

POLYTHETIC CLASSIFICATION: CONVERGENCE AND CONSEQUENCES

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The conventional definition of a conceptual class is that its members must possess certain properties in common. Vygotsky and Wittgenstein have shown that this definition is unrealistic and logically unnecessary. The resultant recognition of classificatory concepts formed by family resemblances has recently led to a revision of anthropological analyses of kinship and of belief statements. The present article reports the discovery that, by a remarkable convergence of ideas in the past decade, family resemblance predicates had already been adduced in certain natural sciences under the term 'polythetic classification'. The methodological and experimental results of this approach are set out, and a variety of consequences for social anthropology are drawn from them. A main conclusion is that comparative studies carried out in the stock classificatory terms of anthropology are subverted by the realisation that they refer not to common features but to polythetic classes of social facts. It is suggested that effective comparison may nevertheless be practicable by reliance on a purely formal terminology of analytical concepts, and it is envisaged that these may permit the determination of basic predicates in the study of human affairs.

It is hazardous to think that a coordination of words . . . can have much resemblance to the universe.

J. L. BORGES

I

'A perfect intelligence would not confine itself to one order of thought, but would simultaneously regard a group of objects as classified in all the ways of which they are capable' (Jevons 1874, 2: 349).

This dizzying postulate, by virtue of its very extremity, is a commonplace in theology; the capacity is central to that 'respectful chaos of unimaginable superlatives' by which men refer to God (Borges 1965: 147). Yet for all its unsettling power the notion still proffers a false security, for it implies that a finite sense can be given to the prospect of 'all' the ways there are of classifying things, whereas the truth instead is that the description of reality is in principle inexhaustible: 'Of any two things whatever, there is some respect in which they can be said to resemble each other and not to resemble some third thing' (Hampshire 1959: 31). This is so even on the traditional premiss asserted by Jevons: 'Of every class, so far as it is correctly formed, the great principle of substitution is true, and whatever we know of one object in a class we also know of the other objects, so far as identity has been detected between them' (1874, 2: 345); in other words, 'a class must be defined by the invariable presence of certain common properties' (412). Logically, however, there is no such necessity; and once it is admitted that the common-feature definition of a class need not be the only possible method, the hypothetical

number of 'ways' in which things can be classified is multiplied over and over again.

Now we know experimentally from Vygotsky that classificatory concepts are not in practice formed by children in the way traditionally supposed in formal logic (Vygotsky 1962, ch. 5); and we have been shown analytically by Wittgenstein that verbal concepts are commonly not constructed on that pattern either (Wittgenstein 1953; 1958). Instead, classes can be composed by means of what Vygotsky calls complex thinking: specifically, in a 'chain complex' the definitive attribute keeps changing from one link to the next; there is no consistency in the type of bonds, and the variable meaning is carried over from one item in a class to the next with 'no central significance,' no 'nucleus' (1962: 64). In a remarkable parallel, Wittgenstein, writing in the same period as Vygotsky, resorted to the image of a rope (later, in the *Philosophical investigations*, a thread) in order to convey the same constitution of a concept: 'the rope consists of fibres, but it does not get its strength from any fibre that runs through it from one end to another, but from the fact that there is a vast number of fibres overlapping' (1958: 87). Among the members of such a class there is a complex network of similarities overlapping and criss-crossing; sometimes overall similarities, sometimes similarities of detail. These features Wittgenstein termed, in a since famous phrase, 'family resemblances.'¹ Thus by an intriguing convergence of psychological and philosophical analyses, reported independently² in Russia and England respectively in 1934, the traditional common-feature definition of a class was demonstrated to be both empirically and formally defective. What Jevons had called 'the great principle of substitution' no longer held as a universal principle of classification, and a class could therefore no longer be defined necessarily by the invariable presence of certain common attributes. Since the members of a class composed by sporadic resemblances³ were not assumed to be identical in any respect, it was no longer true that what was known of one member of a class was thereby known of the other members. What Jevons had described as a 'correctly formed' class was correct only by convention; and in numerous circumstances, as both Vygotsky and Wittgenstein proved, the convention was factually incorrect.

II

This conceptual revolution, after the millennial hegemony of the formal logic of the schools, might well have been expected to have consequences for a wider range of empirical disciplines. How far it actually did so, in one scientific undertaking or another, is a matter yet to be determined by an historian of ideas. It certainly had implications for social anthropology, however, and in 1970 an attempt was made to draw some of these out so far as they affected the analysis of kinship and marriage (Needham 1971*b*; repr. in Needham 1974*a*, ch. 1). A point worth making about that revisionary exercise is that the approach was not deductive, but proceeded instead by way of explanatory difficulties that were commonly encountered in the empirical practice of anthropological comparison.

Among the topics dealt with, a paradigm case was presented by the concept of 'descent,' a notion which in both descriptive and comparative studies had led, I contended, to typological confusion. I proposed therefore the disintegration of this

speciously univocal notion, in favour of formal criteria that represented logical possibilities. The result was the discrimination of six elementary modes in which rights could be transmitted from one generation to the next. An important aspect of this procedure, in connexion with the theme of the present essay, is that an insufficiently discriminative taxonomic concept was replaced by a set of criteria which might be matched only sporadically, and in highly various combinations, by the jural institutions of real societies.

A consequence of conceiving descent systems in this way, I then argued, was that among a number of societies compared in any formal respect there would not be presumed to exist any empirical feature common to all: 'in other words . . . they may not compose a class in the conventional sense' (1974a: 49). This was illustrated by a simple comparison (as in the present table 1) of three hypothetical

TABLE I. Serial likenesses among descent systems

A	<i>p, q, r</i>
B	<i>r, s, t</i>
C	<i>t, u, v</i>

societies (A, B, C), each constituted by three features (*p, . . . , v*).⁴ Let features *r* and *t* be each a type of right transmitted in a given formal mode, e.g., mode 1 (*m* → *m*), male to male (cf. Needham 1974a: 47). There is then a resemblance, *r*, between societies A and B, and another, *t*, between B and C, but none between A and C. Yet in ordinary anthropological practice these three societies could all be classed together as 'patrilineal.' 'A crucial misdirection can thus be given to our thought by the uncritical employment of the received idea as to what a class is' (50).

I have recapitulated this example in some detail in order to provide for a comparison with others that are to follow below, and it is on the paradigm case of descent systems that I wish to focus particular attention. But I should not allow it to be thought that it is only in the analysis of kinship and marriage that there are serious disadvantages for social anthropology in subscribing to a conventional but unrealistic idea of how a class is formed. On this score, let me just allude briefly to a far larger and quite fundamental issue, namely our conception of human nature.

In the development of anthropology the essential capacities of man, the intellectual and psychic resources that make up a common nature in mankind, have largely been taken for granted. In this century, it is true, there has been much prominent debate concerning the question whether all men reasoned alike, but at least it was never denied that reasoning was a universal human capacity. For the rest, the tacit presumption was that such capacities were well known, in advance of specific ethnographic comparison, and also that they were already adequately discriminated by European languages. Prominent among these components of human nature was the capacity for belief, and it has been a standard and unquestioned feature of ethnographic works that they ascribed to their subjects, i.e., to peoples belonging to quite disparate cultural and linguistic traditions, the common mental capacity of believing. It occurred to me, however, that this was in fact highly problematical, especially since philosophers in even our own tradition appeared increasingly baffled in their attempts to determine just what belief

consisted in. I therefore undertook an investigation of this problem (Needham 1972), from a comparative standpoint, and came to the conclusion that (to be very cursory) the supposed capacity for belief consisted in no more than the custom, in a particular linguistic tradition, of making statements about belief.

In the course of the argument that led to this conclusion, a decisive step was the analysis of the verbal concept of belief after the fashion demonstrated by Wittgenstein in his examination of capacities and states of mind such as seeing, comparing, being guided, feeling, expecting, and so on (cf. Needham 1972: 116-19). In these investigations Wittgenstein showed again and again that a verbal concept which was taken to denote a distinct capacity or inner state actually referred instead to a range of phenomena (experiences, utterances, actions) that were linked only sporadically into a class by their family resemblances. In each case there was no common feature among the phenomenal instances making up the class; and in any particular case, even if a common feature could be discerned, there would remain the question why that feature should be the capacity in question (cf. Needham 1972: 121).

The outcome, then, was that a distinct capacity for belief had been attributed to human nature on the basis of an incorrect inference from the uses of a verbal concept; and that the crucial and underlying mistake was the uncritical acceptance of a traditional definition (e.g., that of Jevons: 'the invariable presence of certain common properties') of the composition of a conceptual class. It is the recognition instead of a class composed by sporadic resemblances that makes the methodological connexion between the analysis of descent and the analysis of belief, between the comparative study of jural institutions and of human capacities. The present essay is intended to establish yet more firmly the taxonomic principle in question, and to indicate certain general and inescapable consequences of applying the principle in social anthropology.

III

When I wrote my 'Remarks' I was inspired directly by the writings of Wittgenstein, and I thought I was making an original application of his ideas to the practice of an empirical discipline. Certainly the venture was an independent one, and original within social anthropology, but it proved not to have been the first exercise of the kind. The encouragement to be had from this fact may be underlined if I state the circumstances of this realisation.

At the University of California, where I read my critique of kinship and marriage to a graduate class in 1971, it was mentioned to me that in zoology also a taxonomy existed in which classes of creatures were grouped by what were in fact family resemblances. Such classes, I was told, were termed 'polythetic.'⁵ So there were natural scientists, it appeared, who in their own field had already broken away from the scholastic tradition in the classification of the phenomena they studied. This was very interesting, and a subsequent investigation thoroughly confirmed an instructive convergence of taxonomic method. I present here the main points established, firstly in the hope that the prior recognition of polythetic classes, on the part of different sciences, may induce social anthropologists to take the more seriously the relevance of the taxonomic principle to the study of social facts. In the second place, the convergence in question seems to me to deserve notice for the sake of its intrinsic significance in the history of ideas.⁶

The starting point is the work of the great French botanist Michel Adanson, who proposed that a member of a class of plants did not need to possess all the defining features of the class, and that a deviant specimen did not need to be assigned to a separate class (Adanson 1763, 1: cliv sqq.).⁷ 'The important point he made was that creatures should be grouped together on the greatest number of features in common, and there is no justification for deciding *a priori* on the relative importance of characters in making a natural taxonomy' (Sneath 1962: 292).

In classical taxonomy, nevertheless, taxa were in fact usually described by specifying features that were absolutely diagnostic for the class of things considered. 'Linnaeus constantly revised his differentiae when the characters of newly discovered species showed that a character of a previously known taxon was no longer exclusive to that taxon' (Mayr 1969: 82), and this was a recurrent difficulty encountered generally by adherents to the common-feature principle of classification. But after 1859, and under the influence of the theory of evolution by natural selection, the definition of a class changed: 'the definition of the logicians—"individuals sharing common characters"—was replaced by "members of a group having descended from a common ancestor"' (Mayr 1969: 83). It was then no longer necessary to stipulate that the members of a taxon should invariably possess the definitive features in common; for 'there is no assurance that any given character of an ancestor will persist in all its descendants' (Sokal & Sneath 1963: 217).

In the eighteenth and nineteenth centuries, therefore, the grounds for a revision of taxonomic premisses were already well laid; but it was not until past the middle of the twentieth century that the development with which we are concerned was brought to a point of achievement. Sokal and Sneath write (1963: 13):

Biologists owe a debt of gratitude to Beckner (1959) for the first clear enunciation known to us of one important concept of natural taxa, a concept which Beckner calls 'polytypic.'

This concept, which significantly Beckner remarks is 'not restricted to biological theory' (Beckner 1959: 21), is contrasted with that of 'monotypic' classification, from which we may best approach its meaning. 'Monotypic' is a concept 'defined by reference to a property which is necessary and sufficient for membership in its extension' (23); i.e., it is equivalent to the traditional common-feature definition of a class. A 'polytypic' class, on the other hand, is formally defined (22) as follows:

A class is ordinarily defined by reference to a set of properties which are both necessary and sufficient (by stipulation) for membership in the class. It is possible, however, to define a group K in terms of a set G of properties f, f_2, \dots, f_n in a different manner. Suppose we have an aggregation of individuals (we shall not as yet call them a class) such that:

- 1) Each one possesses a large (but unspecified) number of the properties in G .
- 2) Each f in G is possessed by large numbers of these individuals and
- 3) No f in G is possessed by every individual in the aggregate.

By the terms of (3), no f is necessary for membership in this aggregate; and nothing has been said to warrant or rule out the possibility that some f in G is sufficient for membership in the aggregate.

Beckner goes on to state that a class is polytypic if the first two conditions are met, and 'fully' polytypic if condition (3) is also fulfilled. If the number of individuals is 'large,' all the members of K will resemble one another, though they will

not resemble one another in respect to a given f . 'If the n [the number] is very large, it would be possible to arrange the members of K along a line in such a way that each individual resembles his nearest neighbors very closely and his furthest neighbors less closely. The members near the extremes would resemble each other hardly at all, e.g., they might have none of the f 's in G in common' (22-3). In the case of a fully polytypic class, 'no f is universally distributed in the class' (24).

A consequence of a polytypic definition of classes is that 'there will always be the possibility of borderline cases. . . . Indeed, it is an essential aspect of polytypic classes' (24). Furthermore, Beckner continues (25):

In a sense, the whole point of polytypic . . . definition is to avoid committing oneself to a necessarily arbitrary delimitation of a class before a theoretically adequate definition can be found. . . . [It] leaves the borderline between K and non- K indeterminate where there is no theoretical reason for drawing the borderline at a particular point. Polytypic concepts are sufficiently justified if only on the grounds of scientific economy: new knowledge can be utilized in applying polytypic concepts without the necessity of modifying their definitions.

As an example of the application of this concept, Beckner later cites 'escape reactions,' such as heading for the rocks, withdrawing into a shell, running, and so on. It would be very difficult, he says, to attempt to specify a set of characteristics, other than the functional one, which all these responses, and no others, possess. 'The class of escape reactions . . . is fully polytypic with respect to those features of behavior observable in the single response, and with respect to movements in relation to any environmental coordinate system, e.g., movements toward or away from particular things' (122-3).

Here, then, we have a clear formal statement of the taxonomic principle that I was only later to advocate for social anthropology. But the great interest of the conceptual advance thus made is much augmented by a circumstance that I have not mentioned. In his commentary on the concept of a 'polytypic' class, Beckner makes a philosophical allusion (23) that presents a new aspect of the convergence of thought that I wish to register:

Wittgenstein has emphasized the importance that concepts of this logical character assume in ordinary language, especially in that small segment of ordinary language that contains the semantical concepts of 'meaning,' 'referring,' 'description,' etc. He points out that all the members of such classes have a 'family resemblance' to one another; he does not suggest a general term for classes of this kind.

Beckner does not cite any particular work by Wittgenstein, though it is most likely to have been the *Philosophical investigations* (1953). Nor does he say what part, if any, Wittgenstein's conception had in the formulation of his own statement of the principle, and on this point we may be satisfied enough just to note the reference.

The concept of a polytypic class was afterwards taken up by Simpson in his standard work on principles of animal taxonomy (Simpson 1961). There he first expounds the principles of hierarchy and key classification, and then presents by contrast an account of polytypic grouping, described as a method which results in the formation of a taxon in which each member has a majority of the total attributes. 'This is a perfectly definite, taxonomically valid and meaningful procedure that involves *no* characters in common . . .' (42). As a hypothetical example

of the method, he groups six individuals according to seven attributes (*a*, . . . , *g*) into two species, as in the present table 2.

TABLE 2. Polytypic grouping (Simpson)

Individuals:	1	2	3	4	5	6
Attributes:	a	a	b	a	a	b
	b	b	c	e	e	f
	c	d	d	f	g	g
	Species I			Species II		

The classification is adequate for definition, in that each species has a majority of the attributes. It is adequate also for diagnosis: ‘In Species I, each member has at least one of the attributes *c*, *d*, neither of which occurs in Species II, and in Species II each member has at least one of the attributes *e*, *f*, *g*, none of which occurs in Species I’ (42). Simpson refers in this connexion to Beckner’s formulation of the principle. In his discussion he adds the significant points, by comparison with traditional taxonomies, that ‘Species II has no “differentia” and . . . the two [species], although to us they seem obviously related, have no scholastic or Linnaean “genus”’ (43). Later in his text he gives an account of Beckner’s views on polytypy, as demonstration of the fact that ‘taxa may be recognized and defined by balances or chains of resemblances regardless of characters in common and without having abstractable archetypes’ (94–5). In this phrase there are three terms of special interest: balance, with its statistical implications; chain, with its figurative correspondence with Vygotsky’s idiom and its formal parallel with Wittgenstein’s overlapping fibres in a rope; and the idea of an abstractable archetype, as the metaphorical paradigm of traditional (monotypic) classification.

The designation of the principle, next, was called into question by Sneath (1962). In considering Beckner’s distinction between monotypic and polytypic classification, he raises the objection that these terms have other meanings that are already well established in systematics (291). As better alternatives he proposes ‘monothetic’ and ‘polythetic’ (Gr. *mono*, one; *poly*, many; *thetos*, arrangement). These terms have since been generally adopted in the taxonomies of various sciences (cf. Sokal & Sneath 1963: 13). Sneath glosses the notion of polythetic with a reference to phenetic groups which are ‘composed of organisms with the highest overall similarity, and this means that no single feature is either essential to group membership or is sufficient to make an organism a member of the group’ (291). This method of grouping is described as ‘Adansonian classification’ (292), which rightly confirms that the polythetic principle is by no means a modernity in natural science, let alone a novelty contrived in modern social anthropology. But Sneath at the same time makes the significant operational point that ‘Adanson’s ideas could not show their value until the statistical procedures which they required could be handled by modern electronic computing machines . . .’ (292). It is only in recent years, some two centuries after Adanson’s enunciation of the taxonomic principle, that this has at last been made possible and that the idea of a polythetic class has been applied to full effect. This has been done most notably in numerical taxonomy, where one marked advantage over monothetic classification is, Sneath claims, that

'numerical taxonomies . . . are unlikely to need radical revision, since, being polythetic, they are not liable to the catastrophic effects of finding that a character of presumed crucial importance is after all useless' (325).

Yet it was soon shown that a polythetic classification did not necessarily compel the revision of a monothetic classification of the same materials. Lockhart and Hartman, in a report on a test of method in quantitative bacterial taxonomy (1963), begin by defining polythetic groups as those in which 'no property is necessarily possessed by all individuals in the group, and no organism necessarily has all the properties generally characteristic of members of its group.' In consequence, 'any given organism may score mathematically as being equally similar (though in different respects) to two or more other individuals which, in turn, are quite dissimilar to each other' (68). Thus polythetic groups are 'not mutually exclusive' and are 'not theoretically analogous to the hierarchical taxa of present classifications' (69). Nevertheless, a computerised analysis of fifty bacterial strains, classed by sixty properties, first by polythetic grouping and then again by a monothetic grouping, produced little difference in the results. 'Rather curiously, our monothetic groupings were nearly identical to those obtained on a polythetic basis. It is conceivable, though it seems quite unlikely, that this apparent identity is coincidental and unique to the material used. . . .' These findings, which are illustrated by two remarkably congruent tree-diagrams (71, fig. 1; 73, fig. 2), suggest to the experimenters that 'the polythetic groups of organisms occurring in nature have a monothetic core of common properties' (76). This would entail, in their opinion, the allaying of fears that 'it might one day be necessary to abolish present classifications and to replace them with taxonomies and diagnostic schemes which somehow would take into account the multidimensional polythetic arrangements of organisms which actually seem to occur in nature' (77).

Within the same year as this critical test, Sokal and Sneath published a definitive statement of the principle of polythetic classification, in their comparative and methodological textbook on numerical taxonomy (Sokal & Sneath 1963). They first establish the principle of monothetic classification: 'the ruling idea of monothetic groups is that they are formed by rigid and successive logical divisions so that the possession of a unique set of features is both sufficient and necessary for membership in the group thus defined'; they are called monothetic, it is explained, 'because the defining features are unique' (13). 'A polythetic arrangement, on the other hand, places together organisms that have the greatest number of shared features, and no single feature is either essential to group membership or is sufficient to make an organism a member of the group' (14). For a formal expression of this concept, the authors declare that they cannot do better than to quote Beckner's definition (see above). They add, however, the operational qualification that natural taxa are usually not fully polythetic, since one can usually find some characters common to all members of a taxon: 'it is possible that they are never fully polythetic because there may be some characters (or genes) which are identical in all members of a given taxon.' All the same, they conclude, 'we must regard a taxon operationally as being possibly fully polythetic, since we cannot be sure that we have observed any characters that are common to all members' (14).

Sokal and Sneath supply as a formal illustration the scheme reproduced here as table 3. Here, 'the class of 1 + 2 + 3 + 4 is polythetic (and in this instance is also

TABLE 3. Polythetic arrangement (Sokal & Sneath)

		Individuals					
		1	2	3	4	5	6
Characters	A			A	A		
	B	B	B				
	C	C		C			
	D	D	D				
						F	F
					G	G	
					H	H	

fully polythetic, since no one character is found in all the four individuals). . . . Individuals 5 and 6, however, form a monothetic group' (15).

One of the difficulties of Beckner's definition, Sokal and Sneath add, is that in natural taxa we do commonly find features that are not possessed by large numbers of members of the class. 'Furthermore, we cannot test whether any given *f* [feature, character] is possessed by large numbers of the class before we have made the class, and therefore we cannot decide whether to admit this *f* into the set *G*' (15). But this difficulty, they state, can be avoided by defining membership of the class in terms of common (or shared) attributes. Further on the score of method, they add that polythetic groups can themselves be arranged polythetically to give higher polythetic groups, 'as is done in building a hierarchy in a natural system.' As for the consequences of adopting a polythetic taxonomic principle, the advantages of polythetic groups are that 'they are "natural", have a high content of information, and are useful for many purposes'; the disadvantages are that 'they may partly overlap one another (so that hierarchies and keys are less easy to make than with monothetic groups) and that they are not perfectly suited for any single purpose' (15). By way of a general reminder, the authors later stress that 'when . . . groups are polythetic ones, we must bear in mind that it is never certain, but only more or less probable, that a member possesses any given feature' (171).

Finally, for the purposes of the present survey, we may notice the cast more recently given to the principle by Mayr, citing Beckner and Simpson, in his textbook on systematic zoology (1969). 'Taxa characterized by a set of characters of which each member has a majority are called *polythetic* taxa. . . . No single feature is essential for membership in a polythetically defined taxon nor is any feature sufficient for such membership' (83). In fact, as he continues, many zoological taxa 'are based on a combination of characters, and frequently not a single one of these characters is present in all members of the taxon . . .' (88). Mayr's glossary defines 'polythetic' as: 'Of taxa, in which each member has a majority of a set of characters' (409). In this formulation of the principle, then, the stress is laid on the possession by each member of a class of a simple majority of the defining features, without, that is, the assignment of decisive weight to any of them.

IV

We see therefore that polythetic classification, far from being a unique innovation in current social anthropology, is a recognised taxonomic principle in a range of natural sciences.

In the adoption of this principle two circumstances appear to have been crucial: the promulgation in 1859 of the theory of evolution, when natural history challenged the logicians' method of classifying species; and the development, a hundred years later, of electronic computers which made it practicable at last to put into scientific effect the precept enunciated by Adanson in 1763. In the outcome, polythetic classification became employed in sciences so various as botany, zoology, biology, and bacteriology, and it carried fundamental implications for each. The empirical application of the method, and the elaboration of the principle, were in the main the work of a period so recent as the nineteen-sixties.⁸ In the formal expression of the concept of polythetic classification, a specially significant point is the allusion by Beckner to Wittgenstein's emphasis on concepts of the same logical character in ordinary language. This neatly clinches a noteworthy convergence in the history of ideas, and on this score the main part of my case is made.

Let us now examine some of the consequences for social anthropology that may be derived from this better appreciation of the polythetic principle. This will have to be done in fairly general terms, without a conclusive empirical demonstration of each point of argument, but the practical relevance of the principle to the comparative study of social facts should be evident enough.

I have already entered the caution that, in the study of descent systems in particular, a direct consequence of polythetic classification in anthropology is that 'comparison becomes far more difficult, and on any large and detailed scale perhaps impracticable' (Needham 1974a: 50). In the study of relationship terminologies, similarly, the consequence is that 'while analysis is made more exact, comparison is made more intricate and difficult' (60). More widely, the consequence is the realisation that the social phenomena classed as 'kinship,' etc. 'do not in all cases possess any specific features such as could justify the formulation of general propositions about them' (69); 'there may . . . be something in common, under each general term [e.g., 'marriage,' 'incest'], but not necessarily a definite set of characteristic, specific, or essential features' (70). Operationally, this means also that attempts to find statistical correlations are seriously affected, for these cannot be established without a precisely specified typology of phenomena; but if it is conceded that the social facts in question do not necessarily compose a conventional class of a homogeneous kind, under each type, 'but may instead exhibit an immense array of serial and more complex resemblances,' then the grounds for this method of comparison and explanation are removed (70).

By this account of the matter, the first consequence of the adoption of polythetic classification in social anthropology is that comparative studies, whether morphological or functional or statistical, are rendered more daunting and perhaps even unfeasible. Yet polythetic classes are likely to accommodate better than monothetic the variegation of social phenomena: they have, as Sokal and Sneath put it, a high content of information, and they carry less risk of an arbitrary exclusion of significant features. In other words, it could be said, the polythetic principle is truer to the ethnographic materials. If this is so, then the increased difficulty of comparison is a price that simply has to be paid—if there is some way of meeting it.

This is a point on which social anthropology may have much to learn, by way of explicit method, from the sciences in which the polythetic principle has already

been well recognised. We may find special instruction, in the first place, under the aspect of evolution, since this is so basic to the sciences whose examples we have just considered. I shall therefore deal in some detail with the methodological implications of studying evolutionary materials.

One very striking difference between the materials that a natural scientist has to classify and those which are the concern of the social anthropologist is the presence or absence of evolutionary connexions. It is the factor of common but divergent descent that permits a zoological taxon, for instance, in which not a single one of the definitive characters is present in all members of the taxon (Mayr 1969: 88). This is an acceptable grouping of natural entities, whether animals or plants or bacteria, because lineages of descent can be demonstrated; and this can be done even if intermittently the original characters have changed or fallen away, under the pressures of adaptation, until the descendants bear little resemblance either to one another or to their common ancestor. It is doubtless the factor of phylogenetic descent that encourages some among even the most modern taxonomists to speak of 'natural' groupings of organisms, and to distinguish such aggregates from 'artificial' groupings. I suppose, too, that it is phylogeny that largely accounts for what Lockhart and Hartman isolate as the 'monothetic core of common properties' in polythetic classes of bacterial strains (1963: 76). We might therefore sum up the situation in the natural sciences by saying that, as intrinsic features of the materials under comparison, common descent composes natural classes while natural selection variegates the members of such classes.

In the comparative study of social facts, however, these conditions do not obtain. It is not without good reason that social anthropologists today pay little if any attention to evolutionary connexions among social forms, to the extent indeed that it is hard to find a modern instance of such an approach.⁹ There is, though, one study of the kind, carried out on a scale comparable with that of zoology or bacteriology, in Murdock's *Social Structure* (1949). In his chapter on 'Evolution of social organisation' Murdock propounds five generalisations about 'the normal order of change among the principal elements of social organisation' (221-2), and he applies these generalisations to his sample of 250 societies. He distinguishes eleven 'major types of social structure' defined by rules of descent and types of cousin terminology (224), and these major types are sub-divided by rules of marital residence into a total of forty-seven 'structural subtypes' (324-6). A technique for the reconstruction of antecedent forms is presented (326-31), and each of the societies in the sample is then assigned to a sub-type and thence derived from a series of prior sub-types (332-4). The enterprise is painstaking and detailed, and consistently worked out; but it does not serve to establish a correspondence between natural evolution and social evolution such as could now permit the application in social anthropology of the taxonomic procedures that we have observed in zoology and other natural sciences. I do not refer to defects in the stipulation of those features (e.g., 'cousin terminology') by which the types and sub-types are distinguished, for in principle these could be amended and the evolutionist method remain feasible. What is a graver criticism is that Murdock's evolutionary scheme permits the establishment of such a proliferation of transitional connexions, many of which are reversible, as to prompt doubt concerning its general validity (Needham 1962: 173-80). Certainly there is extraordinarily

little resemblance between the 'evolutionary' connexions among a set of eight societies that I selected from the sample (178, fig. 1) and the phylogenetic trees that can be established in the natural sciences. Another source of doubt concerning the scheme is that by the introduction of an additional discriminating feature (*viz.*, prescriptive alliance), that Murdock did not take into account, it can be shown that a number of societies in the sample can be placed in quite different relative positions from those which they occupy on Murdock's premisses (175). This implies that sociological taxonomy is highly artificial (as in fact it is), and such that the evolution of institutions may be disparately inferred according to the typological features that are more or less arbitrarily decided upon. There is no sign, at any rate, of a correspondence with quasi-natural monothetic clusters of features. Rather, it is the construction of a sociological typology of cultural particulars by monothetic criteria, and these too of the most slender and unreliable kind, that is the chief and irremediable weakness of this evolutionary scheme.

Now Murdock's venture is admirable in its own terms, and in some ways the most impressive evolutionary undertaking in anthropology, but as soon as the polythetic character of the resemblances in question is recognised, the grounds of the taxonomy on which the study rests are quite removed. For example, the Araucanians, Iatmul, Miwok, Nyoro, and Thado are all typed together as 'Normal Omaha' (Murdock 1949: 240), on the basis of patrilineal descent and the terms for cross-cousins (239, 244), whereas the quickest survey of the ethnographic evidence on each society shows that their forms of social classification, not to mention their institutions, are systematically disparate (Needham 1974a: 51; cf. Lowie 1917, ch. 5). The concept of 'descent,' moreover, is itself polythetic (47), and so also are the conventional types by which relationship terminologies are classed as 'Omaha,' etc. (47-54, 59-60). When, therefore, the descent systems under comparison are analysed by polythetic criteria, instead of being typed by a few monothetic features (60), the presumed resemblances are reduced or abolished; the comparison is vitiated, and the attempt to work out evolutionary interconnexions is thereby doomed to failure.

This particular outcome does not mean, however, that no form of evolutionary reconstruction is possible in the comparative study of institutions. I have suggested indeed that prescriptive systems tend to change in regular ways (Needham 1967: 46), and it seems quite feasible to establish regular transformations which bear a resemblance to evolutionary modifications in natural species. Admitted, this line of enquiry has not yet been taken very far, and the series of transformations so far proposed is very tentative, but there remains nevertheless some empirical justification for speaking in these regards of the 'evolution' of social forms. Yet it does not at all follow that the taxonomic methods of the natural sciences, e.g., zoology, can therefore be directly taken over in social anthropology. The crucial feature that is missing from the prescriptive systems compared is still that of common origin. If there is a precise structural correspondence between the terminologies of the Tibeto-Burman Garo of north-eastern India and the Malayo-Polynesian Manggarai of eastern Indonesia (Needham 1966: 155, table 4), this is not because these societies are descended from a common ancestral society. Still less is such an evolutionary connexion the explanation of the resemblances among other two-line terminologies in Australia, south India, and the tropical forests of south America.

In fact, the type of evolution at work in these instances is no more than formally similar to that of natural species.

The outcome, in any case, is certainly not much like that in zoology, for example, for whereas a zoological taxon may comprise very dissimilar individuals, the anthropological taxon of prescriptive systems of social classification is so defined that the individual terminologies shall be distinctly alike. The points of resemblance are not cultural particulars but analytical abstractions; prescriptive terminologies are thereby classed monothetically. The resultant class of social facts, defined by a common feature (the invariant relation articulating the categories) is artificial. All the same, the label 'prescriptive' does not denote a class of societies to which the law of substitution can be applied, for in prescriptive alliance systems (conceived as empirical forms of social life) the categories, marriage rules, groupings, and modes of social action are independent variables (Needham 1973: 174). In other words, whereas prescriptive terminologies are defined monothetically, the class of 'natural' societies characterised by jural categories of this type is polythetic. It may be possible to list certain characteristic features of such societies (Needham 1970a: 257; cf. Korn 1973: 100-4), but the societies do not compose a class such that 'whatever we know of one object . . . we also know of the other objects.'

Even in this quite promising line of 'evolutionary' investigation, therefore, the parallels with a natural science are superficial. An abstract stipulation occupies the place of a common ancestor; the similarity of social forms has nothing to do with common descent; and the postulated transformations of prescriptive systems appear causally different (Needham 1967: 47) from the evolution of natural species. These essential contrasts make it hardly likely that the methods of the natural sciences, in coping with polythetic classes of evolutionary materials, should be directly transferable into social anthropology.

Nevertheless, the polythetic approach in the natural sciences can still seem to offer the promise of a method that might be adapted to the concerns of social anthropology. This is in the practice of numerical taxonomy, and particularly in the method of 'cluster analysis.' We have seen that Adanson's principles demanded statistical operations that were not practicable until the development of electronic computers. Likewise, an obvious difficulty in the project of large-scale comparison of social facts has long been that the great numbers of variables involved made the task prohibitive; and this source of difficulty is very much magnified when the evidences compared are defined polythetically. But in quantitative bacteriology Lockhart and Hartman have managed to compare fifty organisms by reference to sixty properties such as production of indole, colony morphology, and tolerance to sodium chloride (1963: 69-70). This is impressive as it stands, but the researchers add that a certain computer could be programmed to handle as many as 1,500 bacterial strains, each one scored for 200 properties. With figures so high as these we approach the sort of computation that presumably would be called for in the comparison of social facts. If therefore the polythetic classification of bacteria can be coped with by an appropriate computer programme, perhaps the taxonomic principle in itself need no longer be seen as an obstacle to effective large-scale comparison in social anthropology.

Unfortunately, Lockhart and Hartman's experiment does not make the case, for by their method the groups 'are made monothetic by discarding all characters

which vary within them' (Sokal & Sneath 1963: 188). Without going into the technicalities of this procedure so far as bacteriology is concerned, we can at once see that it introduces an arbitrary factor such as in social anthropology we seek to obviate precisely by the adoption of polythetic classification. In the study of social facts what is needed is not a convenient technique for cutting down the number of variables, but a means of accommodating as many as possible. The method of cluster analysis can be justified in the case of Lockhart and Hartman's experiment by the result that the polythetic groupings were nearly identical with the monothetic; but this can be explained by their suggestion that polythetic groups of organisms occurring in nature have a monothetic core of common properties, and such phylogenetic properties do not characterise social facts. We can well follow the bacteriologists in their application of the taxonomic principle, but the evolutionary nature of their materials introduces a difference of kind which prevents us directly taking up their statistical methods.

There are in any case other reasons for not turning to cluster analysis for the purposes of anthropological comparison. Quantitative methods in taxonomy are 'based on the principles that classification is a measure of over-all similarity among organisms and that all properties of organisms are potentially of equal value in creating taxa, so that no *a priori* assumptions need to be made of the relative importance of particular features' (Lockhart & Hartman 1963: 68). Simpson writes of 'balances of resemblances regardless of characters in common' (1961: 94-5); Sneath refers to the 'highest overall similarity' (1962: 291); Mayr defines a polythetic taxon as one in which each member possesses simply a 'majority' of a set of characters (1969: 83, 409). This approach entails a number of difficulties when we conjecture its possible use in the comparative study of social facts.

To begin with, 'it is reasonable to ask whether a definition of a polytypic concept is after all a *definition*, since it is certainly imprecise' (Beckner 1959: 24). In Beckner's formulation, as he points out, 'the vague term "large number" occurs twice in the definition' (24), and there is no rule of method for deciding in general, or for any given context, what is to count as a large number. The same kind of uncertainty attaches to the phrases 'over-all similarity,' 'balances of resemblances,' and a 'majority' of characters. No matter how the definition of a polythetic class is expressed, the difficulty is to know where to draw the line. This problem is not resolved by the admission that 'there will always be the possibility of borderline cases' (Beckner 1959: 24), for the location of the border is itself a function of the degree of numerical preponderance that is thought sufficient, and this in principle is always contestable. In any event, the consequence is that 'it is never certain, but only more or less probable, that a member [of a polythetic group] possesses any given feature' (Sokal & Sneath 1963: 171). A numerical taxonomy, therefore, leaves the social anthropologist in much the same definitional quandary as when he is faced with the question what is to count as an instance of a given institution. For example, in a reassessment of the concept of marriage Rivière has concluded that 'we mislead ourselves by describing with a single term relationships which in different societies have no single feature in common other than that they are concerned with the conceptual roles of male and female' (Rivière 1971: 70). That is to say, 'marriage' is what Wittgenstein called an 'odd-job' word (cf. Needham 1972: 124-5; 1974a: 44); or, as we might better say now, it is a poly-

thetic concept. No statistical method or computer programme can decide what is to count as marriage. Concomitantly, to identify a particular social relationship as an instance of marriage, by a polythetic definition, will not demand the presence of any specific feature by which it is possible to decide that it is to count as marriage.

On the other hand, a polythetic conception of marriage, as determined by overall similarities, still does not preclude the risk of leaving out of account some feature that is regarded indigenously as essential to the relationship, or one that might be more relevant to the purpose of the comparison. This is especially likely when what is in question is an evaluation placed on the relationship, e.g., a moral repugnance for divorce, which happens not to be present in a majority of the cases classed together. In general, indeed, human affairs are semantically so very complex that it must be difficult in the extreme (if it is even conceivable) either to stipulate significance as a polythetic feature or to assess the degree of similarity among the meanings or values attached by different civilisations to any kind of institution that is the subject of a comparative proposition.

Another difficulty is exposed by the possibility in natural science of determining features by difference, e.g., by whether or not an organism produces indole or ferments lactose. 'There may be endless arguments as to whether two organisms are similar in that neither ferments lactose, but everyone agrees that they are different if one does so and the other does not' (Lockhart & Hartman 1963: 70, 77). But what in the study of human affairs would permit this decisive treatment? At the level of social facts, as distinguished from analytical abstractions, it will be often impossible and usually dubitable to determine such absolute contrasts. For that matter, though, it is not always so easy even in the natural sciences to determine features so unambiguously. In bacteriology, for example, the Lockhart and Hartman experiment classified organisms by features such as colony morphology and tolerance to NaCl, but these involved relative discriminations and hence a degree of arbitrariness. Three forms of colony were scored, *viz.*, punctiform, circular, and 'irregular'; and tolerance to NaCl was scored at four levels as sensitive, weakly resistant, moderately resistant, and strongly resistant (Lockhart & Hartman 1963: 70). These morphological and scalar discriminations are uncertain and at least imprecise, and some are no more dependable than are the criteria often employed in social anthropology. Moreover, even on the assumption that features can be well determined, numerical taxonomy has a disconcerting consequence that must limit or even rule out its application in social anthropology. In a polythetic group, as will be remembered, no property is necessarily possessed by all members in a group, and no individual necessarily has all the properties generally characteristic of members of its group. Hence, in bacteriology, 'any given organism may score mathematically as being equally similar (though in different respects) to two or more other individuals which, in turn, are quite dissimilar to each other' (Lockhart & Hartman 1963: 68). Clearly this is an outcome that would be quite unacceptable in the study of social facts.

Underlying all these difficulties that lie in the way of adapting numerical taxonomy to the polythetic concepts that are none the less called for in social anthropology there is a more fundamental obstacle. In the natural sciences the features by which polythetic classes are defined have generally a real, distinct, and independent character, and they can be clearly stipulated in advance. Such features

are, in zoology, skeletal structure (a definite number of bones functionally arranged in a certain order); in botany, roots, leaves, pistils; in bacteriology, chemical elements, compounds, and their reactions; in many sciences, at a deep level of analysis, molecular structures and the particles of which these are composed. This is a rather rough and ready characterisation, of course, and it becomes less appropriate when ultimately the character of 'fundamental' particles comes into question, but it serves all the same to make a crucial contrast between the natural sciences and social anthropology. In what has been presented here as the most relevant example of taxonomic method, namely quantitative bacteriology, the researchers are in no doubt concerning what is or is not lactose or about whether it is or is not present: it can be exactly defined in advance, and its chemical properties and reactions are known or testable. This kind of certainty about the materials under study (whatever ambiguity may attend the discrimination of forms or the assessment of degrees of resistance, etc.) permits the method of classification by differences: a definite feature can be definitely determined as either present or absent.

But in the realm of social facts this aspect of polythetic classification is hardly to be found. A main reason is that in social anthropology the determination of the constituent features of a polythetic class cannot be carried out by reference to discrete empirical particulars, but entails instead a reliance on further features of the same character which themselves are likewise polythetic. In social life, that is, there are no established phenomena, in the form of isolable social facts for instance, which correspond to the elements and particles in nature. The disparity between the natural sciences and social anthropology, in taxonomic method as in much else, reflects a contrast of kind between natural entities and social facts. This contrast is the most marked when the materials for an anthropological classification are collective representations. More generally, in any case, there is no reason that a classificatory technique that is appropriate to one kind of evidence should be applicable to another, and all the less is this so when the evidences in question are contrasted as physical and ideational.

V

In this article I have set on record what strikes me as a remarkable convergence in the history of ideas, and I have briefly considered certain methodological consequences of the discovery that classification by sporadic likenesses is already practised in certain natural sciences.

But the results are not encouraging, for whereas the polythetic methods developed in those sciences are much in advance of those suggested for social anthropology (Needham 1971*b*), both in their formal expression and in their empirical application, there are serious objections to the adoption of such methods into the study of social facts. This setback is not the fault of the taxonomic principle, but has to do with the essential disparity between natural phenomena and collective representations. In social anthropology the idea of a polythetic class does indeed proffer a great rectification of thought and a newly panoptic perspective on human affairs. But at the same time this vantage offers an austere prospect, for amid so many uncertainties as we have glimpsed the one sure outcome is that comparative studies, if they are carried out in empirical terms, will become irresolubly compli-

cated. Polythetic classification therefore tends, by this view, to confirm Evans-Pritchard's aphorism: 'There's only one method in social anthropology, the comparative method—and that's impossible.'¹⁰

There is a great deal to be said perfectly seriously in support of this quip, but the issue is not quite so decided as it implies. Comparative studies are likely to be defective and unproductive so long as they continue to be carried out within conventional, i.e., monothetic, taxonomies and by reliance on substantive paradigms (Needham 1974a: 53, 60). But comparison stands a better and quite different chance of success if it is conducted in formal terms. There have been prominent examples of this kind of approach under the rubrics of 'elementary forms' and 'elementary structures,' and it could yet be pursued further by the formulation of a 'vocabulary of analytical concepts that were appropriate to the phenomena under consideration but would not be merely derived from them' (Needham 1974a: 16). Not only are the terms of common English, such as 'kinship' or 'marriage,' worse than unserviceable in this enterprise, but so also are the quasi-technical terms of anthropology such as 'unilineal,' 'Omaha,' 'patrilocal,' and so on. As will be appreciated by this point, a characteristic flaw in such terms is that they have been presumed to denote monothetic classes of social facts, and thus to permit the operation of substitution; whereas in fact their reference is polythetic, so that comparative propositions are perpetually undermined by the awkwardness that the absence of common features precludes the possibility of substitution. But this objection does not attach to relational concepts such as 'symmetry,' 'alternation,' 'transitivity,' 'complementarity,' etc., or to analysis by reference to logical possibilities (Needham 1974a: 16, 47; 1974b: 39). Here we have formal properties which can be defined in purely formal terms, e.g., in the notation of symbolic logic, without reference to any classes of entities, however the classes may be composed, or to the characteristic empirical features of their members.

Formal analysis of this kind is not merely a methodological postulation, nor just a critical technique in the undermining of received categories of anthropological discourse. In practice, it accurately accommodates social facts, it facilitates systematic analysis, and it makes possible the effective prosecution of comparative studies. In each of these regards this radical style of abstraction evades the disastrous deception of reificatory and monothetic taxonomies; and it renders unnecessary a desperate contention with the alternative hazards of trying to theorise taxonomically about classes of facts that in empirical terms are polythetic.

The formal terms mentioned above, together with others that propose themselves as appropriate to any relational enquiry, have been arrived at largely inductively through their application in the empirical analysis of various types of society. They also happen to accord, however, with certain philosophical conclusions arrived at by Campbell in his study of family resemblance predicates (1965). He argues that, for any given linguistic context, not every predicate can be of this kind; 'it follows that the notion of family resemblance cannot of itself solve the problem of universals'; there must be, for any such context, what he calls 'basic predicates' (243). In social anthropology there have been repeated attempts to establish ultimate theoretical notions, relating to social life and collective representations, in morphological or functional or structural terms, or else by resort to universal needs or motives or mental capacities. These ventures have not proved

successful, but the relational concepts that I have adduced are well adapted, I suggest, to the analytical task of taking each case as it comes and to the comparative task of formulating propositions about classes of social facts. There is pragmatic reason to think that such purely formal terms may provide, for certain purposes, the basic predicates that are called for in social anthropology.

The present article also accords in another important regard with the conclusion to Campbell's analysis. Where we wish to make generalisations in the confidence that they admit no exceptions, he writes, defined terms are to be preferred, other things being equal, to family resemblance terms: 'We should not rest content until family resemblance predicates, admittedly intelligible, have been banished from our sciences' (Campbell 1965: 244). This is exactly the aim of my 'Remarks,' to which this essay forms a sequel, though I do not think that such (polythetic) predicates can ever be eliminated from practical description in the field or from academic discourse about ethnographic reports. Where they can deliberately be dispensed with is in the contrivance of a formal theoretical terminology. Studies of prescriptive alliance systems show particularly well that a definition which is designedly monothetic, in combination with analytical concepts which are necessarily of the same character, can advantageously be applied to classes of social facts that are extensively polythetic.

There is a great deal more that could be written about these fundamental topics, and what I have written here is no more than groundwork for the formulation of theoretical issues, both formal and analytical. Also, if the appreciation of polythetic classification is to have its proper effect, it is desirable that more detailed empirical demonstrations should be fully set out. For the present, though, I conclude with two observations that indicate something of the relevance of this theme to ultimate concerns in the understanding of human affairs.

The first is to take up the point that 'our analysis may be guided by the same logical constraints as must have been effective in producing the systems that we study' (Needham 1974a: 36, 48; 1974b: 39-40). This is particularly clear in the comparative study of descent systems, terminologies, and certain rules of marriage, but there is a wider implication. If we consider that formal terms such as 'symmetry' or 'transitivity' are not peculiar to a particular linguistic and intellectual tradition, but denote properties which must be discriminable (either conceptually or in social practice) by any cultural system of thought, then it follows that the terms are intrinsically appropriate to the study of exotic collective representations. Alternatively, a more speculative notion is that the formal terms denote mental proclivities and constraints which are universal to mankind in the fabrication (deliberate or not) of categories and articulatory relationships. According to either of these conjectures, the kind of theoretical terminology to which I have referred would thus naturally qualify as basic predicates.

The other comment, linked to the former, is to stress the conception of social anthropology that inspires the present essay. The approach is guided by the ambition, inherited from the *Année sociologique* school and directly traceable back to the Enlightenment, to determine what can be called the primary factors of experience by the comparative study of social facts. The facts at issue in the 'Remarks' and in my analysis of belief were words, *viz.*, verbal concepts framed by cultural traditions in the classification of the world, whether this was the

ordering of social life or the ascription of capacities to the human mind. In the analysis of such collective representations, whether they were the quasi-technical generalisations of social anthropologists or the ordinary resources of everyday language, it was found that certain deep miscomprehensions were ultimately the results of the traditional assumption that classificatory concepts were necessarily composed about common definitive features, i.e., that they were monothetic. In each case, however, it was shown that the words in question actually denoted classes composed by family or sporadic resemblances, i.e., that they were polythetic. Now the outcome of analyses of this kind should not be seen as merely a local or technical rectification of European academic argument, but as pointing to a general hazard of language which presumably afflicts men in any tradition when they classify their fellows and their nature. 'In seeking to translate alien concepts . . . we have to appreciate that the foreign words in question are themselves words that may be in the same state as our own,' so that the speakers of another language, constrained through it by their own collective representations, 'must be assumed to be the victims of just such linguistic defects, traps, and diversions as are we ourselves when we formulate our own thoughts' (Needham 1972: 233). Thus the realisation of the confusions brought about in social anthropology by stock classificatory terms may serve to prepare our understanding in coming to terms with alien concepts which, in a fashion that is similarly unrecognised by those whose modes of thought we want to comprehend, are also polythetic.

NOTES

¹ For a brief commentary on the history of the 'family' metaphor, see Needham (1972: 113 n. 4). To the writings cited in that place may be added the specific parallel presented by Nietzsche (1886: 25), who referred to the similarity of Indian, Greek, and German philosophising as literally a 'family resemblance' (*Familien-Ähnlichkeit*). We know moreover that Nietzsche, like Wittgenstein, was an admirer of Lichtenberg, a notable predecessor in this train of thought; he judged Lichtenberg's aphorisms to be worth a place in a 'treasury of German prose,' and found them worth reading again and again. It should be remembered, all the same, that *Familienähnlichkeit* was in literary use over a century before Wittgenstein wrote the *Philosophical investigations*. Grimm, *Deutsches Wörterbuch* (vol. 3, 1862) glosses the word with the Lat. *gentilis similitudo* (1306, s.v.). Mr Thomas Braun has directed me to the statement in the preface (vol. 1, 1854, cols. xxxix-xli), where it is explained that Latin equivalents are supplied as the best means to clarity; and he has kindly given me his opinion also that the phrase is not a literary tag such as would indicate a classical origin of the notion. For a good philosophical analysis of this idea, see Campbell (1965).

² In an earlier reference to this convergence (Needham 1972: 111-14), I thought I was reporting a discovery, but I afterwards found that I had been anticipated by Stephen Toulmin (1969). I regret my ignorance of Toulmin's essay, which I should have been happy to adduce. Toulmin reports that he read Vygotsky in alternation with Wittgenstein's *Zettel* (1967) and found his head ringing with intellectual echoes: 'the theoretical parallels, the similarities in general attitude, even the tones of voice of the two men were too close to be entirely independent' (71-2). He suggests moreover an indirect connexion between Vygotsky and Wittgenstein through the psychologist Karl Bühler, who is frequently cited by Vygotsky in *Thought and language*, was an acquaintance of Wittgenstein's sister (at whose house Wittgenstein could have met him), and was also 'a major contributor to modern linguistic theory' (72).

³ On the ambiguity of the idiom of 'family' resemblances, see Needham (1972: 112-13). I have previously alluded to such resemblances as 'serial likenesses' (1972: 119 n. 6; 1974a: 49), but on the whole the qualifier 'sporadic' seems the most apt.

⁴ That three features were assigned to each society means that formally speaking the demonstration was not conducted in the simplest possible terms; but the logically redundant features emphasized the point, I thought, and conduced perhaps to an easier assimilation of the principle to empirical instances.

⁵ I am very grateful to Mrs Aileen Garsson Baron, then a graduate member of the Department of Anthropology at the Riverside campus of the University of California, as being thus 'the onlie begetter' of this insuing study.

When I returned to Oxford I remembered the term and asked occasionally about it, of biologists and others, but drew blank. The matter then receded from my attention, under the pressure of other concerns, until December 1974, when Dr R. N. Pau, of the Department of Zoology at Oxford, delivered a seminar paper to social anthropologists on the subject of primitivity, and hence taxonomy, in zoology. When I asked him about polythetic classes he very kindly supplied me with two important references (Sokal & Sneath 1963; Mayr 1969) which laid the basis for the present article.

⁶ On the historical count, I should point out that for the present purpose I think it sufficient to make only a summary survey of certain of the sources. There remain a great many more (see, e.g., the bibliography to Sneath 1962) which in a full reckoning, by an historian of science perhaps, would deserve exploitation. For a short but fundamental statement of the development of taxonomic theory since 1851, see Gilmour (1951).

⁷ It is only by way of this reference, I am afraid, that I have belatedly picked up the significance of Bambrough's allusion to 'botanical taxonomists' in his demonstration that five objects may each have four out of five given features and that the missing feature may be different in each case (Bambrough 1961: 209-10). The example he gives is, with a slight alteration in designations: (1) *A B C D*, (2) *A B C E*, (3) *A B D E*, (4) *A C D E*, (5) *B C D E*.

⁸ To stress the temporal continuity in the convergence that I am demonstrating, it may be noted that Mayr's book on systematic zoology was published in 1969 and that the idea of classifying by 'serial likenesses' in social anthropology was proposed to the A.S.A. conference on kinship and marriage in the spring of 1970 (cf. Needham 1971a). The interval between even Beckner's monograph and the delivery of the conference paper was a mere eleven years.

⁹ Evolutionist explanations, such as were common in the last century, are sharply to be contrasted with historical (or 'diffusionist') reconstructions, the feasibility of which is another matter altogether (Needham 1970b: lxxv-lxxxii).

¹⁰ He said it to me, and doubtless to others as well, but so far as I know it does not appear in any of his published writings. Cf. Evans-Pritchard (1965).

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