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Hjørland, Birger

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The Paradox of Atheoretical Classification

Birger Hjørland

University of Copenhagen, Royal School of Library and Information Science, 6 Birketinget, DK-2300 Copenhagen S, Denmark, <birger.hjorland@hum.ku.dk>

Birger Hjørland holds an MA in psychology and PhD in library and information science. He is Professor in knowledge organization at the Royal School of Library and Information Science in Copenhagen since 2001 and at the University College in Borås 2000-2001. He was research librarian at the Royal Library in Copenhagen 1978-1990, and taught information science at the Department of Mathematical and Applied Linguistics at the University of Copenhagen 1983-1986. He is chair of ISKO’s Scientific Advisory Council and a member of the editorial boards of Knowledge Organization, Journal of the Association for Information Science and Technology and Journal of Documentation. His H-index is 37 in Google Scholar and 21 in Web of Science.


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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 about 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 superficially 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...
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 de-contextualized 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 overturned the broad, continuous,
not turn up and thus it has severe problems reflecting a
natural classification of mental illness. Therefore, the sys-
tem 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 theo-
dies should be developed, brought into a dialogue and ne-
gotiated. The disregarding of other theories without
proper basis in research is ideological rather than scien-
tific. The atheroretical attitude contributes to such an ide-
ology. Also Foerstl and Hoff (2009, 57) argued against
atheroretical classification of psychiatric disorders and
wrote:

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

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

1) That it was not atheroretical 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 universal-
ization 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 or-
ganize 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 re-
trieve things of a specific natural kind, those things have to
be identified, named and classified, which presupposes a
subject approach and natural classification.


For example, one may divide rocks—or even ani-
mals—into those weighing at least 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 Fjeldså 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
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 intricably 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 become “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 & 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-
1. The classification 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?

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 classifi-
12. cations and defined it in this way (1989, 393): “Boundary
13. objects are objects that are both plastic enough to adapt
14. to local needs and the constraints of the several parties
15. employing them, yet robust enough to maintain a com-
16. mon identity across sites.”
17. Is a classification like DSM a boundary object? Or, how
18. should it be constructed if we wanted it to serve as a
19. boundary object? Jonathan Tsou suggested (2015a, 43)
20. that DSM should be designed to serve as an “epistem-
21. ological hub”:
22. I argue that the DSM should reconceive of its goals
23. more narrowly such that it functions primarily as an
24. epistemic hub that mediates among various con-
25. texts of use in which definitions of mental disor-
26. ders appear. My analysis emphasizes the importance
27. of pluralism as a methodological means for avoid-
28. ing theoretical dogmatism and ensuring that the
29. DSM is a reflexive and self-correcting manual.
30. As far as I understand an “epistemological hub” is equiva-
31. lent to a “boundary object” (and it seems to me to be a
32. better term). Tsou’s suggestion on how to design DSM for
33. better serving this goal seems to be more clear than the
34. one provided by Star and Griesemer (and it also seems to
35. be better related to our discussions of “descriptive” versus
36. “theoretical” classifications). Tsou (2015a) examined the
37. goals of DSM-III, which were guiding treatment, facilitat-
38. ing research, and improving communication. He suggested
39. that the DSM’s purely descriptive approach is best suited
40. for improving communication among mental health pro-
41. fessionals, but theoretical approaches would be superior
42. for purposes of treatment and research. He also outlined
43. steps required to move the DSM towards a hybrid system
44. of classification that can accommodate the benefits of de-
45.scriptive and theoretical approaches, and discussed how the
46. DSM’s descriptive categories could be revised to incorpo-
47. rate theoretical information regarding the causes of disor-
48. ders. He argues that the DSM should reconceive of its
49. goals more narrowly such that it functions primarily as an
50. epistemic hub that mediates among various contexts.
51. At first reading, this suggestion seemed highly persua-
52. sive to me. However, my second thought was: what is the
53. purpose of communicating “descriptive” classifications if
54. they are not valid? Because the etiology of mental diseases
55. is often unknown, their definitions sometimes must—as
56. also stated by Tsou (2015a, 45)—be based on descriptions
57. of symptoms and on emphasizing that “the importance of
58. pluralism as a means to ensure that the DSM is informed
59. by a multiplicity of, sometimes conflicting, scientific theo-
60. ries on psychopathology.” We cannot here go deeper into
61. this problem, but the solution for DSM—or another sys-
62. tem trying to serve as “boundary objects” or “epistemo-
63. logical hubs” for defining and classifying objects—should
64. probably be to outline the most important alternative theo-
65. ries, their conceptions and classifications. Then a conver-
66. sion table or “crosswalk” should be established (although
67. this may turn out to be difficult due to the taxonomic in-
68. commensurability of different theories, cf. Oberheim and
70. (2006, 42-64) presented a system for representing concepts
71. by means of dynamic frames, which may also be worth
72. further examination in the KO-community in relation to
73. epistemological hubs. The concept “bird,” for example,
74. may be defined by among other attributes the values of the
75. attributes “beak,” “neck,” “color,” “size,” foot” and “gait.”
76. This system is also used to relate concepts in different sci-
77. entific theories or paradigms.

9.0 Conclusion

This article has put forward arguments that “atheoretical classification” is either an artificial classification or an oxy-
80. moron and in both cases unsatisfactory as a basis for sci-
81. entific taxonomies. We have considered one important exam-
82. ple of an atheoretical classification, the DSM-III. This sys-
83. tem claims to be based on an atheoretical approach to clas-
84. sification, but it has been argued that its great success
85. probably was not due to its lack of theoretical commit-
86. ment but rather to two highly attractive ideological under-
87. pinnings: biological reductionism and “reliability before va-
88. lidity.” These two ideological underpinnings may both have
89. impeded scientific progress in the field as well as the
90. treatment of psychiatric patients on a scientific basis. The
91. apparent success of DSM-III may therefore have been
92. 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 organi-
ization 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.

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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