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A *ROSA MULTIFLORA* BY ANY OTHER NAME: TAXONOMIC INCOMMENSURABILITY AND SCIENTIFIC KINDS

ABSTRACT. The following paper attempts to explore, criticize and develop Thomas Kuhn's most mature – and surprisingly neglected – view of incommensurability. More specifically, it focuses on (1) undermining an influential picture of scientific kinds that lies at the heart of Kuhn's understanding of taxonomic incommensurability; (2) sketching an alternative picture of scientific kinds that takes advantage of Kuhn's partially developed theory of disciplinary matrices; and (3) using these two results to motivate revisions to Kuhn's theory of taxonomic incompatibility, as well as, to the purported bridge between taxonomic incompatibility and some of the traditional problems associated with incommensurability.

1. INTRODUCTION

Thomas Kuhn, together with Paul Feyerabend, first introduced the notion of scientific incommensurability in 1962 (Kuhn 1996, Feyerabend 1981). In Kuhn's earliest works incommensurability names methodological, conceptual, and observational disparities that might arise between the practitioners of rival scientific theories. Kuhn's discussion in his seminal *The Structure of Scientific Revolutions (SSR)* suggested that incommensurability must undermine rational theory choice, lead to failures in communication, and relegate rival scientists to "different worlds" (Kuhn 1996, pp. 148–151).[1] As one of several central provocative theses forwarded in *SSR*, Kuhn's early view of incommensurability had a profound influence on an astoundingly wide range of work both inside and outside the history and philosophy of science.[2]

In the wake of *SSR*, however, Kuhn's own picture of incommensurability shifted. Seeking to both clarify and improve upon his earlier treatment, he restricted incommensurability to semantic differences between scientific theories. In his 1982 paper "Commensurability, Comparability, Communicability", Kuhn explained that he now took the claim that two theories are incommensurable to be equivalent to "the claim that there is no language, neutral or otherwise, into which both theories, conceived as sets of sentences, can be translated without residue or loss" (Kuhn 1983a, p.

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670, see also p. 684 fn 3). This later treatment of incommensurability again garnered considerable attention, proving an effective catalyst for a wide range of work on topics including indeterminacy of reference, translation failure, and realism.[3]

In the last fifteen years of his career, Kuhn's diagnosis of incommensurability, however, continued to evolve. He came to increasingly locate the problems of incommensurability specifically in the translation of natural or scientific kind terms (Kuhn 1987, 1990, 1991a, 1993). According to Kuhn such terms and their inter-relations form a theory's "taxonomic structure". That structure is in turn governed by the "no-overlap principle" which states that "no two terms with the kind label, may overlap in their referents unless they are related as species to genus" (Kuhn 1991a, p. 4). Kuhn claimed that translation breaks down when the kind terms of one theory cannot be accommodated within the taxonomic structure of another theory on pain of violating the no-overlap principle. Remarkably, this diagnosis – Kuhn's most mature – has gone largely unnoticed by the vast majority of his commentators.[4]

In an important exception to this surprising neglect, Ian Hacking has recently sought to explicate Kuhn's "no-overlap principle" while offering a nominalist interpretation of the scientific kinds so crucial to Kuhn's most mature view (1993). Hacking suggests that we take the world to be a world of individuals upon which scientific theories impose hierarchic systems of kinds. The picture of scientific kinds which emerges both sharpens Kuhn's mature account of incommensurability and offers a benign interpretation of Kuhn's remarks to the effect that competing scientists "work in different worlds" (Kuhn 1993, 111, 117, 118, 135). According to Hacking, we may say that "individuals do not change with a change of paradigm ... [but that] the world we work in is a world of kinds ... and that is what changes with a change in paradigm: the world of kinds in which, with which, and on which the scientist works" (Hacking 1993, p. 277).

Drawing on the work of both Kuhn and Hacking, the following paper attempts to further explore and develop the notion of taxonomic incommensurability. It divides into three main sections. The first examines the thesis, central to both Kuhn's and Hacking's treatment, that scientific kinds must conform to a particular taxonomic structure. That thesis is found to lack both empirical support and scientific motivation. The second section draws on general themes from Kuhn's theory of scientific development, as well as, on Hacking' nominalist solution to Kuhn's "New World Problem", in offering an alternative picture of scientific kinds that is consistent with the examples of actual scientific kinds discussed in the first section. The third section then revisits Kuhn's two-part thesis of taxonomic incommensurability in light of the discussion in the first two sections.

2. THE HIERARCHY THESIS

The principal difficulty in giving an account of scientific kinds is identifying the feature or features that distinguish them from non-scientific kinds. What makes *reptile* and *potassium* scientific kinds? Why aren't *book* and *brick* scientific as well? We don't want to just say that *reptile* and *potassium* are kinds used by scientists whereas *book* and *brick* are kinds used by non-scientists. For, of course, non-scientists may use scientific kinds just as assuredly as scientists may use non-scientific kinds. Furthermore, we don't want to be committed to the view that every kind recognized primarily by scientists is a genuine scientific kind. Even if chemists commonly distinguish between their colleagues who are tidy in the lab and those who are messy, we don't want to recognize *tidy chemist* and *messy chemist* as scientific kinds.

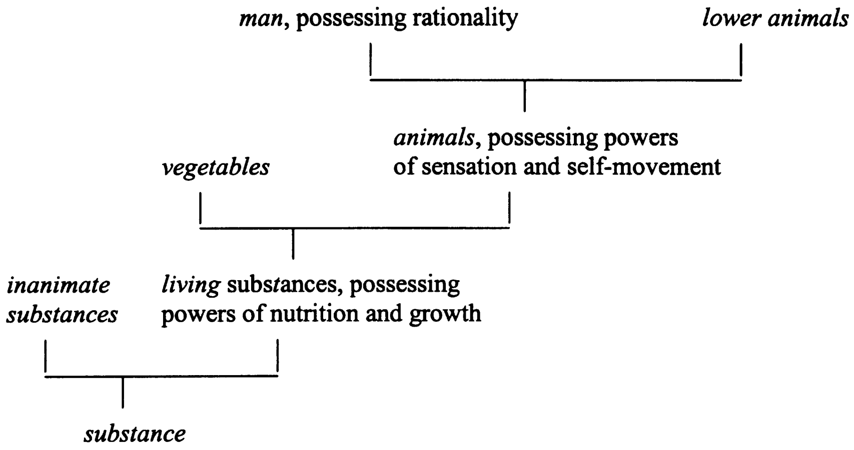
Following Kuhn, Hacking suggests that the most important distinguishing feature of scientific kinds is their conformity to a particular taxonomic structure. In attempting to capture that structure, Hacking offers the following definitions of "taxonomy" and "taxonomic":

> A *taxonomy* is determined by a class of entities C and a transitive asymmetric relation K. $\{C, K\}$ is a taxonomy if and only if (1) it has a head, a member of C that does not stand in the relation K to any member of C but such that every other member of C stands in the relation K to it; (2) every member of C except the head stands in the relation K to some member of C.
> $\{C, K\}$ is *taxonomic* if it breaks up into disjoint taxonomies. That is, there is a finite partition of $\{C, K\}$ into taxonomies $\{C_1, K\}, \ldots, \{C_n, K\}$ such that no member of C bears the relation K to two distinct heads in C. (Hacking 1993, p. 286)

We can state Hacking's central thesis as the claim that scientific kinds must be taxonomic in the sense just defined (Hacking 1993, p. 293). We'll call that thesis the "hierarchy thesis".

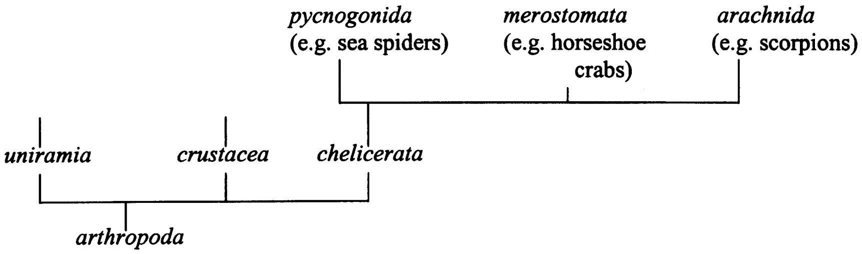
Practically speaking, the hierarchy thesis imposes three conditions on scientific kinds: (1) Each category of kinds must have some one kind as its head. (2) All kinds in a category (except the head) must be related to the head (either directly or indirectly) by a "kind of" relation. (3) If C_1 and C_2 are both related to the same head, C_1 either entirely subsumes or is entirely subsumed by C_2. Intuitively, the hierarchy thesis represents the structure of scientific kinds as a row of trees with each tree representing the kind structure of a particular theory.

Philosophers have long been drawn to various versions of the hierarchy thesis (Browning 1978; Gärdenfors 1994; Hacking 1993; Thomason 1969).[5] Aristotelians, for example, maintained that the real definition of a species must involve both its genus and its difference; a method of metaphysical and scientific classification which results in taxonomic systems of kinds such as the following branching tree for man:[6]



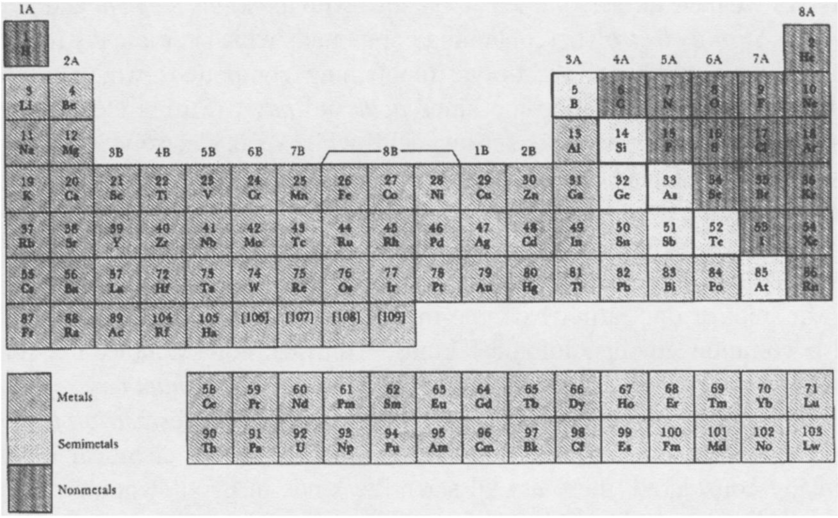
The Aristotelian system of classification satisfies the hierarchic requirement because (1) the kind *substance* serves as a head; (2) all the other kinds are related to *substance* by the genus-species relation; and (3) every species is entirely subsumed by its genus. Following the hierarchy thesis, Aristotelians rejected as unscientific, kinds that do not fit this structure. So, for example, the kind *father* was considered an unscientific kind because it "overlaps" or "cross-cuts" with the kinds *lower animal* and *man*.[7]

Although differently motivated, modern biology also sometimes uses systems of classification that are hierarchic. The following example is drawn from the Linnaean tree of contemporary biology:



Like the Aristotelian example above, this set of biological kinds satisfies the requirements of the hierarchy thesis: (1) the kind *arthropoda* serves as a head; (2) all the other kinds in the system ultimately relate back to *arthropoda* by an asymmetric transitive relation;[8] and (3) each higher kind (e.g., *pycnogonida*, *merostomata*, *arachnida*) is entirely subsumed by a related lower kind (e.g., *chelicerata*).[9]

If we focus on either the Aristotelian system of classification or the Linnaean tree, the suggestion that all scientific kinds must be hierarchic may appear plausible. Things change, however, if we widen the scope of our investigation. Consider, for example, what is perhaps the most powerful system of classification in modern science, the periodic table:[10]



1A	2A	3B	4B	5B	6B	7B	8B			1B	2B	3A	4A	5A	6A	7A	8A
1 H																	2 He
3 Li	4 Be											5 B	6 C	7 N	8 O	9 F	10 Ne
11 Na	12 Mg											13 Al	14 Si	15 P	16 S	17 Cl	18 Ar
19 K	20 Ca	21 Sc	22 Ti	23 V	24 Cr	25 Mn	26 Fe	27 Co	28 Ni	29 Cu	30 Zn	31 Ga	32 Ge	33 As	34 Se	35 Br	36 Kr
37 Rb	38 Sr	39 Y	40 Zr	41 Nb	42 Mo	43 Tc	44 Ru	45 Rh	46 Pd	47 Ag	48 Cd	49 In	50 Sn	51 Sb	52 Te	53 I	54 Xe
55 Cs	56 Ba	57 La	72 Hf	73 Ta	74 W	75 Re	76 Os	77 Ir	78 Pt	79 Au	80 Hg	81 Tl	82 Pb	83 Bi	84 Po	85 At	86 Rn
87 Fr	88 Ra	89 Ac	104 Rf	105 Ha	[106]	[107]	[108]	[109]									
				58 Ce	59 Pr	60 Nd	61 Pm	62 Sm	63 Eu	64 Gd	65 Tb	66 Dy	67 Ho	68 Er	69 Tm	70 Yb	71 Lu
				90 Th	91 Pa	92 U	93 Np	94 Pu	95 Am	96 Cm	97 Bk	98 Cf	99 Es	100 Fm	101 Md	102 No	103 Lw

The small squares, of course, represent different kinds of elements on the basis of atomic number. The shaded regions divide those elements into the kinds *metals*, *semimetals*, and *nonmetals*. The vertical columns, labeled "1A", "2A" ... across the top, divide the elements into various families such as the *Alkali metals* (1A), *Alkaline earth metals* (2A), *Pnicogens* (5A), *Chalcogens* (6A), *Halogens* (7A), and *Noble gases* (8A).

All of these are clearly scientific kinds in good standing. They figure in powerful and informative generalizations in modern chemistry. A standard textbook, for example, explains that:

> As we move from group 6A to group 7A, the nonmetallic behavior of the elements increases, as we would expect. Unlike the group 6A elements, all the halogens are typical nonmetals. Their melting and boiling points increase with increasing atomic number. ... The electron affinities of the halogens are among the greatest (most negative) of the ele-

> ments. Thus it is not surprising that the chemistry of the halogens is dominated by their tendency to gain electrons from other elements to form halide ions . . . (Brown et al. 1991, 239).

The scientific kinds represented in the periodic table do not, however, satisfy the hierarchy thesis. As the table itself perhaps best illustrates, the kinds *metals*, *semimetals*, and *nonmetals* do not fall into neat subsumption relations with the families labeled across the top.

In violating the hierarchy thesis, the kinds of the periodic table are only a particularly striking example of scientific kinds which "overlap" or "cross-cut" one another. Indeed, countless other examples could be drawn from chemistry alone. *Ion* overlaps not only with many of the classes we have already looked at, but also with the kinds *element* and *molecule*. *Strong electrolyte* (containing compounds which completely ionize in solution) and *weak electrolyte* (containing compounds which partly ionize in solution) overlap the kinds *acid* and *base*. (Strong electrolytes include both strong bases and strong acids while weak electrolytes include both weak acids and weak bases.) *Salt* in turn cross-cuts the kinds *strong electrolyte* and *weak electrolyte*. (Most, but not all, salts are strong electrolytes.) The structure of chemical kinds more closely resembles a quilt of overlapping strips and patches than a hierarchic tree.

In spite of our earlier example involving *arthropoda*, cross-cutting is also common among biological kinds. Consider, for example, the following set of kinds taken from contemporary biology: {*mammal*, *reptile*, *amphibian*, *fish*, *carnivore*, *herbivore*, *omnivore*, *marine*, *terrestrial*, *freshwater*, *extinct*, *non-extinct*, *male*, *female*}. As with the chemical kinds already considered, these are all scientific kinds in good standing. They are all examples of kinds recognized and used by biologists, and they figure importantly in powerful biological generalizations. Nonetheless they clearly do not fit the hierarchic picture suggested by Kuhn and Hacking. To take just one example: some females are mammals and carnivores; some mammals are females but not carnivores; some mammals are carnivores but not females. As with chemical kinds, biological kinds in general suggest not a hierarchic tree, but a series of overlapping patches.

Hacking admits that not all putative scientific kinds are hierarchic, and he offers his own example of a set of kinds that appears to violate the hierarchy thesis: {*poison*, *arsenic*, *hemlock*, *vegetable*, *mineral*} (Hacking 1993, p. 286). This set of kinds is not hierarchic, Hacking suggests because – arsenic being a poisonous mineral and hemlock being a poisonous vegetable – the kind *poison* cross-cuts the kinds *mineral* and *vegetable*.

Although he offers this counterexample himself, Hacking clearly does not recognize that violations of the hierarchy thesis are the norm rather than the exception for even putative scientific kinds. As a result, rather than abandoning the hierarchy thesis, he suggests a further requirement to rule out *poison* as a scientific kind and thus save the hierarchy thesis from his singular counterexample (Hacking 1993, p. 299).

Hacking's additional requirement appeals to a notion of *real kinds* borrowed from John Stuart Mill. *Real kinds* are kinds for which there is an almost inexhaustible number of things that might be discovered about them, and Hacking's additional requirement is that all scientific kinds must also be *real kinds*. The putative scientific kind *poison* is thus ruled out as non-scientific on the assumption that there is not an inexhaustible number things to be learned about poisons.

Hacking's appeal to *real kinds* is problematic for at least three important reasons. First, as should be clear by now, it is insufficient to block the numerous counterexamples to the hierarchy thesis. *Metal* and *halogen* are *real kinds* if any kinds are. Second, the appeal to *real kinds* brings with it a host of genuinely philosophical concerns. How, for example, do we know that there are in fact an inexhaustible number of things to discover about alkanes, acids, or amphibians? And what if we should discover that we now know all the properties of, say, strong electrolytes? Surely, if they continued to figure prominently in chemical research, we should be hesitant to call them unscientific. Third, the appeal to any requirement merely to eliminate counterexamples to the hierarchy thesis seems ill-conceived. For the problem with kinds such as *poison*, *plant* and *potassium* is not that they are non-scientific and need to be eliminated, but that they are clearly scientific and suggest that not all scientific kinds can be fitted into the straightjacket of the hierarchic model. We can, of course, lay down any restrictions we wish, and so exclude any kind we desire, but the measure of the success of an account of scientific kinds will not be how well we can make the data fit it, but how well it fits the data.

Hacking's *ad hoc* appeal to real kinds also makes felt a perhaps even deeper concern: The hierarchy thesis in general is utterly devoid of scientific motivation. Hacking emphasizes that "It is not an accident that the taxa of natural history are hierarchic. We make them that way: we 'force' our classifications to be hierarchic" (Hacking 1993, p. 288).[11] But even if this were true – and clearly it is not! – we are given no reason *why* science should be driven to limit its classificatory options so drastically.

Hacking does hint that "The relations between the kinds that create the taxonomy [i.e., hierarchic taxonomy] are logical, conceptual, or lexical", but the only explication we get of this is an insinuated connection to Fred Sommers's thesis that the predicates of natural language are hierarchic in their applicability conditions (Hacking 1993, p. 293, see also p. 295, and Sommers 1963). Defining the "*P* set" of a predicate as "the set of individuals of which it may be either true or false", Hacking writes:

> Chairs and questions are spanned by "hard", so the *P* sets of "hard" and "expensive" have members in common, but neither is properly included in the other. Sommers forces a [hierarchic] taxonomy by declaring, plausibly in this case, that "hard" is ambiguous, two meanings for one word. In a similar way Kuhn's taxonomies, we will see, force scientific kinds from distinct paradigms to be untranslatable, which may also be a case of two meanings for one word (Hacking 1993, p. 289).

There is an intriguing mirror-image similarity here. Sommers moves from meaning to taxonomic structure while Hacking moves from taxonomic structure to meaning. Nonetheless, Sommers's hypothesis is far too weak to support the hierarchy thesis. For Sommers's thesis applies only to the applicability conditions of predicates: if F can be applied to both a and b without making a category mistake, and G can also be applied to a, then G can be applied to b. The predicates "carnivore" and "mammal" do not violate Sommers's thesis because wherever the predicate "carnivore" can be applied without category mistake, the predicate "mammal" can also be applied. Nonetheless not all mammals are carnivores, and thus the kinds *mammal* and *carnivore* violate the much stronger hierarchy thesis. It is not a category mistake to suggest that cows are meat eaters; it's just false.[12]

Although the hierarchy thesis has enjoyed a long tradition of philosophical support, it does not appear to enjoy either empirical support or scientific motivation. In the next section an alternative way of characterizing scientific kinds is offered that appeals to their functional role in mature sciences rather than to their taxonomic structure.

3. TOWARDS A PATCH-WORK MODEL OF SCIENTIFIC KINDS

In his 1969 postscript to *SSR*, Kuhn introduced the term "disciplinary matrix" in order to clarify one important sense in which he had previously used the term "paradigm". He explained his choice of words as follows:

> For present purposes I suggest [the term] 'disciplinary matrix': 'disciplinary' because it refers to the common possession of the practitioners of a particular discipline; 'matrix' because it is composed of ordered elements of various sorts, each requiring further specification. All or most of the objects of group commitment that my original text makes paradigms, parts of paradigms, or paradigmatic are constituents of the disciplinary matrix, and as such they form a whole and function together. They are, however, no longer to be discussed as though they were all of a piece (Kuhn 1996, p. 182).

Kuhn's rough sketch of disciplinary matrices (DM's) captures two ideas central to our investigation of scientific kinds. First, a DM is a set of *local* descriptive, evaluative and methodological presuppositions shared by a discipline in such a way that a full understanding of the work of a discipline presupposes a grasp of its DM, and that to be a committed member of a particular discipline involves, among other things, largely endorsing its DM.

Second, a DM may itself be thought of as composed of various components. Kuhn himself identifies as components "symbolic generalizations", "values", "metaphysics", and "exemplars" while also emphasizing that these are in no way exhaustive (Kuhn 1996, pp. 182–187, see also pp. 40–42, 103).[13] In much the same way that a DM may be broken down into components, however, the components themselves may also be divided into what we might call "elements". So, for example, symbolic generalizations may be divided into axiomatic generalizations (e.g., $F = ma$) and canonical, but nonetheless derivable, generalizations (e.g., $g = 9.80$ m/s^2). Likewise values may be divided into those governing the acceptance of research results and those, generally less demanding values, governing permissible research (see Laudan 1984, p. 14; for a more extensive discussion see his 1977, chapter 4). Most importantly for us, the metaphysics component of a DM might be divided into individuals (e.g., Jupiter, Mars), properties (e.g., velocity, gravity) and kinds (e.g., planets, stars).

Building on these two important Kuhnian ideas and our earlier discussion, we may begin to sketch out a new model of scientific kinds with the four following theses:

(T1) Scientific kinds are to be understood within the tradition of nominalist metaphysics.

(T2) Scientific kinds are defined by their functional role as an element of a scientific discipline's DM.

(T3) A full understanding of a particular DM presupposes a grasp of its scientific kinds.

(T4) A full endorsement of a particular DM involves commitment to the view that its kinds offer the best current description of its domain.

Before fleshing out these four theses, we should be careful to remove two sources of potential confusion. First, the notion of a scientific discipline as used by Kuhn in his definition of a DM, and consequently in (T2), is, perhaps, more fine-grained than is customary. In both contexts, disciplines are primarily distinguished one from another neither by historical lineage, nor by domains of inquiry, but rather by the descriptive, evaluative, and methodological presuppositions shared by a group of practitioners. Thus, as Kuhn uses the term in his definition of a DM, not only will astronomy in general be counted as a distinct discipline from biology in general, but (say) Copernican astronomy will be counted as a distinct discipline from Ptolemaic astronomy. In order to avoid confusion in what follows, let us introduce the term "research program" to capture specifically the more thinly divided notion of a discipline suggested by Kuhn's discussion of DM's and consequently adopted in our statement of (T2).

Second, (T1)–(T4) should not be confused with criteria that might be thought applicable to the ontologically fundamental kinds often sought by philosophers. Just as Aristotle's substances, Leibniz's monads, and Spinoza's God need not satisfy (T1)–(T4), scientific kinds need not satisfy possible criteria for "fundamental kinds". Acids need not be ontologically basic. Pigeon facts need not be non-supervenient upon atomic facts. Sponges need not satisfy any respectable criterion of substantial unity. There can be little doubt that considerable confusion has resulted from the tendency to both suppose that metaphysically basic kinds must be scientific, and that scientific kinds must be metaphysically basic.[14] With these two caveats in place, we may properly turn to the task of explicating our four theses.

(T1) attempts to preserve what is best in Hacking's interpretation of Kuhn's "new world problem".[15] Recall that on Hacking's interpretation, the world is a world of mind-independent individuals, which do not come pretheoretically sorted into metaphysically privileged kinds – or at least not into all the various kinds recognized by the mature sciences (Hacking 1983, p. 110). In the hoary tradition of nominalism, the work of distinguishing the world into classes is therefore left up to human beings whose classificatory systems may be more or less helpful, but do not reflect more or less accurately independently existing kinds. When those classificatory systems change, the world as we know it changes in a limited way as well. Scientific revolutions alter the existence of mind-dependent kinds, but leave untouched the existence of ontologically independent individuals.

In adopting Hacking's nominalist solution to Kuhn's "new world problem", we should not follow him in treading "warily between kinds and terms" while stepping over concepts altogether (Hacking 1993, p. 292). For to speak merely of terms is not helpful. There is surely nothing special about scientific terms themselves – their figure or formatting for example – that makes them important to a discussion of scientific theories, incommensurability, or scientific kinds. It is true that linguistic clues may help us to identify scientific kind terms, and therefore scientific kind concepts, but it is the latter that are important both to understanding science and the things to which scientists refer. Scientific kind terms would be lifeless and uninteresting but for the use that scientists make of them to build theories and describe the world.

Nor should we hope that we might avoid talk of concepts in favor of extensional sets.[16] For scientific kinds determine extension rather than the other way around. *Phlogiston* is famously a scientific kind of pre-Lavoisierian chemistry although there is considerable debate over whether it succeeds in referring to anything at all (see, for example, Kitcher 1983, pp. 689–698 and Kuhn 1983b, pp. 713–715). Similarly *aether* is a good candidate for a scientific kind of early Newtonian physics but it perhaps has even less of a chance of picking out an extensional set. Given the functional roles that these kinds play in their respective disciplines, however, their failure to refer will not show that they are not scientific kinds. Nor would the discovery that they refer to the same extensional set – in this case probably nothing – show that they are the same scientific kind. There is, of course, nothing wrong with using the names of scientific kinds ambiguously to speak both of scientific kind concepts and of their extensions, but in giving a general account of scientific kinds it will be important to recognize that the concepts are primary and their extensions secondary.

It is perhaps important to note that in endorsing some version of concept nominalism over predicate nominalism, we do not commit ourselves to any particular philosophical view concerning concepts themselves.[17] We might identify concepts with brain states or functions, abilities or dispositions, mental representations, or what have you. Although any adequate account of scientific kinds will have to appeal to the concepts that scientists use in formulating theories and conducting research, whatever turns out to be our best account of concepts in general should also be our favored account for the relevant subset of concepts used by scientists in classifying entities.

In the first section we noted that the principal difficulty in giving an account of scientific kinds lies in characterizing the feature or features which distinguish them from ordinary kinds. (T2) suggests that rather than attempting to characterize scientific kinds in terms of a common structure, we should look towards the functional role that scientific kinds play in guiding the work of normal science. That is, we should look toward the ways in which scientists distinguish their respective domains into classes of kinds, and how those classes are related to other elements of the research program's DM (see, for example, Kuhn's discussion in his 1977d).

Identifying scientific kinds with an element of a DM thus allows us in characterizing the defining features of scientific kinds to take advantage of Kuhn's already partially developed theory of disciplinary matrices. Just as the existence of a DM capable of guiding normal research is what distinguishes scientific disciplines from non-scientific disciplines, being an element of a DM is what distinguishes scientific kinds from other species of kinds (e.g., philosophical or commonsense). So, for example, although *water* and *coffee* are both kind concepts used by chemists and lay persons alike, only the former is a candidate scientific kind because only the former has a role to play in guiding the normal research of a scientific discipline. Likewise, because research programs are individuated by their respective DM's, a set of scientific kinds belongs to one research program rather than another in virtue of its membership in that program's DM. This grounds the intuitive sense we have that, say, the kind *quark* belongs to physics rather than biology even though nothing bars the biologist from using the term "quark" with its full physical meaning.

(T3) and (T4) set the stage for understanding the relations between scientific kinds and the practitioners who use them. (T3) is related to the important distinction between internal and external history of science. Internal history of science, as found for example in Whewell, Koyré, and Kuhn, suggests that a full understanding of a particular discipline comes only when one has grasped the set of presuppositions that guide its day to day research.[18] The identification of scientific kinds with an element of a DM thus suggests a practical means for recognizing scientific kinds. In particular it allows us to distinguish between kind concepts that are merely used by scientists and scientific kinds. So, for example, although contemporary physicists use the kind concept *experimentalist* to denote members of the profession particularly adept at constructing advanced experimental devices, we may nonetheless recognize that *experimentalist* is not a scientific kind because it is not essential for fully understanding modern physics.

(T4) suggests that since scientific kinds are classificatory concepts used to describe and organize a domain of investigation, no one could fully endorse a particular research strategy who did not also endorse its descriptive classificatory system as the best currently available. It is therefore no accident that the practitioners of a particular research program are usually fiercely committed to its kind structure. Indeed, commitment to a program's classificatory scheme might even be psychologically essential to some members of the profession. That is, some practitioners may find themselves unable to expend the time and energy necessary for normal research, unless they believe that they are working with the best currently available description of the domain in question.

The distinction between (T3) and (T4) highlights the difference between understanding a system of kinds and endorsing that system. The need for such a distinction should have been clear from the activity of historical research alone. In learning, say, the kind system of phlogiston chemistry, one need not come to believe that that system is the best *current* description of the chemical domain, any more than one need believe that the world is composed of windowless monads in order to understand Leibniz's mature metaphysics. The distinction is, of course, no less valid when applied non-historically. We needn't suppose, for example, that current debates over, say, the promise of string theory are simply the result of misunderstandings between contemporary physicists.

The distinction between fully understanding a DM and fully endorsing that DM in turn points out what is wrong with thinking of scientific kinds in terms of entrenchment.[19] While it is to be expected that practitioners of a particular discipline will endorse that discipline's system of classification as the best available for the domain in question, such commitment is not essential to the functional role that defines scientific kinds. For a scientist may work under the guidance of a DM without fully endorsing it. She may put in her time at the lab during the day while developing an alternative theory in the evening. She may continue her research for the sake of duty or a pension long after she suspects that it is hopelessly off track. Although the two may often go hand in hand, a full understanding of a DM does not entail commitment to it.

Together (T1)–(T4) lay the foundations for a picture of scientific kinds much different from the pictures to which philosophers have grown accustomed. Traditional accounts have attempted to distinguish scientific kinds by identifying a common structural theme. Our new model, on the other hand, attempts to distinguish them primarily by the functional roles they play in guiding scientific research. The next section will consider some possible implications of this model with regards to Kuhn's thesis of taxonomic incommensurability.

4. TAXONOMIC INCOMMENSURABILITY REVISITED

Kuhn's mature thesis of taxonomic incommensurability involves two aspects. The first aspect consists in a theory of kind incompatibility guided by his "no-overlap principle". The second aspect relates kind incompatibility to the unique problems of mutual understanding and acceptance associated with incommensurability via a theory of successful translation. Our discussion in the previous two sections suggests re-evaluations of both of these aspects.

In reconsidering the first aspect of Kuhn's treatment of taxonomic incommensurability, it will be important to recognize that Kuhn applies his no-overlap principle in two significantly different contexts. In the first context, the no-overlap principle is applied only to the kinds within a single taxonomy as, for example, in the following passage where Kuhn explains what he takes to be the second of two essential properties of kind terms:

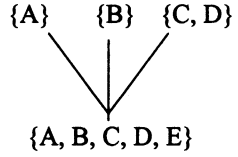
> Second – a limitation I sometimes refer to as the no-overlap principle – no two kind terms, no two terms with the kind label, may overlap in their referents unless they are related as species to genus. There are no dogs that are also cats, no gold rings that are also silver rings, and so on: that's what makes dogs, cats, silver, and gold each a kind. Therefore if the members of a language community encounter a dog that's also a cat (or, more realistically, a creature like the duck-billed platypus), they cannot just enrich the set of category terms but must instead redesign a part of the taxonomy. (Kuhn 1991a, p. 4)

When applied to the kinds within a single research program, the no-overlap principle leads to restrictions familiar from our discussion of the hierarchy thesis. In particular, it suggests that no two kinds within a single research program may share members unless one kind is a subset of the other kind. When offered as a restriction on kinds within a single taxonomy, we might call Kuhn's maxim his "no-internal-overlap principle".

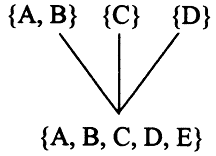
In the second context, Kuhn applies his no-overlap principle to kinds drawn from distinct taxonomies. By way of illustration, we may consider a standard example used by Kuhn involving the transition from Ptolemaic to Copernican astronomy: In Ptolemaic astronomy the sun and moon are classified as planets, but the earth is not. In Copernican astronomy, the earth is classified together with Mars and Jupiter as a planet, the sun as a star, and the moon as a new sort of entity, a satellite. Kuhn plausibly suggests that the taxonomies of Ptolemaic and Copernican astronomy are incompatible, and that they violate his no-overlap principle (Kuhn 1987, p. 8; see also Kuhn 1991a, p. 5; 1991b, p. 19).

Note, however, that in this standard example no reference is made to the internal structure of either Ptolemaic or Copernican taxonomy. Whether or not Ptolemaic taxonomy or Copernican taxonomy is hierarchic seems beside the point. What is directly relevant are only the inter-relations holding between the two taxonomies. Intuitively, the incompatibility of the Ptolemaic kind *planet* with the Copernican kind *planet* flows not from any of violation of Kuhn's no-internal-overlap principle, but rather from the fact that the kinds drawn from Ptolemaic astronomy crosscut the kinds of Copernican astronomy (and *vice versa*). Where the kinds from two distinct taxonomies overlap one another, we might say that they violate a no-external-overlap principle.

If one takes the hierarchy thesis for granted, the two principles may be related by treating the no-external-overlap principle as an indirect consequence of the no-internal overlap principle. Consider, for example, the following two systems for classifying the members of the set of capital letters {A, B, C, D, E}:



System 1



System 2

Again, intuitively, System 1 and System 2 are incompatible because they directly violate the no-external-overlap principle – that is, the kinds of System 1 crosscut the kinds of System 2 (and *vice versa*). Because both systems independently satisfy the hierarchy thesis, however, an alternative analysis of their incompatibility is available: System 1 and System 2 may be deemed incompatible because importing the kinds from System 1 into System 2 (or *vice versa*) would result in a new system of kinds that would not satisfy the hierarchy thesis.

Kuhn never distinguishes between the no-internal-overlap principle and the no-external-overlap principle, and so it is difficult to see whether or not he views the latter merely as a consequence of the former. Having made the distinction, however, it is easy enough to see how Kuhn's original no-overlap principle should be revised in light of our discussion in the first section. Because incommensurability only concerns the relations between two distinct research programs, we may adopt the no-external-overlap principle while abandoning the no-internal-overlap principle altogether. Doing so leaves us with an intuitive account of the incompatibility of rival taxonomic systems while freeing us from the untenable consequences of the hierarchy thesis.

The second aspect of Kuhn's thesis of taxonomic incommensurability involves relating kind incompatibility to the problems associated with incommensurability *via* a theory of successful translation.[20] Although there is considerable controversy over the nature and scope of the problems of incommensurability, their origins in Kuhn's work are relatively straightforward and can be traced to his distinction between two different kinds of scientific research.

The first kind of scientific research closely resembles the sort of progress featured in traditional models, and consists in solving outstanding problems suggested by the presuppositions internal to a research program. Problems include determining important facts highlighted by accepted theory, matching those facts with theory, and further articulating theory details (Kuhn 1996, p. 34). This is the work that Kuhn calls "normal science", and it is what most scientists spend most of their time doing.

The second kind of progress highlighted by Kuhn is not closely related to more traditional models of scientific development. In addition to solving outstanding problems articulated by the presuppositions internal to a research program, scientists may also work to reshape the presuppositions guiding normal science. This is the work Kuhn calls "extraordinary science", and concrete examples of it include "the transition from an Aristotelian to a Newtonian understanding of motion, from the contact to the chemical theory of the Voltaic cell, and from Planck's to the now familiar derivation of the law of black-body radiation" (Kuhn 1987, p. 7).

By changing the presuppositions internal to a scientific research program, extraordinary science can lead to unique problems of understanding and acceptance for their respective practitioners. In order to illustrate those unique difficulties, Kuhn repeatedly points towards his own experience in trying to understand Aristotle's physics after having been a student of classical mechanics (Kuhn, 1977, pp. xi–xiv; 1977a, p. 20; 1987, pp. 8–12). Kuhn suggests that because he took for granted many of the presuppositions of Newtonian physics, "Aristotle appeared not only ignorant of mechanics, but a dreadfully bad physical scientist as well. About motion, in particular, his writings seemed to me full of egregious errors, both of logic and of observation" (Kuhn 1987, p. 9). His assessment of Aristotle's natural philosophy changed dramatically, however, once he began to recognize the different, but powerful, presuppositions that guide the normal science of Aristotle. In the process, Kuhn "to some extent learned to think like" an Aristotelian physicist, at which point he "had few problems understanding why Aristotle had said what he did about motion or why his statements had been taken so seriously" (Kuhn 1977, p. xii). Kuhn claims that he "still recognized difficulties in his [i.e., Aristotle's] physics, but they were not blatant and few of them could properly be characterized as mere mistakes" (Kuhn 1977, p. xii).

Kuhn's theory of translation is, at a minimum, supposed to help bridge an explanation as to how the sorts of difficulties he experienced in confronting Aristotelian physics might arise from differences between Aristotelian and Newtonian taxonomies. So far nothing in our discussion precludes following this general Kuhnian strategy of understanding incommensurability as "a sort of untranslatability, localized to one or another area in which two lexical taxonomies differ" (Road, 5). Nonetheless, the new model of scientific kinds offered in the second section suggests that the relation between the problems associated with incommensurability and taxonomic incompatibility need not run through the ambit of translation.

Consider for the purposes of an extended analogy two different but related card games. Panguingue, also known as "Pan", and Canasta both belong to a family of games generally known as Rummy which trace their origins to the game of Conquian. As is characteristic of Rummy games, the object of Panguingue is to meld sets of cards containing three or more members. In Pan there are two distinct kinds of sets that may be melded: (1) A *sequence* is composed of any three cards of the same suit in order. So, for example, a set containing the members Jack, Seven, and Six of Clubs constitutes a sequence (there are no eights, nines, or tens in a standard Panguingue deck). (2) A *group* is a set of three cards of the same rank. If the rank is ace or king, any three cards are valid regardless of suits. All other ranks are governed by the rule that the three cards must either be all of different suits or all of the same suit. So, for example, three fives may be melded if they are {heart, spade, diamond} or {heart, heart, heart} but not if they are {heart, heart, spade}.[21]

The object in Canasta is also to meld sets of three or more cards. The sets recognized in Canasta, however, differ somewhat from those recognized in Panguingue. In particular, sequences are not recognized as valid melds in Canasta. Other differences arise in connection with restrictions that govern the inclusion and exclusion of the wild cards used in Canasta but not in Pan. For example, a valid meld in Canasta must contain at least two non-wild cards and cannot have more than three wild cards. Furthermore it is an essential characteristic of Canasta that a special distinction is assigned to a meld, called a "canasta", of seven or more cards.

Although the systems of kinds employed in Panguingue and Canasta are much less complex than those used in the advanced sciences, they illustrate on a simplified scale the unique problems that may arise for someone shifting from one set of guiding presuppositions to another. A seasoned Pan player might easily make mistakes in first playing Canasta that a neophyte would not even imagine making. For example, he might hold cards for sequences which are not valid in Canasta, or discard a six and a seven on the assumption that the stock contains no eights. One can easily imagine that these sorts of confusions will be compounded by habit and commitment to one's more familiar game. A person who thinks that Canasta is a bastardized version of Panguingue will no doubt be harder to teach than a player less committed to the game of Pan.

Analogously, where the taxonomies of two research programs are incompatible, we might expect unique difficulties of understanding and acceptance, such as those experienced by Kuhn in confronting Aristotelian physics with a Newtonian background. Since mastery of a scientific research program presupposes a full understanding of the presuppositions embedded in its taxonomic structure, a practitioner accustomed to one scientific program may make problematic assumptions that would be unlikely for a neophyte. So, for example, a background in classical mechanics may encourage one to misunderstand Aristotle's rejection of motion in a void or Galileo's principle of (circular) inertia. These sorts of difficulties may be compounded by the commitment that often (but not necessarily) accompanies a full understanding of a program's DM. Notoriously, Priestly's commitment to phlogiston theory greatly influenced his appraisal of the oxygen theory of combustion.

In addition to giving rise to some unique problems, however, familiarity with one card game may also prove advantageous in learning others. In spite of the important differences between Panguingue and Canasta, there is nonetheless a great deal of similarity between the two games. Someone who has mastered Panguingue has already acquired most of the skills necessary to play Canasta, and already has a handle on many of the most important presuppositions that guide play of the game.

Likewise, although often overlooked, mastery of one scientific research program may also prove advantageous when confronting even an incommensurable rival. For the skills acquired in mastering one scientific program will usually prove helpful in mastering a second. Furthermore, especially in the case of closely related incommensurable theories, many of the presuppositions embedded in one theory will carry over to the other theory. An understanding of Galileo's physics is a great help in understanding Descartes's physics even though crucial differences between their theories are a potential source of novel error.

Finally, it is worth noting that the unique difficulties that (say) a Pan player faces in learning Canasta would not seem to be limited to problems related to classification. Similar concerns may arise wherever the presuppositions that guide play in Panguingue diverge from those that guide Canasta play. The two games differ not only in the sets they recognize as valid, but also in some of their conventions for dealing, going out, and scoring. Such differences are no less a fertile breeding ground for unique problems of learning and understanding.

Analogously, although we have focused on the problems of incommensurability as they might arise from kind incompatibility, nothing in our discussion suggests that those difficulties might not arise from other sources as well. Indeed just the opposite. We have been treating scientific kinds as one element of a disciplinary matrix. If the changes brought about by extraordinary science can result in kind incompatibility leading to incommensurability, there is good reason to suppose that it can also lead to incompatibility between any of the other elements which constitute a disciplinary matrix. Our discussion therefore suggests that rather than reducing the origins of incommensurability to problems of translation or taxonomy, we would be better off investigating the composition of disciplinary matrices and the many relations which might hold between the various elements of different scientific disciplines.

5. CONCLUSION

Kuhn's provocative thesis of scientific incommensurability underwent at least two important shifts during the course of his career. The first shift constituted a move from a concern over methodological, conceptual, and observational disparities between the practitioners of rival scientific theories to a concern over semantic differences between such rivals. The second shift moved from semantic concerns taken generally to supposed semantic breakdowns resulting specifically from differences of taxonomy. Although the first two stages in the development of Kuhn's incommensurability thesis have garnered considerable attention in the secondary literature, and have paved valuable inroads to a wide range of important topics both inside and outside the philosophy of science, Kuhn's most mature view of incommensurability remains largely unexplored by his commentators.

In this paper I have therefore attempted to explore, criticize and develop Kuhn's thesis of taxonomic incommensurability. In particular, I have focused on (1) undermining the influential picture of scientific kinds that lies at the heart of that thesis; (2) sketching an alternative picture of scientific kinds that takes advantage of Kuhn's partially developed theory of disciplinary matrices; and (3) using these two results to motivate revisions to Kuhn's theory of taxonomic incompatibility, as well as, to the purported bridge between taxonomic incompatibility and some of the traditional problems associated with incommensurability.[22]

NOTES

[1] For helpful overviews of the development of incommensurability in Kuhn's work see, especially, Honyningen-Huene (1993, pp. 206–222), and Sankey (1993, 1997).

[2] For an extensive bibliography of works dedicated primarily to the topic of incommensurability see, Hoyningen-Huene and Sankey (2001, 303–316).

[3] See, for starters, the companion pieces to Kuhn (1983a and 1983b): Kitcher (1983) and Hess (1983).

[4] This point is emphasized in Sankey (1998).

[5] Which is not to say, of course, that the hierarchic thesis has not had its critics. See, for example, Dupré (1981, 1993), Khalidi (1998), and Kitcher (1984b).

[6] I owe the following diagram to Ayers (1991, 251).

[7] Khalidi (1998) uses the term "cross-cut" in connection with kinds that violate taxonomic structure. Throughout, I use the terms "overlap" and "cross-cut" interchangeably.

[8] Kinds of the modern Linnaean system are not, of course, all related by the genus-species relation. Nonetheless, an asymmetric, transitive relation *K* could easily be defined to fit its requirements. So, for example, the Blackburnian warbler belongs to the species *Dendroica fusca* which is a kind of *Dendroica* (Genus) which is a kind of *Parulidae* (Family) which is a kind of *Passeriformes* (Order) which is kind of *Aves* (Class) which is a kind of *Chordata* (Phylum) which is a kind of *Animalia* (Kingdom).

[9] Hacking (1993, p. 293) also includes a condition that "scientific kinds have infima species" that "are kinds [presumably at the tips of the tree] such that, as a presupposition of the science, no subdivisions of those kinds can count as scientific kinds". He latter, however, (1993, p. 299) admits that he is unsure of this condition and concedes that he "can't make very clear nontrivial sense of it". The condition would only further weaken Hacking's view, and in light of his own misgivings, I will not press the issue here. We might note, however, that if such a condition is applicable to any classificatory tree, it is unlikely to be a biological one. *Merostomata* is a particularly interesting class of organisms in part because it has only one order, *horseshoe crab* (*Xiphosura*). But although *merostomata* has only one order, there is no reason to suppose that is an infima species in the strong sense suggested by Hacking's second condition. A future mutation in merostomata, or more likely a new discovery in the fossil record, could always introduce a new order without showing that *horseshoe crab* is not a kind.

[10] The table is borrowed from Brown et al. (1991, p. 223).

[11] I have substituted "hierarchic" for Hacking's "taxonomic" in order to avoid confusion over terminology.

[12] Khalidi makes a similar point. See his (1998, pp. 39–41).

[13] Some fairly obvious additions would include techniques and apparatus.

[14] Kuhn himself often seems to confuse scientific kinds with more strictly philosophical "fundamental kinds". Kuhn (1996, pp. 4–5) writes, for example, "Effective research scarcely begins before a scientific community thinks it has acquired firm answers to questions like the following: What are the fundamental entities of which the universe is composed? How do these interact with each other and with the senses? What questions may legitimately be asked about such entities and what techniques employed in seeking solutions?"

[15] Kuhn indicates (very briefly) that he would resist this suggestion. See Kuhn (1993, p. 315f).

[16] At least not of actual objects. If one is willing to swallow a domain of possibilia (Lewis 1986) one might try to characterize scientific kinds in terms of extensional sets of actual *and possible objects* (Lewis 1999). Note, however, that even the friend of modal realism will still have to give an account of how scientists are related to those extensional sets (Lewis 1999, pp. 49–50), and thus does not escape the need to talk about scientific kind concepts.

[17] I follow Armstrong's (1978) terminology.

[18] For discussion of the relations between Kuhn's work in the history and philosophy of science see Kuhn's (1977a, 1977b, and 1977c); (1977b) has a helpful bibliography for further sources.

[19] See, for example, Quine (1969, p. 129).

[20] For a nice discussion of this link, as well as further references, see Sankey (1993, 765ff).

[21] There are eight fives of the same suit in a standard Panguingue deck.

[22] I would like to thank Jose Benardete, Dave Bzdak, John Draeger, John Hawthorne, Tom McKay, Erik Schmidt, Brian Weatherson and especially Brent Mundy for discussions and suggestions on earlier drafts of this paper. I have also benefited from the suggestions of two anonymous referees for this journal.

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