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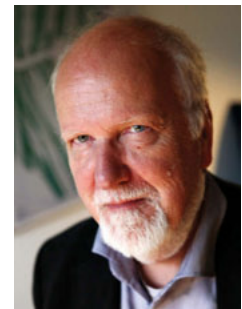
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The Paradox of Atheoretical Classification

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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

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

9
10 Given this general evolution of American psychiatry,
11 the history of the DSM is paradoxical. The third edi-
12 tion (*DSM-III*), published in 1980, constitutes ac-
13 cording to historians a true nosological revolution.
14 But this revolution did not consist in having fully
15 embraced neurological or genetic factors, as we
16 might have expected given the general evolution of
17 psychiatry, and especially of American psychiatry.
18 Rather, it consisted in remaining, or trying to remain,
19 atheoretical. The *DSM-III* offered a classification
20 that voluntarily ignored the etiological models of
21 mental disorders, in order to focus instead on the
22 task of providing unambiguous descriptions of these
23 disorders by means of precise and exhaustive diag-
24 nostic criteria. It is often said that the *DSM-III* broke
25 with psychoanalysis, which was dominant in large
26 American cities. It is less often said that it also broke
27 with the essentially biological direction of its prede-
28 cessors (while the influence of psychoanalysis on the
29 *DSM-I* and *DSM-II* is regularly stressed by histori-
30 ans, we should not forget that the first two editions
31 of the DSM gave an important role to biology, as il-
32 lustrated by the key category of “Organic Brain syn-
33 drome”).

34
35 Despite the atheoretical nature of *DSM-III* and following
36 editions (or because of it?) it became an extreme success
37 (Demazeux and Singy 2015, xv-xvi):

38
39 There is another paradox in the history of the DSM:
40 its [*DSM-III*’s] extraordinary success. While the
41 *DSM-III* did not offer any theoretical innovation,
42 but only a stupendous methodological audacity, its
43 impact on contemporary psychiatric discourse and
44 practice has been considerable. The first printing of
45 the *DSM-III*, in 1980, was quickly sold out. It was
46 soon translated into many languages...The succes-
47 sive editions, the *DSM-III-R* (1987), the *DSM-IV*
48 (1994), and the *DSM-IV-TR* (2000), only reinforced
49 the world domination of the DSM. The DSM
50 quickly supplanted most national classifications,
51 wiped them from memory, and contributed to an
52 unprecedented homogenization and universalization
53 in the practice of psychiatry. It even influenced the

54 WHO’s ICD [International Classification of Dis-
55 eases], which, from 1992 on, came to adopt a struc-
56 ture and methodological principles that are very simi-
57 lar to the DSM’s Without a doubt, today no psy-
58 chiatrist in the world is unaware of the meaning of
59 the acronym “DSM,” even when many clinicians
60 continue to resist the imperative to use this manual.
61 Most certainly, all scientifically ambitious researchers
62 in psychiatry must refer to the DSM, however unsat-
63 isfied they might be with the manual. Today, with the
64 publication of the *DSM-5* in 2013, and despite all
65 the criticisms and complaints directed against this
66 new edition even before it was published (and often
67 formulated by American psychiatrists themselves),
68 the hegemony of the American system remains in-
69 tact.”

70
71 How can this success be explained? It seems also para-
72 doxically because, as Poland’s (2015) analysis demon-
73 strates, overall *DSM*-based research has not yielded any
74 solid result and *DSM* provides researchers with de-
75 contextualized diagnoses that are symptom-based, atheo-
76 retical, polytheist, and not associated with well-confirmed
77 tests and models. Rather, *DSM* has a flawed epistemologi-
78 cal structure and categories provide “artificial groupings”
79 of individuals experiencing mental illness.

80 What is stated here is that the success of *DSM-III* is
81 not based on solid research and that the classes in this sys-
82 tem are not associated with research-based diagnostic tests
83 or procedures. Therefore, its success seems paradoxical.
84 Demazeux and Singy (2015) also write, however, that
85 much criticism was raised against the new *DSM-5*. Thus,
86 its success was perhaps temporary, and psychiatric diagno-
87 sis—and thus psychiatry as a whole—seems to be in a se-
88 rious crisis, very different from the general pattern of
89 progress in other subfields of medicine (see, e.g., Frances
90 2013; Greenberg 2013; Maj 2015; Paris and Phillips 2013;
91 and Zachar et al. 2014). Nonetheless, the atheoretical
92 principle of *DSM-III* was considered extremely successful,
93 even a revolution (at least for a period), which is a fact that
94 calls for an explanation.

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

101
102 The revisions to the *DSM-III* sought to increase re-
103 liability through moving psychiatry away from the
104 fluid psychoanalytic understanding of mental illness
105 toward a standardized nosology of fixed disease
106 categories. They overthrew the broad, continuous,

1 and vague concepts of dynamic psychiatry and re-
 2 placed them with a discrete system of classification
 3 that treated mental disorders as discrete diseases.
 4 This nosology rigorously segregated the pathologi-
 5 cal from the normal, in a way that the previous
 6 psychodynamic model never did.

7
 8 While the DSM-III revisions were advertised as ag-
 9 nostic toward different theoretical schools of psy-
 10 chiatry [APA 1980], the entire endeavor—
 11 delineating discrete disease categories to facilitate di-
 12 agnostic consistency—implied an endorsement of
 13 the biomedical model. The revisions were sold as
 14 ways to improve treatment through empirically
 15 based research programs and targeted diagnoses.
 16 The new paradigm of diagnostic psychiatry organ-
 17 ized symptoms into discrete disease entities with the
 18 expectation that the organic bases of these entities
 19 would soon be discovered [Horwitz 2002]. In other
 20 words, the revisions to the DSM were a strategy to
 21 attain a biomedical model by understanding illnesses
 22 as stable entities that can be explained in terms of
 23 specific causal mechanisms located in the brain. The
 24 hope was that the identification of the elusive bio-
 25 logical or genetic markers for mental disorders
 26 would follow from the standardized classification
 27 system. DSM-III promised a future when specific
 28 etiologies were discovered for specific disorders and,
 29 in turn, specific treatments would emerge.

30
 31 Whooley and Horwitz (2013) thus argue that *DSM-III* was
 32 indeed based on a (meta)theoretical view, the view of bio-
 33 logical psychiatry as opposed to, among others, psycho-
 34 dynamic and cultural views. A narrow biological view of
 35 mental illness is opposed to a rather broadly recognized
 36 view that mental illness should be understood from a
 37 combined biological, psychological and social perspective.
 38 For those, who believe that a form of biopsychosocial
 39 model provides the best scientific explanation of mental
 40 illness, the adaption of the strict biological view underly-
 41 ing *DSM-III* does not provide scientific advance, but on
 42 the contrary: it represents a reductionist scientific ideol-
 43 ogy. This ideology was extremely successful by strength-
 44 ening the professional powers of psychiatrists at the ex-
 45 pense of other professions involved in mental health, in
 46 standardizing psychiatry and gaining support from power-
 47 ful institutions such as health assurance and the pharma-
 48 ceutical industry. This is the basic explanation of the
 49 success of *DSM-III* as described by Whooley and Horwitz
 50 (2013). However, because this ideology is not scientifically
 51 based—but based on anticipation, the expectation that the
 52 organic bases of these entities would soon be discov-
 53 ered—the system fails because the expected findings did

54 not turn up and thus it has severe problems reflecting a
 55 natural classification of mental illness. Therefore, the sys-
 56 tem is today in a serious validity crisis as revealed by a
 57 growing critical literature. The issue about the biological
 58 basis of mental diseases has not been settled but remains
 59 highly controversial. The point here is that different theo-
 60 ries should be developed, brought into a dialogue and ne-
 61 gotiated. The disregarding of other theories without
 62 proper basis in research is ideological rather than scien-
 63 tific. The atheoretical attitude contributes to such an ide-
 64 ology. Also Foerstl and Hoff (2009, 57) argued against
 65 atheoretical classification of psychiatric disorders and
 66 wrote:

67
 68 Wir sollten auf dem Boden einer aufgewerteten Psy-
 69 chopathologie die unfruchtbare Polarisierung „theo-
 70 riereich“ vs. „theoriearm“/„theoriefrei“ hinter uns
 71 lassen Was wir brauchen in der Psychiatrie, ist kei-
 72 ne „Theoriephobie,“ sondern reflektierte und damit
 73 undogmatische Theorie.
 74

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

- 77
- 78 1) That it was not atheoretical as it claimed to be. Rather, it
 79 was supporting a reductionist biomedical theory of
 80 mental illness (although only in a very general way).
 81 This is the hidden theoretical assumption that Whooley
 82 and Horwitz (2013) uncovered.
- 83 2) Its contribution to the homogenization and universali-
 84 zation of the practice of psychiatry and to the power of
 85 the psychiatric profession. This was obtained by the
 86 emphasis on reliability at the expense of validity as we
 87 shall consider in a following section.
 88

89 4.0 Artificial versus natural classification

90
 91 Poland (2015) characterized *DSM*'s categories as “artificial
 92 groupings.” The distinction between artificial and natural
 93 classification is important. An artificial classification may
 94 be useful for limited purposes. For example, we often or-
 95 ganize documents and document representation in artificial
 96 ways (e.g. in alphabetical order), which for many purposes
 97 have proven to be very practical. However, in order to re-
 98 trieve things of a specific natural kind, those things have to
 99 be identified, named and classified, which presupposes a
 100 subject approach and natural classification.

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

102
 103 For example, one may divide rocks—or even ani-
 104 mals—into those weighing less than ten grams,
 105 those weighing at least ten but less than twenty
 106 grams, and so on; but this is likely to be of little

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

3
4 And later:

5
6 [A classification] is fruitful to the extent that it sug-
7 gests new hypothesis, explanations, and theories
8 concerning its subject matter. For example, the pe-
9 riodic table—the classification of the elements—
10 proved extremely fruitful, since it suggested the ex-
11 istence of hitherto unknown elements and even
12 suggested what physical properties they would
13 have. It should be noted that natural classifications,
14 by definition, are more fruitful than artificial ones.

15
16 To make an extreme case, one could classify all mental
17 patients according to their weight, which would be “an
18 artificial grouping” that would not be helpful for treat-
19 ment decisions (but it would be highly objective and reli-
20 able!). “A natural classification” of mental patients, on the
21 other hand, would be one that could be helpful predicting
22 which patients would benefit from a particular treatment.
23 Such a natural classification should be based on theory
24 and knowledge of mental diseases. This example is ex-
25 treme, of course, but theoretical clarity is often obtained
26 by considering such “pure” examples. This clarity can
27 thereafter also be applied to less extreme examples.

28 29 5.0 Reliability versus validity of classifications

30
31 The issue concerning artificial versus natural classification
32 is related to the problem of “reliability” versus “validity”
33 of classifications. “Reliability” means that different persons
34 report the same result, (i.e., obtain consistency), corre-
35 sponding to the concept known as “inter-indexer consis-
36 tency” in knowledge organization (see Lancaster 2003, 68-
37 82). “Validity,” on the other hand, means that a description,
38 a measurement or a representation is adequate in relation
39 to its purpose. Often there is a high prestige for profes-
40 sionals connected to reliability (the same is also, of course,
41 expected for validity, but professionals in disagreement are
42 easily revealed, and low reliability is in itself a sign of low
43 validity, therefore there is a tendency to make priority to re-
44 liability). Probably the most important issue in the success
45 of *DSM-III* was that it made psychiatric diagnosis much
46 more reliable—thereby increasing the status of psychia-
47 trists (this has been the overall opinion, but Kirk and
48 Kutchins (1992) nonetheless found *DSM-III* to be highly
49 unreliable). High reliability was the top priority for the de-
50 signers of that system and this importance is reinforced by
51 the demand from, in particular, (American) insurance
52 companies to make medical diagnoses reliable. Philips
53 (2014, 164) states:

54 The quest for this Holy Grail began back in 1980
55 with *DSM-III* (American Psychiatric Association
56 1980). The immediate goal of that manual was to
57 achieve the first step in a scientific nosology—
58 diagnostic reliability—with the use of operational
59 definitions and diagnostic criteria. With *DSM-III* we
60 could be confident that clinicians and researchers in
61 different countries would be talking about the same
62 phenomenon when they discussed, for instance,
63 schizophrenia. Built into the *DSM-III* process, how-
64 ever, was the understanding that reliable diagnoses
65 could not yet claim to be valid; we could *not* feel
66 comfortable that the diagnostic concept in question
67 represented a distinct, real entity in the world. How
68 did we know, for instance, whether the diagnostic en-
69 tity called schizophrenia described one distinct illness
70 or several? In that way, the accomplishment of
71 *DSM-III* immediately unleashed a new anxiety and a
72 new goal—securing diagnostic validity.
73 In prioritizing reliability over validity, the architects
74 of *DSM-III* assumed that ongoing research would
75 lead to valid diagnostic constructs.

76
77 However, let us return to the previous thought experiment:
78 if psychiatrists classify their patients according to, for ex-
79 ample, their weight, they would be able to provide ex-
80 tremely reliable groupings, but would not create categories
81 that would be helpful in determining treatments and pre-
82 dicting recovery. *DSM-III* of course, used more valid crite-
83 ria than the patients’ weight, but the question of lack of
84 validity of psychiatric diagnosis is today very pressing and
85 the word “crisis” is probably not too strong a term to
86 characterize the situation. A given mental disorder may be
87 defined by a combination of symptoms. “Schizophrenia,”
88 for example, is mostly characterized (Zielasek and Gaebel
89 2015, 9) by a combination of symptoms including delu-
90 sions, hallucinations and disorganized thinking with disease
91 onset in early adulthood. Any such set of symptoms may
92 be labeled schizophrenia as an artificial definition and clas-
93 sification. However, the claim that these symptoms corre-
94 spond to one and only one natural kind is a scientific hy-
95 pothesis that has to be proven. Some scientific classifica-
96 tions (e.g., bird classifications) have today a solid scientific
97 basis (about the recent progress in classification of birds,
98 see Fjeldsø 2013). No corresponding solid scientific basis
99 supports our present classification of psychiatric diagnoses
100 and the important point is that the categories in a system
101 like *DSM* have to be considered scientific hypotheses.
102 Their revision should therefore be based on considering
103 the supporting evidence as well as the evidence supporting
104 alternative categories. Again, we may conclude that it is
105 paradoxical not to consider these categories as theories.
106 What kind of decision has produced these categories? Of

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

5
6 In attempting to resolve various diagnostic issues,
7 the Task Force relied, as much as possible, on re-
8 search evidence relevant to various kinds of diag-
9 nostic validity. For example, when discussing a
10 problematic diagnostic category, the Task Force
11 considered how the disorder, if defined as pro-
12 posed, provided information relevant to treatment
13 planning, course, and familial pattern. It should
14 come as no surprise to the reader that even when
15 data were available from relevant research studies,
16 Task Force members often differed in their inter-
17 pretations of the findings.

18
19 This general method has comically been summarized as
20 the BOGSAT method: “A bunch of guys sitting around a
21 table” (Kendler and Parnas 2012, 141). It should be con-
22 sidered that in evidence based medicine (EBM), evidence
23 from expert committee reports or opinions and/or clini-
24 cal experience of respected authorities is considered a
25 very low criterion of evidence, (cf. Hjørland 2011, 1302).
26 There is much criticism about the *DSM* system and its
27 emphasis on reliability and a quote from the book *Philoso-*
28 *phical Issues in Psychiatry* (Parnas and Sass 2008, 271) reads:
29 “Excessive and, in our view, epistemologically naïve focus
30 on the issues of reliability created serious problems con-
31 cerning the more fundamental level of validity.”

32 Such an excessive focus on reliability at the expense of
33 validity is connected to positivist currents and naïve real-
34 ism. While consistency has normally been regarded as an
35 ideal also in knowledge organization, Cooper (1969) pro-
36 vided some important reservations in relation to this
37 ideal. He demonstrates that indexers that are most consis-
38 tent with each other do not necessarily produce the best
39 work and that indexing “can be consistently bad.” While
40 this might seem strange, it is understandable if, for exam-
41 ple, indexers have been taught bad principles. Superficial
42 indexing may, for example, rely too much on titles of the
43 documents being indexed, which tends to produce consis-
44 tent but bad indexing. The same is of course the case
45 about “indexing” psychiatric patients into diagnostic
46 classes. The principle “reliability before validity” is thus a
47 problematic principle connected to the ideal of atheoreti-
48 cal classification.

50 6.0 “Descriptive” versus “theoretical” classifications

51
52 *DSM-III* and later editions (see Tsou 2015a) are said to be
53 “descriptive” rather than theoretically committed. How-

54 ever, as Gregory Bateson wrote (1977, 147), it is problem-
55 atic to consider “descriptions” an atheoretical activity:

56

57 You can never get away from theories of the nature
58 of description whenever, wherever you have descrip-
59 tions. All descriptions are based on theories of how
60 to make descriptions. You cannot claim to have no
61 epistemology. Those who so claim have nothing but
62 a bad epistemology. And every description is based
63 upon, and contains implicitly, a theory of how to de-
64 scribe.

65

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

68

69 Indeed, since Kuhn’s (1962) *Structure of Scientific Revolu-*
70 *tions*, the notion of the theory-laden nature of observa-
71 tions (and by implication the theory-laden nature of de-
72 scriptions) has become a fundamental assumption in the
73 philosophy of science. By implication, the distinction be-
74 tween “descriptive” and “theoretical” classifications is
75 problematic—as already indicated with Sellars’ (1956) no-
76 tion “the myth of the given” mentioned above. (This
77 recognition of the theoretical ladenness of descriptions
78 may have further important implications for the use of
79 this term in knowledge organization, e.g., in the concepts
80 “descriptive bibliography” and “descriptive cataloging”).

81 In order to understand the atheoretical approach in
82 *DSM-III*, we shall look at what is here meant by “descrip-
83 tive” versus “theory-based” classification. It is well
84 known that psychology and psychiatry have been split in
85 many conflicting views or schools, e.g., psychoanalysis,
86 cultural psychology, behaviorism, cognitivism, neurosci-
87 ence. Such views tend to develop their own conceptual
88 systems and classifications. “Neurosis,” for example, is a
89 psychoanalytic concept, “organic brain syndrome” is a
90 concept associated with neuroscience. Although different
91 theories may have their own domains for which they pro-
92 vide good explanations, they also tend to provide op-
93 posed views on the same phenomena. Different thera-
94 pists tend to subscribe more or less to one or another of
95 those views (although eclecticism, the subscribing to dif-
96 ferent, perhaps conflicting views, is a widely held posi-
97 tion, it is not a position that can avoid the problems of
98 theoretical commitment, cf. Slife and Williams 1995, 46-
99 48). The atheoretical nature of *DSM* is first of all an at-
100 tempt to make one classification that can serve all thera-
101 pists whatever view they have about the causes of mental
102 diseases. When etiology is unknown or controversial,
103 *DSM-III* found (APA 1980, 7) that the definition of a
104 disorder must be at the “lowest order of inference neces-
105 sary to describe the characteristic features of the disorder.”
106 Wakefield (1999, 966) suggested that rather than

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

5 The atheoretical approach in *DSM-III* aimed at fulfill-
6 ing three different goals:

- 7
- 8 1. It aimed at moving beyond ideological divisions and
9 towards science;
- 10 2. It aimed at temporarily setting aside the etiological is-
11 sue in order to focus on acute descriptions of mental
12 disorders; and,
- 13 3. It aimed at reforming the usual clinical vocabulary by
14 avoiding as much as possible any claims about mental
15 events.
- 16

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

23

24 It is nevertheless interesting to note that the opposi-
25 tion between science and ideology has different
26 meanings in Sabshin’s work: in 1964, Sabshin and his
27 colleagues defined “ideology” as including any
28 “theoretical models of mental disorders” (see Strauss
29 et al. 1964, 8). This conciliatory definition tends to
30 consider that science and ideology are always inextric-
31 ably intertwined. But in his 2006 book, Sabshin de-
32 fines “ideology” as all “scientifically unsupported
33 dogmas” (Sabshin 2006, 36). Here, ideology is clearly
34 set outside the range of science (see Demazeux
35 2013, 152). With distance and hindsight, it is clear
36 that, according to Sabshin, the atheoretical perspec-
37 tive of the *DSM-III* is scientific in opposition to
38 ideological in the second sense.

39

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

50

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

54 come “self-perpetuating feedback loops.” Although
55 they may only be rough approximations to “real”
56 conditions (if such things can be said to exist), they
57 become the phenotypes that are used in research. A
58 genetic study of schizophrenia will be a study of
59 *DSM*-defined schizophrenia. An antidepressant drug
60 trial will be carried out on patients with *DSM*-
61 defined depression. *DSM* categories determine what
62 questions can be asked and therefore risk becoming
63 *a system that impedes rather than advances scientific progress.*
64

65 Descriptions are not atheoretical, although the persons
66 doing the descriptions may be unaware of their theoreti-
67 cal influences and thereby produce “biased” descriptions.
68 As argued in Hjørland (2016), explicit and considered
69 subjectivism should be preferred for subjectivity dis-
70 guised as objectivity. Further problems in the descriptive
71 approach to classification are presented in the section be-
72 low about epistemological hubs.

74 **7.0 Ontological versus epistemological approaches** 75 **to classification**

76

77 In the community of knowledge organization research-
78 ers, there seem to be two different approaches, the onto-
79 logical approach and the epistemological approach, and
80 some researchers try to combine these two approaches
81 (Kleineberg 2015, 194): “As pointed out by Gnoli (2008),
82 the open question remains in which way ontology-
83 oriented and epistemology-oriented approaches might be
84 integrated in order to benefit from their possibly com-
85plementary character.”

86 However, as argued by Sadegh-Zadeh (2015, 759),
87 “ontology cannot be independent of epistemology. The
88 quality of an epistemology will influence, via the knowl-
89 edge it approves or refutes, the quality of the corre-
90 sponding ontology. For instance, compare the world of
91 an astrologer with that of an astronomer.” In knowledge
92 organization, a well-informed paper about this question,
93 Martínez-Ávila & Fox (2015, 16) wrote:

94

95 The existence of different categorial schemes and
96 the disagreements about the “nature” of those cate-
97 gories highlights the importance of epistemology as
98 a complement to ontology rather than as a separate
99 entity. Though one might claim a singular ontological
100 arrangement of concepts, the philosophical objec-
101 tions to this ontological arrangement must be under-
102 stood in order to justify one’s claims.

103

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

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

11 8.0 Classifications as epistemological hubs?

13 Susan Leigh Star and James R. Griesemer introduced in
 14 1989 the concept of “boundary objects” which since has
 15 become very influential. They explicitly included classifi-
 16 cations and defined it in this way (1989, 393): “Boundary
 17 objects are objects that are both plastic enough to adapt
 18 to local needs and the constraints of the several parties
 19 employing them, yet robust enough to maintain a com-
 20 mon identity across sites.”

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

27 I argue that the *DSM* should reconceive of its goals
 28 more narrowly such that it functions primarily as an
 29 epistemic hub that mediates among various con-
 30 texts of use in which definitions of mental disor-
 31 ders appear. My analysis emphasizes the importance
 32 of pluralism as a methodological means for avoid-
 33 ing theoretical dogmatism and ensuring that the
 34 *DSM* is a reflexive and self-correcting manual.

36 As far as I understand an “epistemological hub” is equiva-
 37 lent to a “boundary object” (and it seems to me to be a
 38 better term). Tsou’s suggestion on how to design *DSM* for
 39 better serving this goal seems to be more clear than the
 40 one provided by Star and Griesemer (and it also seems to
 41 be better related to our discussions of “descriptive” versus
 42 “theoretical” classifications). Tsou (2015a) examined the
 43 goals of *DSM-III*, which were guiding treatment, facilitat-
 44 ing research, and improving communication. He suggested
 45 that the *DSM*’s purely descriptive approach is best suited
 46 for improving communication among mental health pro-
 47 fessionals, but theoretical approaches would be superior
 48 for purposes of treatment and research. He also outlined
 49 steps required to move the *DSM* towards a hybrid system
 50 of classification that can accommodate the benefits of de-
 51 scriptive and theoretical approaches, and discussed how the
 52 *DSM*’s descriptive categories could be revised to incorpo-
 53 rate theoretical information regarding the causes of disor-

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

57 At first reading, this suggestion seemed highly persua-
 58 sive to me. However, my second thought was: what is the
 59 purpose of communicating “descriptive” classifications if
 60 they are not valid? Because the etiology of mental diseases
 61 is often unknown, their definitions sometimes must—as
 62 also stated by Tsou (2015a, 45)—be based on descriptions
 63 of symptoms and on emphasizing that “the importance of
 64 pluralism as a means to ensure that the *DSM* is informed
 65 by a multiplicity of, sometimes conflicting, scientific theo-
 66 ries on psychopathology.” We cannot here go deeper into
 67 this problem, but the solution for *DSM*—or another sys-
 68 tem trying to serve as “boundary objects” or “epistemo-
 69 logical hubs” for defining and classifying objects—should
 70 probably be to outline the most important alternative theo-
 71 ries, their conceptions and classifications. Then a conver-
 72 sion table or “crosswalk” should be established (although
 73 this may turn out to be difficult due to the taxonomic in-
 74 commensurability of different theories, cf. Oberheim and
 75 Hoyningen-Huene 2013). Andersen, Barker and Chen
 76 (2006, 42-64) presented a system for representing concepts
 77 by means of dynamic frames, which may also be worth
 78 further examination in the KO-community in relation to
 79 epistemological hubs. The concept “bird,” for example,
 80 may be defined by among other attributes the values of the
 81 attributes “beak,” “neck,” “color,” “size,” “foot” and “gait.”
 82 This system is also used to relate concepts in different sci-
 83 entific theories or paradigms.

85 9.0 Conclusion

87 This article has put forward arguments that “atheoretical
 88 classification” is either an artificial classification or an oxy-
 89 moron and in both cases unsatisfactory as a basis for scien-
 90 tific taxonomies. We have considered one important exam-
 91 ple of an atheoretical classification, the *DSM-III*. This sys-
 92 tem claims to be based on an atheoretical approach to clas-
 93 sification, but it has been argued that its great success
 94 probably was not due to its lack of theoretical commit-
 95 ment but rather to two highly attractive ideological under-
 96 pinnings: biological reductionism and “reliability before va-
 97 lidity.” These two ideological underpinnings may both have
 98 impeded scientific progress in the field as well as the
 99 treatment of psychiatric patients on a scientific basis. The
 100 apparent success of *DSM-III* may therefore have been
 101 achieved at great cost.

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

1 principle in knowledge organization. It seems obvious, that
2 for all KOSs the most important evaluation criterion is
3 their theoretical commitments (which are often their hid-
4 den theoretical assumptions). This should therefore also be
5 emphasized in the principles of how to construe KOSs.
6 This may not be easy, but something that cannot be ne-
7 glected without loss of scientific credibility for knowledge
8 organization. As claimed by Hjørland (2013), it is difficult
9 to find other approaches to the field of equal importance:
10 knowledge organization has to be based on theories of
11 knowledge.

12 The idea of atheoretical classification is, as mentioned
13 above, associated with “positivism.” However, Turner
14 (1993) realized that the founder of positivism, Auguste
15 Comte, had been misinterpreted and wrote “Comte would
16 turn over in his grave” if he was confronted with the present-
17 day understanding of positivism. He demonstrated
18 that Comte’s “positive science” clearly and explicitly in-
19 cluded a central role for theory and that Comte dismissed
20 as unscientific the kind of empirical research that is con-
21 ducted in the absence of theory. My use of the term
22 “positivism” is derived from Kuhn’s (1962) criticism of
23 logical positivism (although Kuhn was badly informed in
24 the writings of the logical positivists, see Tsou 2015b).
25 The opposition between Kuhn and the positivist can per-
26 haps best be explained in this way: the positivists try to
27 compare our theories with “reality” but according to
28 Kuhn, this is not possible because we have no unmediated
29 access to “reality.” We cannot compare our theories or be-
30 liefs about the world with “reality,” but only with alterna-
31 tive theories (see also Devlin 2015, 157). In Hjørland
32 (2016) I also present my understanding of “positivism”
33 and the Kuhnian alternative. The basic argument is that
34 researchers who do not reveal their theoretical basis and
35 claim to be “objective” do in reality act on the basis of a
36 subjectivity disguised as objectivity. Therefore, we may
37 conclude with Karl Theodor Jaspers (1913, here quoted
38 from Maj 2015, 68): “if anyone thinks he can exclude
39 philosophy and leave it aside as useless, he will eventually
40 be defeated by it in some obscure form or another.”

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

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

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

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