Book Reviews

Edited by Clément Arsenault

Book Review Editor

Eric R. Scerri. *The Periodic Table: Its Story and Its Significance*. Oxford: Oxford University Press, 2007. xxii, 346 pages. ISBN-13: 978-0-19-530573-9.

The book is about the classification of chemical elements known as the periodical system. It is described as "one of the most potent icons in science […] One sees periodic tables everywhere: in industrial labs, workshops, academic labs, and of course, lecture halls" (p. xiii). Among all taxonomies in all domains, there is probably none more respected and more useful than this one. As Scerri states (p. 25):

> The periodic table ranks as one of the most fruitful and unifying ideas in the whole of modern science, comparable perhaps with Darwin's theory of evolution by natural selection. Unlike such theories as Newtonian mechanics, the periodic table has not been falsified by developments in modern physics but has evolved while remaining essentially unchanged. After evolving for nearly 150 years through the work of numerous individuals, the periodic table remains at the heart of chemistry. This is mainly because it is of immense practical benefit for making predictions about all manner of chemical and physical properties of the elements and possibilities for bond formation.

The periodic system provides the basic criteria for organizing knowledge about all the material stuff in the entire universe. It is thus a model that anybody with interests in knowledge organization (KO) should know. Knowledge about the history, philosophy and status of the periodic system also provides important insight for knowledge organization in general.

Eric R. Scerri is a lecturer in chemistry as well as in the history and philosophy of science at the University of California, Los Angeles. He is the founder and editor-in-chief of the journal *Foundations of Chemistry* (http://www.springerlink.com/content/1386-4238). He received his Ph.D. in History and Philosophy of Science from King's College London. The author's background in chemistry may indicate a growing recognition among scientists of the importance of conceptual and philosophical problems.

Given the importance of the periodical system, one supposes that the literature about it must be overwhelming. This is not the case, however, and the few earlier books on the subject in English are presented in the introduction. What is of special importance for us in the field of knowledge organization is that there is no other book in English that deals adequately with the conceptual and philosophical aspects of the periodical system.

The book is organized as follows:

The Periodic Table lacks a bibliography: all references are provided in the notes. There is an index, but it is not exhaustive. For example, van Spronsen is mentioned in the index, but the description of his book on p. xiv is not included in the index.

This is a high-quality scholarly work that is clear and understandable even to those without a background in chemistry and physics. While *The Periodic Table* properly belongs to the philosophy of chemistry, a new field in which the author is a pioneer, it can

also be said that the book is currently the #1 best seller in chemistry. (May 2006-present as compiled by YBP Library Services, http://www.libraryjournal.com/info/CA6408230.html)

This is a well-written and well-illustrated book. The inclusion of Mendeleev's original drafts aid in understanding the development of his system (p. 107). Mendeleev was able to predict the chemical and physical properties of a number of elements to an astonishing degree, although he also made false predictions about other elements. Tables comparing the predicted and observed properties of Gallium, Scandium and Germanium are extremely useful (p. 133–34). As the Russian historian of chemistry Bonifatii Kedrov states, "the scientific world was astounded to note that Mendeleev, the theorist, had seen the properties of a new element more clearly than the chemist who had discovered it" (quoted by Scerri, p. 150). Throughout the book, different views are presented and carefully documented. The author often also presents his own view. Personally, I would have liked a discussion of Hegel's view of the elements. Browne wrote the following in a review of Hegel's *Science of Logic*:

> Another interesting aspect of this book is its innovative contributions to the world of chemistry and the origins of the modern periodic table of the elements. Hegel sheds light on the earliest days of modern chemistry, reminding us of the revolutionary processes that led up to our understanding of chemical elements and compounds. We are reminded that everything stems from and starts with the compound, and the existence of the pure elements is inferred later by analysing phenomenon such as "mixing ratios" and saturation/absorption capacities. Hegel explains these founding pillars of chemical wisdom which many modern scientists take for granted. It is admittedly interesting to read about the processes that led to the discovery of the now-ubiquitous periodic table.

(Ross James Browne is from Atlanta, Georgia, United States, and the quote is from Amazon.com dated March 10, 2003.)

Recently, Hegel's views of chemistry have been somewhat rehabilitated after having been exposed as "grotesque mistakes" almost from the time of their publication (Ruschig 2000), which is why it would have been interesting to have Scerri's view of Hegel although this omission may be justified given the perspective of the book. Scerri's book is based on deep, first-hand knowledge of a very large number of sources.

For the remainder of this review, I would like to concentrate on my own motivations for reviewing this book, as well as demonstrate the general importance of *The Periodic Table* for information science. Researchers in knowledge organization tend to ignore the literature about scientific and scholarly classification and sometimes even speak of it in ways that seem to justify such ignorance. (See, for example, Hjørland & Nicolaisen 2004 and Nicolaisen & Hjørland 2004.)

Some of my own working hypotheses for a general theory of classification are:

> That any classification reflects a theory of the domain it classifies.
> That a classification should be based on pragmatic criteria related to the purpose for which it is constructed (as opposed to "objective" criteria). This is related to the problem known as "natural kinds".
> That knowledge is fallible and that different views compete in any domain. Each view implies its own criteria for description and classification of the phenomena in the domain. Competing views are basically related to different epistemological views, of which the most important are empiricism, rationalism, historicism and pragmatism (of which pragmatism is the most advanced theory, subsuming the other theories).
> That basic conceptions and classifications often first develop in science and scholarship, from which they spread to public media and library classification systems, among other areas.

How does the present book contribute to illuminating these hypotheses?

Concerning (1). The periodic table was mainly constructed before the discovery of quantum mechanics. How can a classification system endure in spite of such a theoretical revolution? The answer is that the periodical system is based on the periodical law stating "that after certain regular but varying intervals the chemical elements show an approximate repetition in their properties" (p. 16). This law is unaffected by later discoveries. In fact, it contributed much to them. The discovery of isotopes did shake the periodic system, but it was rescued by, among other things, a conception of the elements as "basic substances" and not as "simple substances."

Concerning (2). The periodical system is probably one of the most difficult classification systems to defend from a pragmatic point of view. However, it is also important to test our views against the most pre-eminent classifications if our arguments should be convincing.

First, it is clear that although there is only one periodic law, there are many periodical tables (more than 700 different tables have been published), which serve different pragmatic purposes:

Thus, there are many forms of the periodical table, some designed for different uses. Whereas a chemist might favor a form that highlights the reactivity of the elements, an electrical engineer might wish to focus on similarities and patterns in electrical conductivities (Scerri, p. 20).

Scerri discusses elements and their groupings as "natural kinds". The general idea is that the elements represent the manner in which nature has been "carved at the joints": "[o]n this view, the distinction between an element and another one is not a matter of convention" (p. 280). The same is said about their relations: "[i]f periodic relationships are indeed objective properties, as I argue here, it would seem to suggest that there is one ideal periodic classification, regardless of whether or not this may have been discovered" (p. 280).

In the past, this issue has been debated in the literature:

> [S]he [Bryant 2000] nevertheless argues (p. 88–92) that even in the case of chemical elements more than one kind of causal essentialism is scientifically legitimate, that no one kind is privileged.

The fact is, modern scientists classify atoms into elements based on proton number rather than anything else because it alone is the causally privileged factor. Thus nature itself has supplied the causal monistic essentialism. Scientists in their turn have simply discovered and followed (where "simply" ≠ "easily") (Stamos 2004, p. 138–39).

One way to solve this problem has been suggested by John Dupré (2006):

> It is often supposed that one of the goods delivered by successful science is the right way of classifying the things in the world. […] The standard paradigm for such a successful scientific classification is the periodic table of the elements.

However, there is also much potentially wrong with the supposition just mentioned. Most importantly, there is a highly questionable implication of there being some uniquely best classification. Classifications are good or bad for particular purposes, and different purposes will motivate different classifications. It may be that there is such an ideal classification for chemistry, but if so it is because of the specific aims implicit in the history of that discipline. Chemistry aims at the structural analysis of matter and if, as appears to be the case, all matter is composed of a small number of structural elements, a classification based on those elements will be best suited to these purposes. It is also often the case that chemical structure will be the best guide to the properties of kinds of matter, but not necessarily. Two quite distinct chemicals are referred to as 'jade' and, despite some serious debates on the issue, Chinese jade carvers have decided that both are real jade (LaPorte 2004) (Dupré 2006, p. 30).

I see four possible ways of defending the pragmatic view. The first is to assume that (at least certain features of) the periodic system is still open to debate. The second is like Dupré to provide a kind of ad hoc explanation for chemistry: The pragmatic nature of the periodical system is related to the purpose of chemistry, which is the structural analysis of matter. The third is to operate with very general purposes for the sciences, in which case an ideal classification can be understood as the best tool with which mankind can control nature. The fourth is to question the generality of the periodical system's organization of "similar" elements. Chemists are often organized according to pragmatic categories such as agrochemistry, food chemistry, fuel chemistry, pharmacology and toxicology. The periodical system (a "cognitive classification") seems to be somewhat opposed to such "social classifications" of chemists, thus indicating a limit to the prediction of properties.

The properties of objects are not arbitrarily distributed. On the basis of some properties in an object, other properties may be predicted. The atomic number is a strong predictor of basic chemical properties (like the DNA is a strong predictor of biological properties). Thus atomic number and DNA may be considered criteria of natural kinds. Whether or not they are the most relevant criteria in a given classification is another question. Not all properties are predicted by atomic number or DNA, for example. For some purposes, other classification criteria may be more useful.

Concerning (3). Empiricist, rationalist, historicist and pragmatist views can be traced as competing views in relation to the periodical system. This is most clear in relation to the understanding of an "element". Throughout the book, Scerri discusses two ways of understanding chemical elements: as "basic substances" and as "simple substances", which correspond respectively to a rationalist and an empiricist view. According to Scerri, "it is difficult to fully understand the classification of the elements without first attempting to understand what an element is and how such a concept has changed over time" (p. xv). This consideration of conceptual developments in the understanding of the periodical system (often associated with teaching chemistry) is an indication of the importance of the historicist view (again, the consideration of Hegel's view might contribute to strengthening of this view because Hegel is a leading figure in the criticism of empiricism and rationalism). In the chapter about the evolution of the elements, it is stated that "[t]he elements are now believed to have literally evolved from hydrogen by various mechanisms" (p. 250), which also indicates that a historicist metaphysics and epistemology are at play. Finally, the pragmatist view can, for example, be seen in the weight attributed to chemical respective physical properties when determining the "similarities" among the elements. Scerri's view about whether the periodical systems should be explained (and thus reduced to) quantum mechanics alone or whether chemistry has interests of its own can thus be viewed as an indication of the role of a pragmatic philosophy in the development of the periodical system.

Concerning (4). Has the periodical classification influenced the way in which chemical substances are classified in library classification systems, thesauri, etc.? In fact, it can be traced in the UDC (Universal Decimal Classification) and the MEDLINE database. It seems rather obvious that the concepts and criteria used to organize information in library and information science are first developed in other fields—such as chemistry. This, however, is seldom reflected in the methodology of knowledge organization. As already stated, books like Scerri's seem to be ignored in our field.

It should be mentioned that in library and information science, the periodical system was dismissed as a classification system by Hulme (1911), originator of the principle of "literary warrant." Hulme wrote:

> In Inorganic Chemistry what has philosophy to offer? [Philosophy here means science, which produced the periodical system]. Merely a classification by the names of the elements for which practically no literature in book form exists. No monograph, for instance, has yet been published on the Chemistry of Iron or Gold.
> …

Hence we must turn to our second alternative which bases definition upon a purely literary warrant. According to this principle definition is merely the result of an accurate survey and measurement of classes in literature. A class heading is warranted only when a literature in book form has been shown to exist, and the test of the validity of a heading is the degree of accuracy with which it describes the area of subject matter common to the class. Definition [of classes or subject headings], therefore, may be described as the plotting of areas pre-existing in literature. To this literary warrant a quantitative value can be assigned so soon as the bibliography of a subject has been definitely compiled. The real classifier of literature is the book-wright, the so-called book classifier is merely the recorder (Hulme 1911, 46–47)

Hulme's principle of literary warrant seems not to conflict with the way in which the periodical classification has been used in systems like UDC and MEDLINE: if there is no warrant for a given element, the broader category may be applied. However, this issue points to some vagueness in the concept of "literary warrant."

Conclusion

Scerri's book demonstrates how one of the most important classification systems has evolved and what kinds of conceptualizations and classification criteria are at work in it. It is probably *the best book about the best classification system ever constructed*. It should belong to any library supporting teaching and research in knowledge organization.

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Professor Birger Hjørland, Royal School of Library and Information Science, DK-2300 Copenhagen S, bh@db.dk

Marc Ereshefsky. *The Poverty of the Linnaean Hierarchy: A Philosophical Study of Biological Taxonomy*. Cambridge: Cambridge University Press, 2007. x, 316 p. ISBN-13: 978-0-521-03883-6.

This book was published in 2000 simultaneously in hardback and as an electronic resource, and, in 2007, as a paperback. The author is a professor of philosophy at the University of Calgary, Canada. He has an impressive list of contributions, mostly addressing issues in biological taxonomy such as units of evolution, natural kinds and the species concept.

The book is a scholarly criticism of the famous classification system developed by the Swedish botanist Carl Linnaeus (1707–1778). This system consists of both a set of rules for the naming of living organisms (biological nomenclature) and principles of classification. Linné's system has been used and adapted by biologists over a period of almost 250 years. Under the current system of codes, it is now applied to more than two million species of organisms. Inherent in the Linnaean system is the indication of hierarchic relationships. The Linnaean system has been justified primarily on the basis of stability. Although it has been criticized and alternatives have been suggested, it still has its advocates (e.g., Schuh, 2003). One of the alternatives being developed is *The International Code of Phylogenetic Nomenclature*, known as the *PhyloCode* for short, a system that radically alters the current nomenclatural rules. The new proposals have provoked hot debate on nomenclatural issues in biology.

Ereshefsky's book is organized into three parts and eight chapters:

A good starting point is Chapter Six, in which it is stated that Linné's system was based on the assumption that plants have two vital functions: "nutrition which preserves the individual, and reproduction which preserves the kind. To know what kind a plant is one needs to study its function in reproduction, in particular, those parts that play a role in its reproduction" (p. 202). This was Linné's main reason to focus on reproductive organs in classifying plants. Another factor in his decision was that "fructification characters are easy to work" with because they are the "most complex organ-system of plants" and "provide a large number of characters" and "can be described with precision" (p. 202). Linnaeus used thirty-one sexual characteristics and four variables, which he calculated would "suffice for 3,884 generic structures or more than will ever exist."

He [Linné] often lacked representatives of all species of a genus and thus was unable to determine the

unique fructification system of a genus. Given the method of logical division, a classification system cannot be considered real or natural unless the true fructification systems of genera are determined. Consequently, Linnaeus saw his classification as artificial and provisional guides for yet-to-be determined true classifications (p. 204).

Ereshefsky finds that three (false) theoretical assumptions serve as the foundation of the Linnaean system: "creationism, essentialism and the belief that genera are the most important taxa in his hierarchy" (p. 205). Much of the book is an examination of how these assumptions shaped the system and how alternatives of these assumptions should inform an alternative system. Ereshefsky finds that from the perspective of modern biology the "only element of Linnaeus's original system that remains firmly intact is his binominal rule for naming species. But that rule, as we shall see, may need to be altered as well" (p. 221).

Essentialism is a target for much criticism in scientific classification today. This review will not go too deeply into the controversies here, but provide some summary. Mayr (1997, 128) writes:

> The typological or essentialistic species concept […] postulated four species characteristics: (1) species consist of similar individuals sharing in the same "essence;" (2) each species is separated from all others by a sharp discontinuity; (3) each species is constant through space and time; and (4) the possible variation within any one species is severely limited.

Mayr adds: "Philosophers referred to such essentialistically conceived species as 'natural kinds.'"

While such an understanding of essentialism is clearly refuted by Darwinism, it is less certain that the following definition of essentialism (pp. 23 & 95) is also obsolete:

> All and only the members of a kind share a set of traits; those traits make entities the kinds of things they are; thus, those traits are crucial in explaining the other properties typically associated with the members of a kind.

Cooper (2005, 47) summarized the central problem as follows:

> Several different criteria may be employed by biologists seeking to determine species: morphological futures, evolutionary lineages, the criteria of reproductive isolation, or genetic features. On examination none of these appears suitable candidates for being the essential properties of biological species.

Ereshefsky provides a good argument as to why essentialism is in conflict with a theory of (slow) gradual evolution and thus must be rejected in biology (p. 95–96). On the other hand, he seems to accept essentialism in chemistry (p. 17):

> Mendelev's periodic table is often cited as a model for essentialism. All and only the members of a particular element share a common real essence — their unique and common atomic structure. And knowledge of that structure enables us to predict and explain the behavior of instances of that element.

It is one matter to define essentialism and to judge whether or not it constitutes a problematic basis of classification. (Most philosophers today reject essentialism.) Quite another issue is whether Linné's system is based on essentialistic thinking. Müller-Wilhle (2007, 541) finds that the criticism that Linné was an essentialist is a misunderstanding:

> Historians and philosophers of science have interpreted the taxonomic theory of Carl Linnaeus (1707–1778) as an 'essentialist', 'Aristotelian', or even 'scholastic' one. This interpretation is flatly contradicted by what Linnaeus himself had to say about taxonomy in *Systema naturae* (1735), *Fundamenta botanica* (1736) and *Genera plantarum* (1737) … (1) Linnaeus's species concept took account of reproductive relations among organisms and was therefore not metaphysical, but biological; (2) Linnaeus did not favour classification by logical division, but criticized it for necessarily failing to represent what he called 'natural' genera; (3) Linnaeus's definitions of 'natural' genera and species were not essentialist, but descriptive and polytypic; (4) Linnaeus's method in establishing 'natural' definitions was not deductive, but consisted in an inductive, bottom-up procedure of comparing concrete specimens.

Thus, a major line of argument in Ereshefsky's book seems to be based on a controversial interpretation of Linné's principles.

Chapter Seven considers post-Linnaean taxonomy and contains a number of recommendations for changes, such as R11: "Where possible, taxon names should be given phylogenetic definitions" (p. 266). All the suggestions are made in order to provide a classification of organisms based on modern biological research. In this review, we shall not further consider the different alternatives in biological taxonomy today, but rather concentrate on the complex of problems involved in scientific classification, which is the main theme of the first two parts of the book.

Part II (Chapters Four and Five) concerns pluralism. Ereshefsky argues for the view of metaphysical pluralism, i.e. that the forces of evolution produced at least three different types of base lineages (interbreeding, ecological and phylogenetic) that cross-classify the organic world, which is why a plurality of equally legitimate classifications exists. Science should "carve nature at its joints", but perhaps the world is carved in multiple ways, each corresponding to a particular taxonomic approach. Biologists face different commitments to various rules, each of which motivates different avenues of research and different classifications.

Part I is termed "The Historical Turn". It refers to what might be understood as a paradigm shift in classification theory. I believe, however, that it would have been better to have given Part I the title "The Philosophy of Classification", which reflects the different views presented and not just the new paradigm.

In Hjørland (2003, 107) and elsewhere, this reviewer has argued that four basic philosophies of classification correspond to four basic epistemological schools: empiricism, rationalism, historicism and pragmatism.

Ereshefsky seems not to defend a particular epistemological position in relation to biological taxonomy. On the other hand, he seems to be in accordance with the reviewer's rejection of empiricism and rationalism as defined above. Overall, the book seems to confirm the reviewer's epistemological understanding in this domain, underscoring the different paradigms at play in modern biological taxonomy: "Contemporary biology contains no fewer than four general schools of taxonomy: evolutionary taxonomy, pheneticism, process cladism, and pattern cladism" (p. 7). In many ways Ereshefsky, seems to confirm the reviewer's view, although this conclusion must be drawn by inference.

Ereshefsky's presentation of logical division (termed essentialism) as a method in knowledge organization seems to correspond to rationalism and his presentation of cluster analysis and of pheneticism, which divides entities into groups whose members share a cluster of similar traits, corresponds to empiricism.

In Ereshefsky's use of the term, a system following "the historical approach" classifies entities according to their causal relations rather than their intrinsic qualitative features. This corresponds only partly to historicism in epistemology. What Ereshefsky terms "the historical approach" Gnoli (2006) terms "phylogenetic classification" (which, according to Gnoli, includes the classification of musical instruments). Perhaps "genetic classification" or "genealogical classification" would be a better term (understood broadly as the identification of the causes producing a phenomenon, as Michel Foucault uses it). My point here is that Ereshefsky's use of the term "historical" only refers to the object of study, not to the researchers' way of understanding the object (as reflected in, for example, the hermeneutic circle and in Fleck's (1935) study of syphilis). If Ereshefsky had argued that it is necessary for the biological taxonomist to consider the different conceptions and theories (as, for example, those presented in his own book), historicism would be at work. For Ereshefsky's book to correspond to epistemological historicism properly speaking, this additional reflection on theory would be necessary.

Finally, let us consider pragmatism in relation to this book. It is worth mentioning that pragmatism evolved out of the evolutionary biological view, which is why this view should not be strange or unfamiliar to biologists. Ereshefsky carefully considers the purpose of classification and also, at several points, the practical issues related to classification. But at a deeper level, the pragmatic view is connected to the argument that a certain way to classify organisms is in accordance with certain goals of biological research. Perhaps Ereshefsky's defence of metaphysical pluralism can be seen as an attempt to answer to the overall interests of biology. If only one of the three different types of base lineages mentioned (interbreeding, ecological and phylogenetic) were considered, this might have negative implications for biological science. For example, interbreeding works only for the minority of organisms which have sexual reproduction. If this definition of species was the only one used, the systematics of all non-sexual organisms would suffer. If this argument is acceptable, Ereshefsky can be interpreted as being a pragmatist.

In the rest of this review, I shall consider some implications this book may have for knowledge organization (KO) and library and information science.

Consider, for example, the principle of logical division, which is a basic method in the facet analytic tradition. As Vickery writes (1960, 12): "Facet analysis is therefore partly analogous to the traditional rules of logical division, on which classification has always been based." Ereshefsky criticizes this method throughout the book. He writes, for example: "If taxa lack essences, then the method of logical division has no role in classification—there is no essences on which it can operate. Accordingly, that method has been dropped from biological taxonomy" (Ereshefsky, p. 211). Furthermore, he notes (p. 296):

> Mayr (1982, 174, 179, 191) and Atran (1990, 108), for instance, offer a historical case against the method of logical division. According to that method, entities are sorted into a hierarchy of classes such that each class is subdivided into two [sic!] lower classes by a set of differentiating properties [= a criterion of division?]. As Mayr and Atran observe, the method of logical division breaks up natural groups (see Section 1.1). Thus that method fails to provide empirically accurate classifications that serve as a basis for making inferences about the organic world.

(That a class be subdivided into two lower classes is not a requirement. I believe that Ereshefsky has made a mistake at this point. One logical principle of division, e.g. division by age, may result in a number of classes, not just two. In the index of the book, the method of dichotomous division is considered equivalent to the method of logical division, which seems to be inconsistent with what is written about Linné's use of this method on p. 201–202.)

Insofar as Ereshefsky's criticism of logical division is valid, the whole school of facet-analysis in library and information science seems to suffer, for logical division is the basis of facet-analysis. At least, it seems important for information science to reconsider its approach in light of the recent developments in scientific classification.

Another implication of Ereshefsky's book is a problematization of Mai's (2004) understanding that the classification of documents is distinct from the classification of biological organisms (and from physical objects). Mai (2004, 41) maintains:

> Scientific classification and logical division has [sic] worked fairly well in the classification of natural kinds, such as Linnaeus' classification of living things. The reason is that the characteristics chosen, such as the shape of a fruit, are easy to perceive and describe. Furthermore, all biologists and botanists would agree on the interpretation of the characteristics (Lakoff, 1987). Such taxonomies do not intend to analyze the meaning of the terms, but are merely classifications of kinds of things. The chosen characteristics by which the genus is divided into genera are properties of the things classified and the characteristics are subject to inspection. However, the users of such taxonomies know that the use of the classification requires some sort of interpretation. That is why a zoologist would not dispute a statement like "this cat has three legs," since he knows that there can be handicapped cats. He would still classify cats as four legged mammals and he would still say that the property of being four-legged belongs to cats, but he would not say that cats are four-legged necessarily or analytically (Eco, 1984). In other words, nothing specific is said about individual cats in such a classification.

And further (Mai 2004, 42):

> It is my contention that scientific classification of natural objects, and the bibliographic classification of the content of a document, are distinct for two main reasons. The first has to do with when and how the items are classified, and the second has to do with the nature of the classified items.

Ereshefsky claims in many ways the opposite of what Mai expresses above. He does not find that "Scientific classification and logical division has [sic] worked fairly well in the classification of natural kinds" or that criteria for classify organise "are easy to perceive and describe". Mai's claim "that scientific classification of natural objects, and the bibliographic classification of the content of a document, are distinct" seems also problematic because each school of biological taxonomy has different criteria, which may be applied to both organisms and their descriptions in documents. It should be mentioned, however, that Mai himself also questions some of the cited assumptions based on Broadfield (1946).

A third and final consideration for knowledge organization is the distinction made between classification and categorization. Jacob (2004, 15) contrasted classification with categorization and defined "classification" in a restricted way that does not account for Ereshefsky's "three general philosophical schools [of classification] […]: essentialism, cluster analysis, and historical classification." Although Jacob claims that Ereshefsky misuses the term "classification", thus confusing "classification" and "categorization," we might ask for textual evidence showing that Ereshefsky's terminology is faulty. My own feeling is that it is not.

Conclusion

Ereshefsky (2000) has been cited once in this journal. The citation concludes (Gnoli 2006, 144):

To summarize what we have seen in various domains, classification can be based on two major principles: similarity, and common origin.

Gnoli here seems to have overlooked the fact that Ereshefsky (2000) discusses three major principles: logical division based on essential characteristics, cluster analysis based on similarity measurement and historical classification based on common ancestors. (He has also overlooked that Hjørland (1998 and 2003) discusses four major principles of classification based on, respectively, empiricism, rationalism, historicism and pragmatism.)

I believe that Ereshefsky's book has much to offer to KO and that we really need to consider the literature of scientific classifications.

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Professor Birger Hjørland, Royal School of Library and Information Science, DK-2300 Copenhagen S, bh@db.dk

Rachel Cooper. *Classifying Madness: A Philosophical Examination of the Diagnostic and Statistical Manual of Mental Disorders*. Berlin: Springer, 2005. vii, 172 p. (Philosophy and Medicine, vol. 86). ISBN: 978-1-4020-3344-5 (hbk.). (Also available as electronic "Kindle" book from Amazon.com).

The author, Rachel Cooper, Senior Lecturer at Lancaster University, holds a Ph.D. in History and Philosophy of Science from Cambridge University. The title of her thesis is also *Classifying Madness*.

Classifying Madness: A Philosophical Examination of the Diagnostic and Statistical Manual of Mental Disorders concerns a particular classification system for mental disorders, the DSM, published by the American Psychiatric Association. The DSM is the classification system used most often in diagnosing mental disorders in the United States. Although the International Classification of Diseases (ICD) is a commonly-used alternative outside the U.S., the DSM still holds immense weight internationally. Today, the DSM has almost the status as a bible within

the psychiatric community and has been used to challenge the pervasive criticism that psychiatric diagnoses are unreliable and invalid. The first edition (DSM-I) was published in 1952. The DSM-II was published in 1968, the DSM-III in 1980 and the DSM-III-R as a revision in 1986. The current fourth edition, the DSM-IV was first published in 1994. A text revision known as DSM-IV-TR appeared in 2000. Work on a new fifth edition is underway.

Rachel Cooper structures the bulk of her argument in five parts: (1) What is Mental Disorder?, (2) Are Mental Disorders Natural Kinds?, (3) The Problem of Theory-ladenness, (4) The D.S.M. and Feedback in Applied Science, and (5) Conclusions. *Classifying Madness* also contains an appendix, bibliographic references and an index.

1 What is Mental Disorder?

Just as any system of knowledge organization is always, implicitly or explicitly, consciously or unconsciously, based on an understanding (or "theory") of the domain it organizes, the DSM is based on an understanding of what mental disorders are. An important part of constructing or evaluating classifications is to examine such understanding.

This book shows how the assumptions behind a classification system can be examined and, in being made explicit, used fruitfully towards improvements in the classification of the domain. The chapter claims that the DSM is based on an incorrect understanding of disease, however (p. 41):

> The account of disease used by the D.S.M. committee in practice, I suggest, was not far wrong. This being said, there may be reason to doubt the extent to which decisions to include particular conditions in the D.S.M. were influenced by accounts of disease.

The chapter provides fine arguments for an explicit and consequent account of disease and concludes (p. 43): "I have argued that whether a condition is a disease is in part a value-judgement. As doctors are not experts in making value-judgements, it follows from my account that it not appropriate for them alone to have a say in deciding which conditions are diseases."

2 Are Mental Disorders Natural Kinds?

The problem of "natural kinds" is important for classification theory because it contains the idea that classifications are not made for a purpose, but reflect an underlying natural order. Cooper writes (p. 47):

In recent years traditional essentialist accounts of natural kinds have come in for fierce criticism. A major difficulty is that for biological species, which are traditionally considered amongst the best examples of natural kinds, no plausible candidates for the essences can be found. Several different criteria may be employed by biologists seeking to determine species: morphological features, evolutionary lineages, the criteria of reproductive isolation, or genetic features. On examination none of these appears suitable candidates for being the essential properties of biological species.

One of the theories discussed is John Dupré's theory of "promiscuous realism," according to which classifications may reflect a real structure of nature (hence their "realism"), but that many different classification systems can be extracted from a given pattern without any one of them being privileged over the others (hence their "promiscuity"). Cooper has developed her own theory about natural kinds (p. 51): "I suggest that the right account of natural kinds claims that members of a natural kind possess similar important properties. These important properties are important because they determine many of the other properties possessed by members of the kind. For this reason I will call them 'determining properties.'" On page 72 she provides a specific example: "Huntington's Chorea is caused by a single dominant gene on chromosome four. Symptoms generally appear in middle-age and include jerky involuntary movements, behavioural changes, and progressive dementia. Plausibly Huntington's Chorea is a natural kind of mental disorder; in all cases an identical determining property, the defective gene, produces characteristic symptoms." The author warns, however (p. 74): "It should be remembered that classification systems should not only provide information about the entities they categorise, but also need virtues that will enable them to be used in practice. In some cases it may be best to reflect the natural structure of a domain, in other cases it will be better to employ categories that make sharp divisions where naturally there are none." She concludes, that even if some mental disorders are natural kinds (p. 76):

> There may be difficulties constructing a classification that reflects the natural similarities between types of mental disorders. In the next two chapters two potential sources of difficulty will be considered. These arise from the possi-

bility that observation in psychiatry is theory-laden, and from the fact that the D.S.M. is shaped by pressures emerging from the various ways in which it is used in practice."

3 The Problem of Theory-ladenness

This important chapter concerns the theory-ladenness of observations, as well as that of classifications. For people without knowledge of this philosophical problem, it may be hard to accept that our observations are not direct reflections of a true reality, but are influenced by the theories we have. This chapter does a very fine job in presenting the problem in a clear way and could be assigned as required reading in classes on classification and knowledge organization.

Although Cooper discusses at length the kind of theory relevant in discussing the theory-ladenness of the DSM system, I feel that she does not present a clear picture of which different metaphysical theories may be the most relevant ones. My on view is informed by, for example, Danziger (2000).I suspect that psychiatrists tend to focus more on symptoms, methods and criteria related to metaphysical theories such as atomism, universalism and decomposability, while disregarding, for example, the roles of language and cultural objects and thus more holistic and relativistic metaphysical assumptions. The positivist researchers claim to be anti-metaphysical, but in reality use implicit metaphysical theories that limit their perspectives. Relevant theoretical issues may be uncovered by considering underlying positivist assumptions in psychiatric research.

DSM-I and DSM-II were strongly influenced by the psychodynamic approach to mental disorders, but with DSM-III, the psychodynamic view was abandoned and the biomedical model became the primary approach, introducing a clear distinction between normal and abnormal. The DSM claimed to be *atheoretical* since it had no preferred etiology for mental disorders. When DSM-III was first published in 1980, it embodied a radical new method for identifying psychiatric illness. The most central problem for a theory of classification is how it is related to theories in its domain, for a system cannot be neutral with respect to those theories. The next section goes into this question in more detail.

Chapter 3, Section 4:
The Theory-ladenness of Numerical Techniques of Classification

At the end of Chapter 3, Cooper discusses the technique of cluster analysis and relates it to numerical techniques in general. This important section deserves a chapter of its own. The question here taken up could also be asked of research in information science and knowledge organization: are techniques such as bibliometrics and automatic indexing providing neutral, objective, atheoretical classifications?

Cooper says about this is valid. First, she finds that, although DSM is not based on cluster analysis to any extent worth mentioning, it succumbs to presuppositions implicit in the latter (p. 96):

> The numerical taxonomy movement in biology made much of the supposed "objectivity," "empiricism," and "naturalness" of the classes produced. Similarly, the D.S.M.-III committee called for a rejection of theory-based classification on the grounds of the paucity of theoretical knowledge. Like the Numerical Taxonomists, they also aimed at a classification system constructed on empirical, atheoretical grounds.

Cooper's most important conclusion is that one cannot select empirical variables for numerical techniques for classification without a basis in domain-specific theory. The arguments are mostly based on thought-experiments, however, and not upon empirical studies. I believe, nonetheless, that in this her reasoning is sound. Firstly, such techniques have been used very much (e.g. in intelligence research) and no clear pattern seems to have been established. Secondly, such studies appear to be based on unrealistic assumptions that disregard cultural factors.

4 The D.S.M. and Feedback in Applied Science

This chapter should prove the most stimulating for information scientists. It not only relates how the DSM is used in different kinds of practice and explains why the growth in use has been tremendous, but also investigates the impact of its application on the system. Its wider influence has also meant that psychiatrists have succeeded in controlling the ways in which other professionals such as psychologists and social workers see and do things. The pharmacological industry, as well as the insurance industry, has had much influence. Cooper shows how social

interests and pragmatic factors influence a classification that claims to be purely scientific.

Relevance for LIS

When a system becomes as powerful as the DSM has, other systems of knowledge organization come under pressure to adapt to them. For example, the *Clinician's Thesaurus* (Zuckerman, 2000), which is more like a handbook than a traditional thesaurus, was described as follows in the publishers advertising:

> Clinician's Thesaurus helps mental health practitioners find the right words to describe their clients quickly and accurately. The new edition of this popular guidebook has been updated and expanded and is fully compatible with DSM-IV. It offers an exhaustive checklist of thousands of words and phrases in an easily accessible format—in effect, the whole language of the mental health professions. Enabling practitioners to quickly select the appropriate terms to describe almost every clinical situation, it makes constructing meaningful reports easier than ever before.

Similarly, the *Thesaurus of Psychological Index Terms*, 5th edition, claimed to reflect the DSM (Walker and Mulholland 1992, 48):

> With the publication of the third revised edition of the *Diagnostic and Statistical Manual of Mental Disorders* (DSM-III-R), all index terms in the psychological disorders area were reviewed. A major reorganization and reconstruction of index terminology for mood disorders, schizophrenias, psychoses, and anxiety disorders was completed. Most disorder terms now reflect changes in diagnostic categories represented by the DSM-III-R.

[This information was not given in the Thesaurus, American Psychological Association, 1988. In *PsycINFO News*, Vol. 20(3), p. 3, it is mentioned that the 9th ed. of the thesaurus has harmonized mental disorders terms with current DSM-IV terminology. Again, this information is not provided in the thesaurus itself or in any scholarly information source, but only in the more commercial-oriented documents. I believe that the *Thesaurus of Psychological Index Terms* runs into difficulties by trying to adjust their terminology to the DSM.]

"Scientific classifications" are clearly relevant for "bibliographic classifications", thesauri and other kinds of knowledge organizing systems. This important connection is, however, often forgotten in LIS-contexts. One reason might be that the literature about scientific classification is too technical and difficult.

Knowledge-organizing systems are made to serve goals, interests and values. They can only do so properly with consideration of the kinds of problems revealed by *Classifying Madness*. This applies to the development of ontologies, which have become a strong trend: Cooper's book would be of equal interest to information and computer scientists developing ontologies of mental diseases. If information scientists are unfamiliar with these issues, they cannot influence their own systems in a conscious way. The DSM has formerly been considered within our field (e.g. Spasser, 1998).

Conclusion

The literature on the DSM is huge. However, *Classifying Madness* remains particularly clear and articulate in its analysis of the DSM's conceptual underpinnings. Furthermore, it is important in illuminating some core issues in classification theory as they present themselves in the case of mental disorders. Often books about classification in specific disciplines are very technical and difficult, but *Classifying Madness* is comprehensible, even to those without specialized knowledge in psychiatry or philosophy—although some philosophical background would probably provide the patience necessary to read through the complicated details of classification problems. This book is not too specialized for information science students, either: knowledge gained by *Classifying Madness* can be transferred and used to question other classification systems.

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Professor Birger Hjørland, Royal School of Library and Information Science, DK-2300 Copenhagen S, bh@db.dk