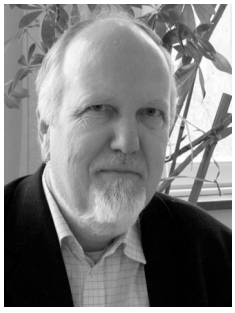
Forum: The Philosophy of Classification

The Periodic Table and the Philosophy of Classification

Birger Hjørland



1.0 Introduction: This debate

Thanks to Professor Eric Scerri for engaging in debate in this journal (Scerri 2011) by replying to my review (Hjørland 2008a) of his book (Scerri 2007). One of my points has been that we in our community (Knowledge Organization, KO / Library and Information Science, LIS) have been too isolated from broader academic fields related to classification and the organization of knowledge. The present debate is a step towards reversing this situation. Strangely enough, at the end of his reply, Scerri (2011) seems to question (even) this view. In this connection he seems, however, to confuse two different things: 1) I have never said that the periodic system has not influenced LIS-classifications. On the contrary, I mentioned in my review two examples (MEDLINE and UDC) which are clearly influenced by the periodic system. 2) What I did say was that books such as Scerri's—and the broader field of the philosophy of classification—are mostly ignored by scholars in KO/LIS. That people in KO seem to consider themselves "the professionals" in classification (cf. Beghtol 2003), but that the relation to the philosophy of classification—as well as to specific scientific classification research—needs to be strengthened (cf., Hjørland & Nicolaisen 2004 and Nicolaisen & Hjørland 2004).

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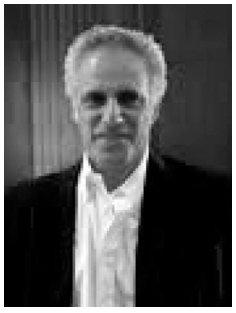
What is the Nature of the Periodic Table as a Classification System?

Eric Scerri



I would like to thank Professor Birger Hjørland for his generous review of my book on the periodic table of the elements (Hjørland 2008). As he states, the periodic table represents perhaps the strongest claim for a natural classification that one can find in any discipline. As such it is worthy of the attention of scholars of classification and knowledge organization in general. I in turn wish to compliment Hjørland for promoting this interdisciplinary activity via his review and many previous writings on the subject. I think that there is much that can be learned from taking this debate a little further.

I now turn to some specific responses to his book review. Hjørland claims that classification should be, and is in fact, based on pragmatic criteria and thereby insists that even in the case of the periodic table, classification is pragmatic rather than 'natural.' He also claims that the classification of knowledge can be carried out by using one of four approaches that he identifies as empiricism, rationalism, historicism and pragmatism, of which he believes the last to be the most "advanced theory." Hjørland concedes (2008, 253) that the periodic system is: "probably one of the most difficult classification systems to defend from a pragmatist point of view," but adds that: "it is also important to test our views against the most pre-eminent classifications if our arguments should be convincing."

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A Note on the Debate Between Hjørland and Scerri on the Significance of the Periodic Table

John Dupré

I am very grateful to Professor Birger Hjørland for giving me the opportunity to comment on his exchange with Professor Eric Scerri on the periodic table of the elements. However, since Hjørland himself has provided a detailed commentary on Scerri's response to his (Hjørland's) review of Scerri's book on the topic, and since I am not an expert on the philosophy of chemistry, I shall limit myself to a few comments on what Scerri says about my own views. (In fact one paragraph in a two page article on scientific classification, and one footnote from my 1993 book, *The Disorder of Things*, most of which are quoted in the discussion to date, as far as I can recall exhaust my published writing specifically on the topic of the periodic table.)

Scerri suggests that I am guilty of circularity in assuming that chemists aim at a structural analysis of matter and then find a classification based on structural elements suits this purpose. It seems to me that my assumption might be false, but hardly justifies a charge of circularity. I am happy to defer to Scerri's far greater expertise if he tells me it is false. However, I cannot accept his claim that chemists aim for "the analysis of matter period." I do not believe there could be any such thing. Matter has an enormous range of properties: macrostructural properties such as strength, elasticity or hardness; nutritional and toxicological

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Scerri points out that Hulme (1911) is outdated; I am *not*, however, supporting Hulme's view about the irrelevance of the periodic system (and the philosophy of classification); *on the contrary*, I disagree, like Scerri, with Hulme on this point—it was just mentioned to underline my view about the critical attitude towards the philosophy of classification in KO. That Hulme's paper is very old is an illustration of how difficult it is to identify more recent discussions about the periodic system in the literature of KO. Hulme was also named because he is known for the important principle of *literary warrant* in KO and as the founder of statistical bibliography, which is now known as bibliometrics.

If Scerri wishes to prove me wrong on this point, a proper argument would be to point to more recent texts and to demonstrate how writings about the periodic system, about other scientific classifications and about the philosophy of classification have influenced theory and writings in KO and LIS (e.g., checking citations in LIS to this literature, e.g., by using *Web of Science*). I do not believe Scerri knows the literature of KO/LIS and that his denial of my claim about the neglect of the larger field of classification research in KO is valid. Much more relevant is Scerri's view about the nature of the periodic system (or periodic systems) and the implications for the theory of classification, which are considered below.

To say that a kind is *natural* is to say that it corresponds to a grouping or ordering that does not depend on humans.

2.0 The emerging field "the philosophy of classification"

Classification research is not just done within KO/LIS. Below are listed some important scholars in the interdisciplinary field which may be termed "philosophy of classification" (or "theory of classification," "classification theory," "classification research," "the science of classification," etc.). There are some philosophers who work with classification theory in general (e.g. Bunge 1983; Dupré 1993; Sutcliffe 1993; Bryant 2001). A few philosophers who tend to specialize in the

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He presents his first line of attack on the notion that the periodic system classifies the elements as natural kinds in the following way. Hjørland points out that although there may only exist one periodic law, there have been over 700 periodic tables published, which depend upon the particular pragmatic interests of any designer. My response to this point would be to shift the discussion to the periodic law rather than periodic tables, which as I am sure Hjørland recognizes are attempts to capture the more abstract periodic law. I do not believe that the failure of chemists to arrive at one commonly agreed table, or representation, should be taken to mean that the elements themselves are not natural kinds or that periodic classification is inherently of a pragmatic nature. It may just be that the current attempts at representation are infused with pragmatism since individual chemists may indeed be interested in putting the periodic table to particular uses rather than arriving at one correct representation that reflects a natural classification. It is rather to the philosophy of chemistry, I suggest, that one should look for the more general nature of the periodic law and the periodic table (Scerri 2009).

More philosophically speaking, the identity and properties of any class of entities are somewhat separate issues

Hjørland turns to denying that elements are natural kinds more directly and quotes a recent book by Bryant as a source of support. Bryant (2001, 88) writes: "even in the case of chemical elements more than one kind of causal essentialism is scientifically legitimate." Hjørland seems to counter Bryant's claim by quoting from a review of her book in which Stamos has shown convincingly that she (Bryant) is mistaken. Hjørland nevertheless seems to side with Bryant and the pragmatist approach by further drawing upon the work of Dupré in order to adjudicate between Bryant and her critical reviewer, Stamos. Hjørland thus quotes from Dupré (2006) who is commenting specifically on the periodic table and who believes that there is: "much potentially wrong with the supposition that there is a right way of classifying things in the world," and that: "the standard paradigm for such a successful

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properties; aesthetic properties; and so on. If there is such a thing as "analysis period" it is, as the Greek etymology suggests, the breaking down of things into parts. Of course, chemists might have found that there were no such parts, that all kinds of stuff were homogeneous and unanalysable, in which case they would have had to content themselves with a rather unilluminating natural cataloguing of stuffs. The point is that there are many different enquiries that could be launched about matter, and chemists are interested in matter from a specific, perhaps fundamental in some sense, perspective.

This is the point about jade—not that chemists don't distinguish jadeite and nephrite, but that from another perfectly respectable perspective they need not be distinguished, thus showing that the perspective of chemistry is not the only one possible on matter.

If gemology seems too unscientific an activity to be relevant, one need only look to geology for classifications of material stuff that follow a different path from those in chemistry. The mineral olivine, one of the commonest substances on Earth, constituting a major part of the Earth's upper mantle, is referred to by the chemical formula $(Mg,Fe)_2SiO_4$. The parenthetical part of this formula indicates that olivine is a variable mixture of forsterite (Mg_2SiO_4) and fayalite (Fe_2SiO_4). Of course one can describe the chemical composition of olivine; I have just done so. But the reason that there olivine is a significant category is not that it is a particular mixture of chemicals, but that it plays a crucial role in a description of the Earth's structure and composition. (Compare brass or bronze, also variable mixtures of chemical substances—in this case elements—the significance of which derives entirely from metallurgy not chemistry.)

"It is a classification system which, once discovered, seems inevitable."

The other major component of the upper mantle is referred to as pyroxene, a large group of minerals with the general formula $XY(Si,Al)_2O_6$, where X and Y can be any of a wide range of cations, and (Si,Al) refers to a variable quantity of silicon and aluminium. Many specific pyroxenes can be further identified and have more precise chemical compo-

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classification of a specific field, e.g. biology (Ereshefsky 2000; Hull 1998) or mental diseases (Cooper 2005). Then there are some scientists approaching the field from an interest in classification problems in their respective domains (e.g., in chemistry: Scerri 2007; in biology: Mishler 2000; Stevens 1994; in social science: Marradi 1990 and Wallerstein 1996). Then there are researchers approaching classification from mathematical, statistical or methodological angles (e.g., in mathematics: Mirkin 1996; in statistical clustering: Hartigan 2001). Also many researchers in psychology, sociology, linguistics and anthropology study how children and adults, languages, social groups and cultures classify the world (e.g., in psychology: Keil 1989).

We have also KO researchers such as Anderson (2003), Beghtol (2010), Gnoli (2006), Hjørland (2008d), Miksa (1994) and Szostak (2004) approaching classification from the perspective of library and information science, i.e. researching methods for arranging books on shelves, for the construction of subject catalogs, for information retrieval in bibliographical databases, for the construction of knowledge organizing systems (KOS). This field may be named *bibliographical classification*. It is about the classification of recorded knowledge (documents in a wide sense). Beghtol (2010) uses the term "knowledge organization classification" for this field, but LIS specialists are not the only professionals to deal with knowledge organization. Knowledge organization is primarily studied by scientists such as Scerri (the periodical system) or by philosophers, or by sociologists. When LIS professionals classify a given book, the concepts used are derived from the literature, not concepts and relations primarily constructed by LIS-professionals. As Hulme (1911, 46-47) said: "The real classifier of literature is the bookwright, the so-called book classifier is merely the recorder."

Bibliographic classification has now expanded into concerns with technologies of automated classification and to principles of ontologies, bibliometric maps, folksonomies etc. Many computer scientists are also contributing to this field (not to say dominating it), among them Sebastiani (2005); and some philosophers such as Barry Smith (2004) are working in the intersection between computer science and philosophy in relation to principles of ontologies.

There are many more researchers in classification theory than those listed above. I have just tried to provide examples, not an exhaustive listing (there is a need for review articles in this interdisciplinary domain).

The periodic system has a privileged place in the philosophy of classification. Hubert Feger, for example, wrote (2001, 1967-1968; breaks with hanging indentations added):

> A well known, still used, and expanding classification is Mendelejew's *Table of Elements*. It can be viewed as a prototype of all taxonomies in that it satisfies the following evaluative criteria:
>
> (a) Theoretical foundation: A theory determines the classes and their order.
> (b) Objectivity: The elements can be observed and classified by anybody familiar with the table of elements.
> (c) Completeness: All elements find a unique place in the system, and the system implies a list of all possible elements.
> (d) Simplicity: Only a small amount of information is used to establish the system and identify an object.
> (e) Predictions: The values of variables not used for classification can be predicted (number of electrons and atomic weight), as well as the existence of relations and of objects hitherto unobserved. Thus, the validity of the classification system itself becomes testable.

The examination of the periodic system may thus illuminate core philosophical and theoretical issues in the philosophy of classification, which have largely been ignored by KO/LIS. They include:

- The concept of "Natural kind"
- The theory-laden nature of observations and its implication for classification theory
- The relation between classification and subject theory (such as, for example, the relation between the periodic system and theory of quantum mechanics (QM) or the relation between the theory of evolution and biological taxonomy)
- Whether classifications are "correct" representations or more or less purposeful constructions
- The methodological basis for construing classifications (different epistemological ideals such as observation, logical analysis, historical reconstruction or pragmatic analysis).

I wish to make the following observations and conclusions of this section:

- The field of Philosophy of Classification is today very scattered and still rather limited as to the number of researchers and papers. It should be an obvious goal to get it more integrated (by mutual citations). In all modesty I see my own book reviews (among other writings) as an attempt to establish such a platform (e.g., Hjørland 2000, 2008a, 2008b, 2008c, 2009a).
- Classification was downgraded by logical positivism because it was regarded as unscientific compared with measurement (see Marradi 1990, §3.2.1). If this is correct, then the fight for classification implies also a fight against these positivist tendencies.
- KO would benefit very much if we were better integrated with scholars in other fields and if journals such as *Knowledge Organization* could present more of this interdisciplinary research. Hopefully our field may contribute by connecting different fields.

3.0 Epistemology and scientific methods

Any scientific classification has to be constructed and verified by some methods. Even if we agree on the superiority of a given classification (say the periodic table) we may still disagree in our interpretation of the scientific methods that ended up producing it.

Today there is no consensus concerning epistemology and scientific methodology. In the beginning of the 20th century, logical positivism was the dominant view. After 1962 Thomas Kuhn among others challenged this view. It has also been challenged by hermeneutics, pragmatism, feminist epistemology, critical theory, postmodernism and other views. My main point here is that we cannot defend any claim properly unless our arguments are based in an epistemology (that again needs to be defended). Therefore, we have to consider which epistemology we find useful. Other researchers may oppose a given view and forward an alternative view. The point is that there is no neutral position, and it is not possible to argue from a neutral platform or from nowhere. The choice of epistemology is not a question of personal taste. It is a question of scientific truth. Therefore the task of working out a proper epistemology is a collective responsibility that involves all scientists.

Scerri is obviously skeptical regarding my view that historicism and pragmatism are the best answers; however, he does not discuss any alternative: if he revealed his own position I would be in a better position to demonstrate why I think that historicism and pragmatism are better grounded.

The pragmatic position is related to hermeneutics (cf., Heelan & Schulkin 1998), which I regard as a philosophy with the potential of contributing to the successful practice of science. If a philosophy does not have this potential, it seems difficult to justify it. (However, Heelan (1998) in a paper about hermeneutics in natural science said that "we do not ask of a philosophy that it contribute to the successful practice of science." I disagree on this view, and I do think that Scerri also feels that his work on the philosophy of chemistry should indeed contribute to the development and to the successful practice of chemistry.) Kuhn's book (1962/1996) *The Structure of Scientific Revolutions* can be seen as an hermeneutic interpretation of the sciences because it conceives of scientists as governed by assumptions which are historically embedded and linguistically mediated activities organized around paradigms that direct the conceptualization and investigation of their studies. Scientific revolutions imply that one paradigm replaces another and introduces a new set of theories, approaches and definitions. According to Mallery, Hurwitz and Duffy (1992) the notion of a paradigm-centered scientific community is analogous to Gadamer's notion of a linguistically encoded social tradition. In this way hermeneutics challenges logical positivism. Observations are always made on the background of theoretical assumptions: they are theory dependent. It should be added that in my opinion pragmatism and hermeneutics are realist positions: People are developing their ideas in a world that exists independently. Pragmatism is at the same time fallibilist: No scientific method is able to guarantee the truth of knowledge claims.

Scerri's skepticism towards historicism and pragmatism is not unique. In an informal communication, the Swedish Professor Anders Ekholm wrote (30-07-2009 11:37, translated from Swedish. Eckholm's own view of classification can be seen in Ekholm (1996)):

> You identify four main traditions within epistemology: Empiricism, Rationalism, Historicism and Pragmatism. The first two are described by Bunge [1983a+b] as the two main views, while of epistemology the others should just be considered variations.… The discussion is interesting but I tend to follow the view expressed by Bunge that the first [historicism and pragmatism] after all are variations of the later [empiri-

> cism and rationalism]. This is by the way also what Bunge says about his own view, which he describes in detail in vol. 6 [Bunge 1983b] under the label Scientific Realism (vol. 6:255).

I cannot in this place provide a full analysis of the views of Ekholm and Bunge. It would certainly be a good idea if somebody made an article in this journal which presented and examined Bunge's view. In this place I can just say that I do not believe that historicism and pragmatism can be reduced to variations of empiricism and rationalism and that I consider the latter insufficient. Probably my best arguments have been put forwards in Hjørland (2009b).

Another criticism against (parts of) historicism could be based on a view that has been expressed by Cooper (2005, 48) based on Dupré:

> Nor can the essential property of a species be its evolutionary lineage. John Dupré [1981] shows this in his paper "Natural Kinds and Biological Taxa". Relationships of ancestry cannot be the essential properties of species because "Any sorting procedure that is based on ancestry presupposes that at some time in the past the ancestral organisms could have been subjected to some kind of sorting" (p. 88). The point is that in order to make sense of claims such as "Cats are the offspring of cats, while dogs are the offspring of dogs" one must have some way of distinguishing the ancestor cats from the ancestor dogs. Relations of ancestry are only of any use once the parent organisms have been sorted into kinds. As such, sorting on the basis of ancestry must always be a secondary, parasitic method of sorting. When we are seeking the essential properties of species it is thus more appropriate to look to the basis of the primary method of sorting, whatever it might be, rather than to relations of ancestry."

If this analysis is correct it seems to affect not just my view, but many others' as well, e.g., Gnoli's (2006) phylogenetic classification and Hull's (1998, 272), who wrote: "Two fundamentally different sorts of classification are those that reflect structural organization and those that are systematically related to historical development."

I cannot go much deeper into the specific implications for the scientific methods. The short reply is that the traditional views (empiricism, rationalism and their combination in logical positivism) are not satisfactory in describing scientific progress. Historicism and pragmatism (as I understand these positions) are more satisfactory interpretations of what eminent scientists have done. Another way to express this is, as, for example, Dupré (1993) says: Science is a human activity and in the end it is evaluated by its contributions for human beings. The implication is that a pragmatic factor is at play in science. On the other hand Kuhn (1962/1996) emphasized that nature cannot be forced into any conceptual structure that we provide. Nature makes resistance. Our conceptual structures therefore—in the long run—have to adapt to reality.

What is important for classification is that *different methods tend to provide different classifications*. If the different methods and epistemologies led to the same classification, there would be no problem, of course.

- Empirists' principles of classification emphasize: Classifications based on many properties which have been described in theory-independent ways;
- Rationalists' principles of classification emphasize: The classifications that are logical coherent and based on clear principles;
- Historicists' principles of classification emphasize: a) (on the side of the objects): the classifications that are systematically related to historical development of their elements;† and, b) (on the side of the subjects): classifications connected to explicit theoretical views;
- Pragmatists' principles of classification emphasize: The classifications that are best suited for the purpose for which they are intended. (In 6.0 below I'll introduce two different kinds of pragmatism).

If these different approaches provide the same classification, we could speak of "natural kinds" in a sense of that term. They often provide, however, different classifications, as demonstrated in, for example, biological taxonomy. How do we then decide which classification is the best? The pragmatic answer is that stability in our theories may arise after several iterations involving empirical, rational, theoretical and pragmatic *interacting* considerations as a historical process.

One of Scerri's objections to the pragmatic view was that the individual scientists are not motivated by pragmatic issues. This problem is addressed in 4.0.

> †Scerri (2007, 250) presents Prout's hypothesis, according to which all the elements are essentially made out of hydrogen, and he writes: "The elements are now believed to have literally evolved from hydrogen by various mechanisms." Karpatschof (2000, 105) wrote: "Paradoxically, the most basic of all sciences, the sciences of pre-biological matter (i.e., the disciplines of astronomy, physics and chemistry) were the latest to develop a theory of evolution… The idea of eternal, immutable laws is not easy to combine with the idea of development." A simpler explanation may be that our knowledge of the evolution of the universe since the Big Bang is very recent and that this knowledge is a prerequisite for an evolutionary theory in these fields.

4.0 Individualism versus collectivism

Scerri argues that scientists are not motivated by pragmatic factors, they just want to find the truth. He also wrote: "Mendeleev, the chief architect of the periodic system, repeatedly expressed his dislike for atomic theories...." Well, I believe that Thomas Kuhn has something important to say about this. He wrote (1996, 200):

> To understand why science develops as it does, one need not unravel the details of biography and personality that lead each individual to a particular choice, though that topic has vast fascination. What one must understand, however, is the manner in which a particular set of shared values interacts with the particular experiences shared by a community of specialists to ensure that most members of the group will ultimately find one set of arguments rather than another decisive.

The case for the pragmatic philosophy of science is not primarily about the individual motives of scientists. It is about whether there are pragmatic factors involved in theory acceptance in the scientific community. Whether, for example, astronomic theories were accepted because they helped construe better calendars? Whether medical progress is determined in part by its ability to cure people? In the case of the aim of chemistry: Whether there are divisions of labor between pure chemistry and, say, pharmacology, in which the first aims at describing more general features, whereas the latter, in particular, aims at describing medical effects and side effects? The claim made by John Dupré is that the periodic system may be extremely well suited to the tasks of *general* chemistry and that the general principle of the pragmatic nature of classification can thus be "saved."

When Scerri writes: "More importantly, Dupré's claim is somewhat circular. The aim of chemists is not necessarily the *structural* analysis of matter but rather the analysis of matter period" — I believe he disregards that *many* different sciences are studying "matter," and therefore we should try to find out how they differ—and consequently how each of them conceives and classifies the world.

In conclusion of this section: The individual scientists' view of the world is shaped in cultural-historical and disciplinary contexts which influence their criteria of, among other things, classification. The single scientist may be influenced by pragmatic factors whether or not this is admitted in his writings or whether or not it is a conscious choice. Traditionally it has been the ideal for science to be objective and free of pragmatic influences, and therefore such influences may be relatively hidden and thus hard to detect.

5.0 The concept of "element" in the periodic table

Scerri wrote: "Hjørland mentions the distinction between elements as simple substances (sense) and as basic substances (reference) but in a different context. As he sees it this distinction shows the coexistence of the empiricist and rationalist approaches to the classification of the elements. But in the very next sentence this dual nature in the meaning of the term 'element' also becomes an example of the importance of the historicist approach to knowledge, although no reason is given for this claim. "

Answer: I have formerly defined "concept" according to my pragmatic understanding (Hjørland 2009b, 1522-23):

> Concepts are dynamically constructed and collectively negotiated meanings that classify the world according to interests and theories. Concepts and their development cannot be understood in isolation from the interests and theories that motivated their construction, and, in general, we should expect competing conceptions and concepts to be at play in all domains at all times.

In the same paper I argue that empiricist, rationalist, historicist and pragmatic concepts may compete in

any domain at any time. That implies that the notion of "element" in chemistry may vary. Scerri (2007) discusses two conceptions of "element," as simple substances and as basic substances. My interpretation is that the first sense corresponds with the empiricist ideal of defining terms, the second perhaps with a rationalist ideal. It should not be difficult to see what I mean by the historicist and pragmatist view if you read my papers. It should not be difficult to follow the above definition of concept and apply it to "element." The reason for my claim that Scerri asks for is this: Chemists cannot define "element" while disregarding the theoretical developments in chemistry. When a chemist (such as Scerri) looks at the world, he does so from the perspective of his chemical understanding.

I can add that the discussion of the concept "species" in biology (often considered the element of biological classification) is a really difficult discussion.

6.0 Classifications are theories (of what is being classified)

Philosopher Marion Bunge wrote about the relation between theory and classification (1983a, 330, emphasis in original):

> Classing and theorizing are then mutually complementary activities. Categorization precedes theorizing if only because every theory is about some category of objects. In turn, theory allows one to refine the coarse and shallow pretheoretical classifications. Moreover a classification *is* a theory of a kind.

Ereshefsky (2000) argued that *the Linnaean Hierarchy* is an obsolete classification because it is not based on the theory of evolution. In a similar way *we should expect that any classification corresponds to a theory and vice versa: Any theory has implications for the classification of its object*s. Ørom (2003) demonstrated how library classifications of arts are related to theories or paradigms of art. In the field of mental diseases different theories are related to different classifications (cf., Cooper 2005). A given classification (say DSMIV) may correspond to some views, but is a bad reflection of, for example, the psychoanalytic view (and thus a bad tool for psychoanalysts. Whether psychoanalysis itself is a bad theory is another matter that has to been considered separately.)

Concerning the periodic table, we may consider theories such as Einstein's theory of relativity and quantum mechanics (QM) as a possible theoretical basis. Scerri writes about those theories (2007, 24-25):

> The first of these [Einstein's theory of relativity] has had a limited impact of our understanding of the periodic system but is becoming increasingly important in accurate calculations carried out on atoms and molecules;

and,

> The interesting question raised here is the relation between chemistry and modern atomic physics and, in particular, quantum mechanics. The popular view reinforced in most textbooks is that chemistry is nothing but physics "deep down" and that all chemical phenomena, and especially the periodic system, can be developed on the basis of quantum mechanics. There are some problems with this view, however, which are considered in this book.
>
> For example, in chapter 9 it is suggested that the quantum mechanical explanation for the periodic system is still far from perfect. This is important because chemistry books, especially textbooks aimed at teaching, tend to give the impression that our current explanation of the periodic system is essentially complete. This is not the case, or so it will be argued.

Scerri also considered the influence of chemists, or rather of inductive conclusions (p. 224):

> All this work was achieved without any arguments based on theoretical physics or, more specifically, without using quantum theory. The chemists' configurations were obtained inductively on the basis of the chemical properties of the elements. This aspect of the history of the periodic system is seldom emphasized, with most accounts promoting the view that electronic configurations resulted entirely from the work of theoretical physicists such as Bohr. In truth, Bohr had also reached electronic configurations inductively, frequently drawing on chemical evidence, at the chemists themselves had done.

Where does this leave us regarding the question of the relation between a classification and a theory? It seems that the periodic system has been constructed by the

interaction of, in particular, chemists' "inductive" view and quantum mechanics. I make the claim that this is not fact versus theory, but that this is (at the least) two competing perspectives or" theories" (although they are interacting, mutually overlapping and probably still developing theories). My argument is based on the premise that the idea of theory-neutral observation is hardly ever supported today; by implication chemists—when making their observations and inductions—are conceptually mediated by the research tradition in which they have been trained. Chemists' views, I suppose, are influenced by the properties which have been seen as most important in chemistry. The periodic law is itself a part of chemists' theoretical luggage (the periodic law is a profound achievement in chemistry and physics because it links the internal structure of the atoms with their bondage into molecules, their chemical interaction properties as well as with (some) physical features).

In the overall pattern these views tend to support each other, but, as Scerri argues, our current explanation of the periodic system is *not* essentially complete. Could it be that there is not one perfect periodic system satisfying all views? And could it be that these disagreements are related to different theories about chemical elements and the importance of different properties? Some authors emphasize the large number of periodic tables. Stewart, for example, wrote (2004, 156):

> Of the making of Periodic Tables there is no end. No version can ever be definitive because there are various incompatible objectives. Some authors provide a schematic version that is readable and easily reproduced, while others exploit devices such as the third dimension to express complexity. Some aim at simplicity or grace while others want to convey detailed information on such things as relative atomic mass, valency, electronic structure, melting and boiling points, electronegativity, radioactivity, metallic or non-metallic nature, geological affinities and so on.

The chemist Henry A. Bent wrote (2006, 108; emphasis in original):

> **Best periodic table**? Because analogies among the elements are *many-sided* (Mendeleev), no periodic table is superior to all other tables in all respects "There is no single best form of the periodic table since the choice depends on the purpose for which the table is used" (1, 3 p.136). The question "Which periodic table is best?" is like the question: "Which table of data in a Handbook of Chemistry and Physics is best?" "Best for what purpose(s)?" Display of<. Chemical valencies? Trends in electronnegativity? Atomic structure? Secondary Periodicity? Secondary Kinships? Tertiary Kinships? Gapless Periods? Periods' complements of shells and subshells? Periodicty's dyadic character? Madelung's Rule? Locations of "problems elements"? Block-to-block trends? The unique character of the *s*-block?
> No periodic table has all the features listed in Appendix XV. The question "Which periodic table is best?" is as impossible as unnecessary to answer.

Scerri (2009) in an article, the title of which has the following ending: "the Optimal Form of the Periodic Table, *if any*" (emphasis added). This title thus opens the door for the possibility that there is not one optimal form of the periodic system. What is interesting is whether a specific theory such as quantum mechanics tends to correspond better to a specific version?

In Wikipedia (2010) there is an article "Alternative periodic tables". These tables are based on the fact that not all correlations between the chemical elements are effectively captured by the standard periodic table:

> Alternative periodic tables are developed often to highlight or emphasize different chemical or physical properties of the elements which are not as apparent in traditional periodic tables. Some tables aim to emphasize both the nucleon and electronic structure of atoms. This can be done changing the spatial relationship or representation each element has with respect to another element in the table. Other tables aim to emphasize the chemical element isolations by humans over time.

And an example:

> Timmothy Stowe's physicist's periodic table [1988] is three-dimensional with the three axes representing the principal quantum number, orbital quantum number, and orbital magnetic quantum number.

My question—as a non-expert in chemistry and physics—is: Could it be that the Stowe Periodic Ta-

ble, for example, simply is a better match with QM? Scerri (2007) asks how well QM explains the periodic table, to what degree the periodic table can be reduced to QM. His way of asking—it seems to me—presupposes that the periodic table is "given," not something still being discussed and negotiated by scientists (in spite of what I quoted above about the best form).) If it is correct that different theories (such as QM) tend to imply different versions of the periodic system, could this explain some of the alternative versions? Scerri does address this problem (2007, 282), when saying:

> Although one can partly agree with the view that different representations can help to convey different forms of information, I believe that one may still maintain that one particular representation reflects chemical periodicity, regarded as an objective fact, in the best possible manner.

And (2007, 286):

> It is with some trepidation that I advocate the general adoption of the left-step periodic system since I am well aware of the resistance that this proposal will meet, especially from the chemical community, which, rightly or wrongly, regards itself as the sole proprietor of the periodic system.

It is important to say that I agree with Scerri that classifications should be based on the properties of the elements (i.e. the principle of realism). The possible disagreement is about whether different sets of properties of the elements may provide fruitful different classifications for different perspectives. Whether 1) chemical periodicity is only one classification criterion among other and 2) Whether different kinds of chemical periodicity could be at play? It seems to me that Scerri makes a choice by giving priority to some kinds of properties and regularities as compared to others. If so, it confirms my pragmatic understanding. (The quote above even reads as if the social constructivists may have a point: The struggle about the periodic system seems partly to reflect the interest of different social groups!). In the final page of Scerri (2007, 286) the criteria of beauty, elegance and utility are briefly mentioned (but considered difficult and not discussed). Again, this is opening a door to the pragmatic perspective.

In his letter Scerri (2010) wrote:

> Hjørland points out that although there may only exist one periodic law, there have been over 700 periodic tables published, which depend upon the particular pragmatic interests of any designer. My response to this point would be to shift the discussion to the periodic law rather than periodic tables.

If we substitute the expression "the periodic law" with "periodic theory," I would expect that this is (at least one) classification for which we are searching. (But again: Chemical periodicity might be just one among more criteria by which it is relevant to construe a classification of chemical elements – and the concept of element might itself be a problem depending on the perspective.) Scerri suggests that "chemical periodicity" is an objective fact and that one true classification corresponds to this fact. But is everything about chemical periodicity clarified today? (The formulation of the periodic law in Scerri (2007, 16) seems rather vague and thus open: "The periodic law states that after certain regular but varying intervals the chemical elements show an approximate repetition in their properties.") Why not say that there are theories of chemical periodicity and that a given theory of chemical periodicity corresponds to a certain classification? If it turns out that one theory may fully explain chemical periodicity, then this theory implies a certain version of the periodic table. In other words: Chemical classifications are implications of chemical theories (just as biological classifications, art classifications, psychiatric classifications are implications of subject theories in their respective fields). Scerri's search for one true classification should thus be understood as the search for one true theory of chemical periodicity. In this perspective the goal of the classification is to correspond to a theory, and the search for one true classification is thus a trivial implication of the demands that Scerri puts to it.

6.1 The narrower and broader form of pragmatism

At this point it seems important to make a distinction between pragmatism in a narrow and in a more fundamental sense. Scerri (2010) did express that:

> It may just be that the current attempts at representation are infused with pragmatism since individual chemists may indeed be interested in putting the periodic table to particular uses rather than arriving at one correct representation that reflects a natural classification.

I agree that some versions of the periodic table may be short-term pragmatic for some purposes, but not pragmatic in the way of producing a better tool designed for basic science. The broader form of pragmatism should of course be linked to fundamental findings and theories and should be pragmatic for the further advancement of science. (I do not believe that there is a great risk that pragmatism may be proven wrong. If there is a risk, it probably is that pragmatism may turn out to become trivial or circular. Pragmatism cannot be opposed to the search for truth, but implies that truth and relevance for the conduct of life are the same, that universals are to be understood as both something ontological and pragmatic (see also Karpatschof 2000, 317-18, 366, and 447).)

6.2 Conclusion of this section

The strength of the periodic system is, in particular, based on the periodic law, according to which properties of elements are periodic functions of their atomic numbers. Some of the open questions may be "how many properties?" "which properties?" and "properties important from which perspective?" Are the properties relevant for the classification of chemicals theory independent or reflecting a particular theory and interest? In the last case: Which competing perspectives/theories are at play, and which criteria should be used to decide and negotiate among them?

7.0 Natural classification, realism, natural kinds and essentialism

The concept "natural kind" is important in the philosophy of classification. To say that a kind is *natural* is to say that it corresponds to a grouping or ordering that does not depend on humans. The idea of natural kinds may also be expressed by Plato's metaphor "carving nature at its joints." Again chemical elements play an important role because other candidates for the term (such as biological species) have turned out to be problematic examples. In the periodic system there may be different claims about natural kinds:

- That the single element (such as gold (element # 79) or iron (element #26)) are natural kinds;
- The extension of this view to more complex chemical structures (such as molecules); and,
- That the elements group naturally into classes of elements (such as noble gases, alkali metals and the halogens).

Scerri (2007, 280) finds that the elements are natural kinds (and thus not a matter of convention), but that "the criterion for membership to a group is by no means as clear-cut as that which distinguishes one element from another." He continues: "However, one may also argue that the placement of the elements into groups is not a matter of convention. If periodic relations 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."

John Dupré is probably one of the leading critics today of the view of natural kinds and natural classifications as ordinarily understood. He writes, for example (1993, 274):

> Atoms are often suggested as example of natural kinds, with atomic number serving as an essential property. But the fate of cars driven over salted roads for any time provides a reminder that iron atoms are not at all the same as ferric ions, although both have atomic number 26. Atoms are also said to vary with respect to transitory states of orbital electrons, properties said to be of great significance to their chemical behavior.

It is important to realize that Dupré is also a realist in the sense that he classifies objects on the basis of their objective properties. He is however a pluralist (or "promiscuous") realist in claiming that there is more than one way to "carve nature at its joints." This seems to be related to the view expressed by Marradi 1990, 3.1):

> The opposition between "natural" and "artificial" classification is a recurring theme in the last two centuries. Cohen and Nagel have cogently argued that "any division ... according to some actual trait arbitrarily chosen is perfectly natural ... [but it] may also be said to be artificial, in the sense that we select the trait.

Traditional accounts of natural kinds centre on ideas of "essences" or "essential properties". Wikipedia (2010-06-06) defines "essence" the following way:

> In philosophy, essence is the attribute or set of attributes that make an object or substance what it fundamentally is, and which it has by necessity, and without which it loses its identity. Essence is contrasted with accident: a

> property that the object or substance has contingently, without which the substance can still retain its identity.

Cooper (2005, 47) wrote:

> 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 delineate species: morphological features, evolutionary lineage, the criteria of reproductive isolation, or genetic features. On examination none of these appear suitable candidates for being the essential properties of biological species.

A reasonable position may be that "essential properties" are essential from a given theoretical perspective. To the degree that there are competing perspectives, there will be competing views of which properties are essential.

8.0 Conclusion

In Hjørland (2008d) I provided the following model for "the traditional view of classification" in KO:

Scientific Classification → Library classification (KO)

This view may be expressed by stating that there is only one way in which nature has joints or by saying "nature itself has supplied the causal monistic essentialism. Scientists in their turn have simply discovered and followed (where 'simply' ≠ 'easily')" (Stamos 2004, 138-139). Library and information scientists in turn have to study scientific classifications and "simply discover and follow" scientific classifications. This view has, however, almost disappeared in KO in the second half of the 20th century (to be ousted by, for example, facet-analytic and use-oriented perspectives). (My own position is thus closer to this traditional view compared to, for example, facet-analytic, user-oriented and cognitive views.)

Against this traditional view may be put the view that classifications are reflecting the purposes for which they are designed and that different sciences, theories and human activities classify the world (more or less) differently. Both the practice of science and the practice of information science are thus seen as more constructive. The periodic system seems to be the ultimate challenge to this view. I do not believe the last word has been said about this important problem, but this article has tried to bring the KO-community up-to-date in relation to what I see as a fundamental problem in our field.

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What is the Nature of the Periodic Table as a Classification System?
Eric Scerri

(continued from page 10)

scientific classification is the periodic table of the elements." Dupré claims that the chemists' belief that they have arrived at an ideal classification is an illusion, "because of the specific aims implicit in the history of chemistry."

So what are these specific aims that chemists apparently possess and that have rendered their classification system biased in some way? Dupré considers that it is because chemists aim 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 those purposes."

I think that Dupré as well as Hjørland, who quotes him approvingly, are incorrect for two reasons. First of all, the periodic system of classification was arrived at completely independently of any conception of structure, atomic or otherwise. Mendeleev, the chief architect of the periodic system, repeatedly expressed his dislike for atomic theories and never accepted the discovery of the electron that took place in 1897, a full ten years before his death in 1907.

More importantly, Dupré's claim is somewhat circular. The aim of chemists is not necessarily the *structural* analysis of matter but rather the analysis of matter period. It so happens that the analysis of matter in general later revealed that a structural approach, in terms of atoms, protons and electrons, was a fruitful path to adopt. The chemists did not impose a structural prejudice upon chemical analysis. The latter is a feature that arose, presumably because the world itself contains discrete structural components such as atoms and electrons. Dupré further confuses the issue by quoting from LaPorte's well known article on natural kinds where LaPorte reports that the Chinese jade carvers refer to two quite distinct chemicals as "jade." I regard this as irrelevant since it is not the views of Chinese carvers that are under discussion but that of scientists. Scientists do not refer to the two kinds of materials as "jade" but as jadeite and nephrite as LaPorte readily acknowledges.

Hjørland proceeds to discussing what he calls "four possible ways" to defend the pragmatic view of

classification. The first is that at least certain features of the periodic system are still open to debate, a claim made with no further elaboration. But surely this is a weak argument since there is no field of science that is devoid of debate. The mere presence of debates, of some form or other, does not provide sufficient warrant for believing that one must hold a pragmatic view of knowledge rather than believing in the existence of natural kinds in a particular field of science.

The second of Hjørland's reasons is a direct appeal to Dupré as quoted above, namely that the pragmatic nature of the periodic system is due to the purpose of chemistry which Dupré takes to be the structural analysis of matter. I would counter this by rephrasing what I said earlier. The aim of chemistry is to understand the nature of matter by whatever means necessary. It is not to impose a structural conception at the very outset of the enterprise.

The third reason given by Hjørland (2008, 253) reads as follows: "The third [reason] 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." Once again this conclusion is too quick since it does not argue for science being a tool, or that science aims to control nature, but merely asserts these claims as facts.

The fourth and final way in which Hjørland regards the periodic system as a being pragmatically driven, rather than a reflection of natural kinds, is by questioning what he terms, "the generality of the periodic system's organization of similar elements." Hjørland claims that different chemical specialties such as agro-chemistry or food chemistry may be somewhat opposed to each other in seeking to highlight different aspects of the behavior of the chemical elements. He says that the periodic system seems somewhat opposed to such "social classifications" by different kinds of chemists, which he takes to a limit to the prediction of properties by the periodic system itself.

This conclusion is rather puzzling to the present author since it is precisely because the periodic table seeks the most general possible description and relationships among the elements that it may not immediately yield the kinds of predictions that are useful to agro-chemists or food chemists. But if this situation is the case, then I would say, so much the worse for the demands of these professions and that it is not a reason for questioning the generality of the periodic table. If anything it is a graphic demonstration *in favor* of its generality rather than specificity. Hjørland even questions whether atomic number should be considered as a criterion of natural kinds because, "not all properties are predicted by atomic number." This is of course true but irrelevant. Whether or not an element is a superconductor, for example, cannot be predicted from its atomic number but this has little bearing on whether or not elements are identified via their atomic numbers. Why should all properties of an element be predictable directly from just atomic number? As I just mentioned, some properties including superconductivity cannot be predicted from a knowledge of the element's atomic number. This fact does little to shake the confidence that chemists and physicists have in the notion that the identity of a particular element resides in the value of its atomic number.

More philosophically speaking, the identity and properties of any class of entities are somewhat separate issues. In the modern understanding of the term 'element' for example, the identity of gold does not reside in its being a certain color or possessing a shiny appearance or indeed in displaying any particular 'property' as such but just it its having an atomic number of 79. Many philosophers of science who espouse the causal theory of reference are in agreement with this view in claiming that identity is given by focusing on 'reference' rather than 'sense,' or on the essential qualities rather than the properties of an element (Putnam 1975; Scerri 2005).

Hjørland mentions the distinction between elements as simple substances (sense) and as basic substances (reference) but in a different context. As he sees it this distinction shows the coexistence of the empiricist and rationalist approaches to the classification of the elements. But in the very next sentence this dual nature in the meaning of the term 'element' also becomes an example of the importance of the historicist approach to knowledge, although no reason is given for this claim.

Fourthly, we are told that the pragmatist view is exemplified by another aspect of the periodic table, namely, "the weight attributed to chemical respective physical properties when determining the similarities among the elements." I hope that Hjørland might be prepared to explain this statement more fully as I am confused as to its meaning. Similarly, I would ask the author to clarify the meaning of the next claim, namely that the attempts to reduce chemistry to quantum mechanics, that I discuss in my book, are indicative of the pragmatic approach to knowledge.

Finally, Hjørland examines whether a classification, such as the periodic system, that is first developed in science, can then spread into the public media and into library classification systems. His conclusion seems to be that the periodic system has had no influence in these fields because books on the periodic table, such as the one he is reviewing, are largely ignored in the field of knowledge organization. Hjørland quotes from a rather outdated source, written no less than 99 years ago, in which the originator of the principle of "literary warrant" bemoans the fact that the periodic system is merely a classification by the names of elements which was of course incorrect then as it is now. This quoted author, Hulme (1911), also contends that, practically no literature in book form exists concerning the elements because, "no monograph, for instance has yet been published on the chemistry of iron or gold." These days there are literally dozens of books on these two elements alone and so would seem to preclude any conclusions on the nature of classification based on Hulme's writings of 1911.

To conclude, I thank Hjørland for his complimentary remarks about my book in the course of his review but I must disagree with his characterization of the periodic system as being the result of pragmatism rather than the way the world of the chemical elements is actually 'carved at the joints.' I am looking forward to his response and to his clarifying the parts of his review that I did not fully comprehend.

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A Note on the Debate Between Hjørland and Scerri on the Significance of the Periodic Table

John Dupré

(continued from page 10)

sitions, for example jadeite, $NaAlSi_2O_6$. Although this last looks like an exact chemical formula, it would be a mistake to suppose that jadeite was, primarily or essentially, the name of a chemical kind. To be a jadite (or jadetite) rock is to be a rock with a specific predominant composition produced through a characteristic metamorphic process at certain ranges of pressure and temperature. Relatively small differences in composition produce a variety of colours as well as difference in physical properties such as density. It would be quite wrong to think that a piece of jadeite with particular chemical traces that imparted to it a particular shade of green was thereby impure jadeite (or, indeed, impure jade). The presence of such traces is characteristic of jadeite and particular colour-producing traces might, indeed, make a piece of jadeite an exceptionally fine piece of jade rather than an impure one. The point, of course, is that jadeite is a geological term not a chemical term. Many of its properties may be explained by (more or less constant) features of its chemical composition. But the category, as with olivine or pyroxene, is shaped by the concerns of geology. The point would be even clearer for such much more chemically variable substances as magma (from which the rocks just discussed are originally formed), tephra, or lava.

Analytic chemistry, then, is not the only basis for the classification of kinds of stuff. Scerri is right to see the periodic table as the hardest test case for pluralistic accounts of kinds, and it has generally been recognised as such by pluralists including myself. It is a classification system which, once discovered, seems inevitable; it strikes many as embodying the discovery of that feature of a material stuff whereby, in Locke's memorable phrase, it is what it is. A possible response by the pluralist is to acknowledge that this is an exception to the pluralism that becomes irresistible for the more

complex domains of biology and society. (As David Hull once chided me, a pluralist should not be commited to a monistic (i.e. monistically pluralist) metaphysics.) Nevertheless, I think there is no reason for the pluralist to be so concessionary). The chemistry based on the periodic table is a rightly admired scientific project, one of the landmark successes of modern science. Nonetheless, it does not provide the only way of classifying material stuff. Geology, I have suggested, provides a clear example of a science the concerns and classifications of which do not align exactly with those of chemistry. Metallurgy, as I briefly mentioned, is another. Crystallography and parts of astronomy or fluid mechanics may be others. And most important, we should not rule out the possibility that we will in the future develop quite new interests in the classification of matter.

In short, then, I concur fully with Hjørland's insistence that the periodic table, for all its power and elegance, is no threat to a thoroughgoing pluralism, or to the pragmatist attitude that inspires it.

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