basis. Based on such knowledge of different perspectives, researchers and classifiers may negotiate different theoretical views and choose the optimal solution or the best compromise. From this perspective, atheoretical positions must be considered less advanced. However, as we shall see in the following section, atheoretical positions may be extremely successful. It is a paradox that solutions that are more primitive are more successful compared to more advanced solutions. How can this be the case? For answering this question, it is important to consider the extraordinary success of an “atheoretical” classification. Here the *DSM-III* will be used as an example, but it is certainly not an exception. (*DSM* has formerly been investigated in knowledge organization and information science, by, e.g. Bowker and Star 1999; Fujigaki 2006; and Spasser 1998).

3.0 The DSM classification

Diagnostic and Statistical Manual of Mental Disorders (DSM) is a classification system for mental disorders published by the American Psychiatric Association (APA). Today, it is extremely influential—also internationally—and by far the most important diagnostic tool in its domain. This was, however, not the case with the first edition, which was published in 1952 (APA 1952). The following editions are

the second edition, *DSM-II* (APA 1968), the third edition, *DSM-III* (APA 1980), the fourth edition, *DSM-IV-TR* (APA 2000) and the fifth edition, *DSM-5* (APA 2013). The first two editions were relatively theoretically based, but with *DSM-III*, the atheoretical principle took over, which seems particularly paradoxical because American psychiatry at that time was strongly influenced by biologically oriented theory (Demazeux and Singy 2015, xiv-xv):

Given this general evolution of American psychiatry, the history of the DSM is paradoxical. The third edition (*DSM-III*), published in 1980, constitutes according to historians a true nosological revolution. But this revolution did not consist in having fully embraced neurological or genetic factors, as we might have expected given the general evolution of psychiatry, and especially of American psychiatry. Rather, it consisted in remaining, or trying to remain, atheoretical. The *DSM-III* offered a classification that voluntarily ignored the etiological models of mental disorders, in order to focus instead on the task of providing unambiguous descriptions of these disorders by means of precise and exhaustive diagnostic criteria. It is often said that the *DSM-III* broke with psychoanalysis, which was dominant in large American cities. It is less often said that it also broke with the essentially biological direction of its predecessors (while the influence of psychoanalysis on the *DSM-I* and *DSM-II* is regularly stressed by historians, we should not forget that the first two editions of the DSM gave an important role to biology, as illustrated by the key category of “Organic Brain syndrome”).

Despite the atheoretical nature of *DSM-III* and following editions (or because of it?) it became an extreme success (Demazeux and Singy 2015, xv-xvi):

There is another paradox in the history of the DSM: its [*DSM-III*’s] extraordinary success. While the *DSM-III* did not offer any theoretical innovation, but only a stupendous methodological audacity, its impact on contemporary psychiatric discourse and practice has been considerable. The first printing of the *DSM-III*, in 1980, was quickly sold out. It was soon translated into many languages...The successive editions, the *DSM-III-R* (1987), the *DSM-IV* (1994), and the *DSM-IV-TR* (2000), only reinforced the world domination of the DSM. The DSM quickly supplanted most national classifications, wiped them from memory, and contributed to an unprecedented homogenization and universalization in the practice of psychiatry. It even influenced the

WHO’s ICD [International Classification of Diseases], which, from 1992 on, came to adopt a structure and methodological principles that are very similar to the DSM’s Without a doubt, today no psychiatrist in the world is unaware of the meaning of the acronym “DSM,” even when many clinicians continue to resist the imperative to use this manual. Most certainly, all scientifically ambitious researchers in psychiatry must refer to the DSM, however unsatisfied they might be with the manual. Today, with the publication of the *DSM-5* in 2013, and despite all the criticisms and complaints directed against this new edition even before it was published (and often formulated by American psychiatrists themselves), the hegemony of the American system remains intact.

How can this success be explained? It seems also paradoxically because, as Poland’s (2015) analysis demonstrates, overall *DSM*-based research has not yielded any solid result and *DSM* provides researchers with decontextualized diagnoses that are symptom-based, atheoretical, polytheist, and not associated with well-confirmed tests and models. Rather, *DSM* has a flawed epistemological structure and categories provide “artificial groupings” of individuals experiencing mental illness.

What is stated here is that the success of *DSM-III* is not based on solid research and that the classes in this system are not associated with research-based diagnostic tests or procedures. Therefore, its success seems paradoxical. Demazeux and Singy (2015) also write, however, that much criticism was raised against the new *DSM-5*. Thus, its success was perhaps temporary, and psychiatric diagnosis—and thus psychiatry as a whole—seems to be in a serious crisis, very different from the general pattern of progress in other subfields of medicine (see, e.g., Frances 2013; Greenberg 2013; Maj 2015; Paris and Phillips 2013; and Zachar et al. 2014). Nonetheless, the atheoretical principle of *DSM-III* was considered extremely successful, even a revolution (at least for a period), which is a fact that calls for an explanation.

If atheoretical action is an oxymoron, as argued above, it follows that *DSM-III* cannot be atheoretical, that there must be hidden theoretical assumptions in this system in spite of its claim of the opposite. The two following quotes provides an analysis of this issue (Whooley and Horwitz 2013, 79):

The revisions to the *DSM-III* sought to increase reliability through moving psychiatry away from the fluid psychoanalytic understanding of mental illness toward a standardized nosology of fixed disease categories. They overthrew the broad, continuous,

and vague concepts of dynamic psychiatry and replaced them with a discrete system of classification that treated mental disorders as discrete diseases. This nosology rigorously segregated the pathological from the normal, in a way that the previous psychodynamic model never did.

While the DSM-III revisions were advertised as agnostic toward different theoretical schools of psychiatry [APA 1980], the entire endeavor—delineating discrete disease categories to facilitate diagnostic consistency—implied an endorsement of the biomedical model. The revisions were sold as ways to improve treatment through empirically based research programs and targeted diagnoses. The new paradigm of diagnostic psychiatry organized symptoms into discrete disease entities with the expectation that the organic bases of these entities would soon be discovered [Horwitz 2002]. In other words, the revisions to the DSM were a strategy to attain a biomedical model by understanding illnesses as stable entities that can be explained in terms of specific causal mechanisms located in the brain. The hope was that the identification of the elusive biological or genetic markers for mental disorders would follow from the standardized classification system. DSM-III promised a future when specific etiologies were discovered for specific disorders and, in turn, specific treatments would emerge.

Whooley and Horwitz (2013) thus argue that *DSM-III* was indeed based on a (meta)theoretical view, the view of biological psychiatry as opposed to, among others, psychodynamic and cultural views. A narrow biological view of mental illness is opposed to a rather broadly recognized view that mental illness should be understood from a combined biological, psychological and social perspective. For those, who believe that a form of biopsychosocial model provides the best scientific explanation of mental illness, the adaption of the strict biological view underlying *DSM-III* does not provide scientific advance, but on the contrary: it represents a reductionist scientific ideology. This ideology was extremely successful by strengthening the professional powers of psychiatrists at the expense of other professions involved in mental health, in standardizing psychiatry and gaining support from powerful institutions such as health assurance and the pharmaceutical industry. This is the basic explanation of the success of *DSM-III* as described by Whooley and Horwitz (2013). However, because this ideology is not scientifically based—but based on anticipation, the expectation that the organic bases of these entities would soon be discovered—the system fails because the expected findings did

not turn up and thus it has severe problems reflecting a natural classification of mental illness. Therefore, the system is today in a serious validity crisis as revealed by a growing critical literature. The issue about the biological basis of mental diseases has not been settled but remains highly controversial. The point here is that different theories should be developed, brought into a dialogue and negotiated. The disregarding of other theories without proper basis in research is ideological rather than scientific. The atheoretical attitude contributes to such an ideology. Also Foerstl and Hoff (2009, 57) argued against atheoretical classification of psychiatric disorders and wrote:

Wir sollten auf dem Boden einer aufgewerteten Psychopathologie die unfruchtbare Polarisierung „theoriereich“ vs. „theoriearm“/„theoriefrei“ hinter uns lassen Was wir brauchen in der Psychiatrie, ist keine „Theoriephobie,“ sondern reflektierte und damit undogmatische Theorie.

Thus, our analysis so far has shown that the success of the “atheoretical” *DSM-III* was based on:

- 1) That it was not atheoretical as it claimed to be. Rather, it was supporting a reductionist biomedical theory of mental illness (although only in a very general way). This is the hidden theoretical assumption that Whooley and Horwitz (2013) uncovered.
- 2) Its contribution to the homogenization and universalization of the practice of psychiatry and to the power of the psychiatric profession. This was obtained by the emphasis on reliability at the expense of validity as we shall consider in a following section.

4.0 Artificial versus natural classification

Poland (2015) characterized *DSM*'s categories as “artificial groupings.” The distinction between artificial and natural classification is important. An artificial classification may be useful for limited purposes. For example, we often organize documents and document representation in artificial ways (e.g. in alphabetical order), which for many purposes have proven to be very practical. However, in order to retrieve things of a specific natural kind, those things have to be identified, named and classified, which presupposes a subject approach and natural classification.

William Perry and Edward Hacker wrote (1991, 133):

For example, one may divide rocks—or even animals—into those weighing less than ten grams, those weighing at least ten but less than twenty grams, and so on; but this is likely to be of little

use, except perhaps for knowing what it would cost to mail them.

And later:

[A classification] is fruitful to the extent that it suggests new hypothesis, explanations, and theories concerning its subject matter. For example, the periodic table—the classification of the elements—proved extremely fruitful, since it suggested the existence of hitherto unknown elements and even suggested what physical properties they would have. It should be noted that natural classifications, by definition, are more fruitful than artificial ones.

To make an extreme case, one could classify all mental patients according to their weight, which would be “an artificial grouping” that would not be helpful for treatment decisions (but it would be highly objective and reliable). “A natural classification” of mental patients, on the other hand, would be one that could be helpful predicting which patients would benefit from a particular treatment. Such a natural classification should be based on theory and knowledge of mental diseases. This example is extreme, of course, but theoretical clarity is often obtained by considering such “pure” examples. This clarity can thereafter also be applied to less extreme examples.

5.0 Reliability versus validity of classifications

The issue concerning artificial versus natural classification is related to the problem of “reliability” versus “validity” of classifications. “Reliability” means that different persons report the same result, (i.e., obtain consistency), corresponding to the concept known as “inter-indexer consistency” in knowledge organization (see Lancaster 2003, 68–82). “Validity,” on the other hand, means that a description, a measurement or a representation is adequate in relation to its purpose. Often there is a high prestige for professionals connected to reliability (the same is also, of course, expected for validity, but professionals in disagreement are easily revealed, and low reliability is in itself a sign of low validity, therefore there is a tendency to make priority to reliability). Probably the most important issue in the success of *DSM-III* was that it made psychiatric diagnosis much more reliable—thereby increasing the status of psychiatrists (this has been the overall opinion, but Kirk and Kutchins (1992) nonetheless found *DSM-III* to be highly unreliable). High reliability was the top priority for the designers of that system and this importance is reinforced by the demand from, in particular, (American) insurance companies to make medical diagnoses reliable. Philips (2014, 164) states:

The quest for this Holy Grail began back in 1980 with *DSM-III* (American Psychiatric Association 1980). The immediate goal of that manual was to achieve the first step in a scientific nosology—diagnostic reliability—with the use of operational definitions and diagnostic criteria. With *DSM-III* we could be confident that clinicians and researchers in different countries would be talking about the same phenomenon when they discussed, for instance, schizophrenia. Built into the *DSM-III* process, however, was the understanding that reliable diagnoses could not yet claim to be valid; we could *not* feel comfortable that the diagnostic concept in question represented a distinct, real entity in the world. How did we know, for instance, whether the diagnostic entity called schizophrenia described one distinct illness or several? In that way, the accomplishment of *DSM-III* immediately unleashed a new anxiety and a new goal—securing diagnostic validity.

In prioritizing reliability over validity, the architects of *DSM-III* assumed that ongoing research would lead to valid diagnostic constructs.

However, let us return to the previous thought experiment: if psychiatrists classify their patients according to, for example, their weight, they would be able to provide extremely reliable groupings, but would not create categories that would be helpful in determining treatments and predicting recovery. *DSM-III* of course, used more valid criteria than the patients’ weight, but the question of lack of validity of psychiatric diagnosis is today very pressing and the word “crisis” is probably not too strong a term to characterize the situation. A given mental disorder may be defined by a combination of symptoms. “Schizophrenia,” for example, is mostly characterized (Zielasek and Gaebel 2015, 9) by a combination of symptoms including delusions, hallucinations and disorganized thinking with disease onset in early adulthood. Any such set of symptoms may be labeled schizophrenia as an artificial definition and classification. However, the claim that these symptoms correspond to one and only one natural kind is a scientific hypothesis that has to be proven. Some scientific classifications (e.g., bird classifications) have today a solid scientific basis (about the recent progress in classification of birds, see Fjeldsø 2013). No corresponding solid scientific basis supports our present classification of psychiatric diagnoses and the important point is that the categories in a system like *DSM* have to be considered scientific hypotheses. Their revision should therefore be based on considering the supporting evidence as well as the evidence supporting alternative categories. Again, we may conclude that it is paradoxical not to consider these categories as theories. What kind of decision has produced these categories? Of

course many years of psychiatric research, practice and experience has gone into this. But how are the specific decisions in a given edition of the *DSM* made? The *DSM-III* Task Force wrote (APA 1980, 3):

In attempting to resolve various diagnostic issues, the Task Force relied, as much as possible, on research evidence relevant to various kinds of diagnostic validity. For example, when discussing a problematic diagnostic category, the Task Force considered how the disorder, if defined as proposed, provided information relevant to treatment planning, course, and familial pattern. It should come as no surprise to the reader that even when data were available from relevant research studies, Task Force members often differed in their interpretations of the findings.

This general method has comically been summarized as the BOGSAT method: “A bunch of guys sitting around a table” (Kendler and Parnas 2012, 141). It should be considered that in evidence based medicine (EBM), evidence from expert committee reports or opinions and/or clinical experience of respected authorities is considered a very low criterion of evidence, (cf. Hjørland 2011, 1302). There is much criticism about the *DSM* system and its emphasis on reliability and a quote from the book *Philosophical Issues in Psychiatry* (Parnas and Sass 2008, 271) reads: “Excessive and, in our view, epistemologically naïve focus on the issues of reliability created serious problems concerning the more fundamental level of validity.”

Such an excessive focus on reliability at the expense of validity is connected to positivist currents and naïve realism. While consistency has normally been regarded as an ideal also in knowledge organization, Cooper (1969) provided some important reservations in relation to this ideal. He demonstrates that indexers that are most consistent with each other do not necessarily produce the best work and that indexing “can be consistently bad.” While this might seem strange, it is understandable if, for example, indexers have been taught bad principles. Superficial indexing may, for example, rely too much on titles of the documents being indexed, which tends to produce consistent but bad indexing. The same is of course the case about “indexing” psychiatric patients into diagnostic classes. The principle “reliability before validity” is thus a problematic principle connected to the ideal of atheoretical classification.

6.0 “Descriptive” versus “theoretical” classifications

DSM-III and later editions (see Tsou 2015a) are said to be “descriptive” rather than theoretically committed. How-

ever, as Gregory Bateson wrote (1977, 147), it is problematic to consider “descriptions” an atheoretical activity:

You can never get away from theories of the nature of description whenever, wherever you have descriptions. All descriptions are based on theories of how to make descriptions. You cannot claim to have no epistemology. Those who so claim have nothing but a bad epistemology. And every description is based upon, and contains implicitly, a theory of how to describe.

(Unfortunately, this brief but important statement is not much unfolded in the source from which it is here quoted).

Indeed, since Kuhn’s (1962) *Structure of Scientific Revolutions*, the notion of the theory-laden nature of observations (and by implication the theory-laden nature of descriptions) has become a fundamental assumption in the philosophy of science. By implication, the distinction between “descriptive” and “theoretical” classifications is problematic—as already indicated with Sellars’ (1956) notion “the myth of the given” mentioned above. (This recognition of the theoretical ladenness of descriptions may have further important implications for the use of this term in knowledge organization, e.g., in the concepts “descriptive bibliography” and “descriptive cataloging”).

In order to understand the atheoretical approach in *DSM-III*, we shall look at what is here meant by “descriptive” versus “theory-based” classification. It is well known that psychology and psychiatry have been split in many conflicting views or schools, e.g., psychoanalysis, cultural psychology, behaviorism, cognitivism, neuroscience. Such views tend to develop their own conceptual systems and classifications. “Neurosis,” for example, is a psychoanalytic concept, “organic brain syndrome” is a concept associated with neuroscience. Although different theories may have their own domains for which they provide good explanations, they also tend to provide opposed views on the same phenomena. Different therapists tend to subscribe more or less to one or another of those views (although eclecticism, the subscribing to different, perhaps conflicting views, is a widely held position, it is not a position that can avoid the problems of theoretical commitment, cf. Slife and Williams 1995, 46-48). The atheoretical nature of *DSM* is first of all an attempt to make one classification that can serve all therapists whatever view they have about the causes of mental diseases. When etiology is unknown or controversial, *DSM-III* found (APA 1980, 7) that the definition of a disorder must be at the “lowest order of inference necessary to describe the characteristic features of the disorder.” Wakefield (1999, 966) suggested that rather than

speaking about an atheoretical strategy, *DSM* would have been more rigorous if it had used the term “theory neutral” categories or classification criteria with respect to plausible theories of etiology.

The atheoretical approach in *DSM-III* aimed at fulfilling three different goals:

1. It aimed at moving beyond ideological divisions and towards science;
2. It aimed at temporarily setting aside the etiological issue in order to focus on acute descriptions of mental disorders; and,
3. It aimed at reforming the usual clinical vocabulary by avoiding as much as possible any claims about mental events.

However, one might ask: is it possible to avoid theory (“ideology”) in science and scholarship? Melvin Sabshin, the medical director of the APA during this period, considered that the success of the *DSM-III* was a victory of science over ideology (Sabshin 2008). Steeves Demazeux, however, commented (2015, 7, note 3):

It is nevertheless interesting to note that the opposition between science and ideology has different meanings in Sabshin’s work: in 1964, Sabshin and his colleagues defined “ideology” as including any “theoretical models of mental disorders” (see Strauss et al. 1964, 8). This conciliatory definition tends to consider that science and ideology are always inextricably intertwined. But in his 2006 book, Sabshin defines “ideology” as all “scientifically unsupported dogmas” (Sabshin 2006, 36). Here, ideology is clearly set outside the range of science (see Demazeux 2013, 152). With distance and hindsight, it is clear that, according to Sabshin, the atheoretical perspective of the *DSM-III* is scientific in opposition to ideological in the second sense.

Sabshin (2008) is thus wrong in both senses: first, that theory (which he associated with ideology) is not opposed to science. On the contrary, science is based on theoretical developments; and second, because *DSM-III* itself is not sufficiently scientifically supported, this argument cannot be used against other theories. It is rather Sabshin himself (and the atheoretical principle of *DSM-III*) that introduces a kind of ideological bias, which impedes scientific progress by subscribing to the principle “reliability before validity” (Callender 2013, 77, emphasis added):

There are also downsides to what is described in this book as the “reliability first, validity second” approach. One of these is that diagnostic entities be-

come “self-perpetuating feedback loops.” Although they may only be rough approximations to “real” conditions (if such things can be said to exist), they become the phenotypes that are used in research. A genetic study of schizophrenia will be a study of *DSM*-defined schizophrenia. An antidepressant drug trial will be carried out on patients with *DSM*-defined depression. *DSM* categories determine what questions can be asked and therefore risk becoming *a system that impedes rather than advances scientific progress.*

Descriptions are not atheoretical, although the persons doing the descriptions may be unaware of their theoretical influences and thereby produce “biased” descriptions. As argued in Hjørland (2016), explicit and considered subjectivism should be preferred for subjectivity disguised as objectivity. Further problems in the descriptive approach to classification are presented in the section below about epistemological hubs.

7.0 Ontological versus epistemological approaches to classification

In the community of knowledge organization researchers, there seem to be two different approaches, the ontological approach and the epistemological approach, and some researchers try to combine these two approaches (Kleineberg 2015, 194): “As pointed out by Gnoli (2008), the open question remains in which way ontology-oriented and epistemology-oriented approaches might be integrated in order to benefit from their possibly complementary character.”

However, as argued by Sadegh-Zadeh (2015, 759), “ontology cannot be independent of epistemology. The quality of an epistemology will influence, via the knowledge it approves or refutes, the quality of the corresponding ontology. For instance, compare the world of an astrologer with that of an astronomer.” In knowledge organization, a well-informed paper about this question, Martínez-Ávila and Fox (2015, 16) wrote:

The existence of different categorial schemes and the disagreements about the “nature” of those categories highlights the importance of epistemology as a complement to ontology rather than as a separate entity. Though one might claim a singular ontological arrangement of concepts, the philosophical objections to this ontological arrangement must be understood in order to justify one’s claims.

I believe the present paper is a further argument for the necessity of considering epistemological issues in classification, and I challenge everybody to put forward a classi-

fication of mental diseases—or just defend an existing one—based on “the ontological approach,” ignoring epistemological issues. When we consider such a specific example, the necessity for an epistemological approach seems evident. What has been termed “the ontological approach” in knowledge organization is in reality an atheoretical approach. If not, on what basis are the criteria of semantic relations in the classification decided? How is it decided that A is a kind of B?

8.0 Classifications as epistemological hubs?

Susan Leigh Star and James R. Griesemer introduced in 1989 the concept of “boundary objects” which since has become very influential. They explicitly included classifications and defined it in this way (1989, 393): “Boundary objects are objects that are both plastic enough to adapt to local needs and the constraints of the several parties employing them, yet robust enough to maintain a common identity across sites.”

Is a classification like *DSM* a boundary object? Or, how should it be constructed if we wanted it to serve as a boundary object? Jonathan Tsou suggested (2015a, 43) that *DSM* should be designed to serve as an “epistemological hub”:

I argue that the *DSM* should reconceive of its goals more narrowly such that it functions primarily as an epistemic hub that mediates among various contexts of use in which definitions of mental disorders appear. My analysis emphasizes the importance of pluralism as a methodological means for avoiding theoretical dogmatism and ensuring that the *DSM* is a reflexive and self-correcting manual.

As far as I understand an “epistemological hub” is equivalent to a “boundary object” (and it seems to me to be a better term). Tsou’s suggestion on how to design *DSM* for better serving this goal seems to be more clear than the one provided by Star and Griesemer (and it also seems to be better related to our discussions of “descriptive” versus “theoretical” classifications). Tsou (2015a) examined the goals of *DSM-III*, which were guiding treatment, facilitating research, and improving communication. He suggested that the *DSM*’s purely descriptive approach is best suited for improving communication among mental health professionals, but theoretical approaches would be superior for purposes of treatment and research. He also outlined steps required to move the *DSM* towards a hybrid system of classification that can accommodate the benefits of descriptive and theoretical approaches, and discussed how the *DSM*’s descriptive categories could be revised to incorporate theoretical information regarding the causes of disor-

ders. He argues that the *DSM* should reconceive of its goals more narrowly such that it functions primarily as an epistemic hub that mediates among various contexts.

At first reading, this suggestion seemed highly persuasive to me. However, my second thought was: what is the purpose of communicating “descriptive” classifications if they are not valid? Because the etiology of mental diseases is often unknown, their definitions sometimes must—as also stated by Tsou (2015a, 45)—be based on descriptions of symptoms and on emphasizing that “the importance of pluralism as a means to ensure that the *DSM* is informed by a multiplicity of, sometimes conflicting, scientific theories on psychopathology.” We cannot here go deeper into this problem, but the solution for *DSM*—or another system trying to serve as “boundary objects” or “epistemological hubs” for defining and classifying objects—should probably be to outline the most important alternative theories, their conceptions and classifications. Then a conversion table or “crosswalk” should be established (although this may turn out to be difficult due to the taxonomic incommensurability of different theories, cf. Oberheim and Hoyningen-Huene 2013). Andersen, Barker and Chen (2006, 42–64) presented a system for representing concepts by means of dynamic frames, which may also be worth further examination in the KO-community in relation to epistemological hubs. The concept “bird,” for example, may be defined by among other attributes the values of the attributes “beak,” “neck,” “color,” “size,” “foot” and “gait.” This system is also used to relate concepts in different scientific theories or paradigms.

9.0 Conclusion

This article has put forward arguments that “atheoretical classification” is either an artificial classification or an oxymoron and in both cases unsatisfactory as a basis for scientific taxonomies. We have considered one important example of an atheoretical classification, the *DSM-III*. This system claims to be based on an atheoretical approach to classification, but it has been argued that its great success probably was not due to its lack of theoretical commitment but rather to two highly attractive ideological underpinnings: biological reductionism and “reliability before validity.” These two ideological underpinnings may both have impeded scientific progress in the field as well as the treatment of psychiatric patients on a scientific basis. The apparent success of *DSM-III* may therefore have been achieved at great cost.

Only one example has been analyzed in this paper, but the analysis is related to my analysis of the subjectivity of bibliometric maps (see Hjørland 2016). The principle about the theoretical foundation of all knowledge organization systems (KOSs) should be a considered a general

principle in knowledge organization. It seems obvious, that for all KOSs the most important evaluation criterion is their theoretical commitments (which are often their hidden theoretical assumptions). This should therefore also be emphasized in the principles of how to construe KOSs. This may not be easy, but something that cannot be neglected without loss of scientific credibility for knowledge organization. As claimed by Hjørland (2013), it is difficult to find other approaches to the field of equal importance: knowledge organization has to be based on theories of knowledge.

The idea of atheoretical classification is, as mentioned above, associated with “positivism.” However, Turner (1993) realized that the founder of positivism, Auguste Comte, had been misinterpreted and wrote “Comte would turn over in his grave” if he was confronted with the present-day understanding of positivism. He demonstrated that Comte’s “positive science” clearly and explicitly included a central role for theory and that Comte dismissed as unscientific the kind of empirical research that is conducted in the absence of theory. My use of the term “positivism” is derived from Kuhn’s (1962) criticism of logical positivism (although Kuhn was badly informed in the writings of the logical positivists, see Tsou 2015b). The opposition between Kuhn and the positivist can perhaps best be explained in this way: the positivists try to compare our theories with “reality” but according to Kuhn, this is not possible because we have no unmediated access to “reality.” We cannot compare our theories or beliefs about the world with “reality,” but only with alternative theories (see also Devlin 2015, 157). In Hjørland (2016) I also present my understanding of “positivism” and the Kuhnian alternative. The basic argument is that researchers who do not reveal their theoretical basis and claim to be “objective” do in reality act on the basis of a subjectivity disguised as objectivity. Therefore, we may conclude with Karl Theodor Jaspers (1913, here quoted from Maj 2015, 68): “if anyone thinks he can exclude philosophy and leave it aside as useless, he will eventually be defeated by it in some obscure form or another.”

The atheoretical view of classification seems also to be related to standardization as an approach for constructing KOSs (see Fujigaki 2006). Standardization shall not be discussed further in the present paper, but it has recently struck me that standardization seems to be a rather widespread approach in knowledge organization in need for a deeper examination of its theoretical assumptions.

Olson and Fox (2010) recommend Gayatri Spivak’s feminist Marxist postcolonial approach as a theoretical framework for uncovering bias and cultural imperialism in knowledge organization. At first, it seems paradoxical that such a view should provide more objectivity in research, but Harding (2015) puts forward a convincing argument.

There is a lack of research in knowledge organization based on subject knowledge and on considering the influence of different theories on concepts and classification. Hope Olson’s research is an exception from this general tendency and provides a much-needed contribution to the field.

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