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INFORMATION FLOWS IN KINSHIP NETWORKS ¹

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Abstract:

The capacity of kinship networks to be multi-functional, i.e. to shape other domains of the social life such as religion or the economy, reflects their function of channeling information flows. The more tightly the kinship network is structured into self-reproducing exogamous units, the better it resists the historical trend of losing its function as information channel and jointly its grip on the other aspects of the social life. For kinship networks to hold their multi-functionality, do actors need to be aware of their structure? In other terms, do the rules need to be explicit and followed in full awareness by the participants to the network? Or are the structuring principles able to operate behind the scenes even when their subjective representation is absent? The author reports on this the views expressed in personal conversations by his former professors: Lévi-Strauss, Fortes, Leach, Needham, Goody, Barnes and Macfarlane. In supporting the second view, that the awareness of the actors is indifferent, quantitative anthropology and psychoanalysis reveal their surprising affinity.

Introduction

Kinship networks are here envisaged with the emphasis put on their capacity of representing the transmission of information flows. This is somewhat artificial and reductive but presents a number of advantages, among which those of

1. Explaining the demise of kinship as a structuring backbone of society when the type of information kinship networks are likely to convey becomes socially irrelevant,

¹ My work on kinship networks is indebted first to my mentors who led me to think of structure: Claude Lévi-Strauss, Georges-Théophile Guilbaud, Edmund Leach and last but not least, Jacques Lacan (I published the synthesis of my research on kinship algebra in a journal, *Ornicar ? Revue du Champ Freudien* whereof Lacan was the founder); secondly to the co-authors of books and papers I wrote jointly with them on the subject: Geneviève Delbos, Douglas R. White, Gisèle De Meur, Michel Verdon and Elaine Lally; and thirdly to two passionate fellow researchers of mathematical anthropology: Franklin Tjon Sie Fat and Chris Gregory: our late at night and early morning conversations on formalisation of kinship have remained over the years a constant source of inspiration.

² Director, Du sujet: Théorie et Praxis

2. Clarifying the issues raised – sometimes justifiably – by the theoretical opponents of a « social networks » approach to kinship facts, among whom some of my former teachers³.

I borrow from a branch of artificial intelligence, « qualitative physics », the usage of interpreting the directed edges of a network as « conduits » where information circulates unhindered between *processors*, and the vertices as « processors » where information is processed and transformed in a way that can – ideally – be fully described by the application of an algorithm. The converse convention of interpreting edges as processors and vertices as conduits would be counterintuitive. Indeed the homology between a directed edge and circulation of an undisturbed flow within a tube is visually immediate; similarly, for the homology between the box containing the circuitry of a processor and a vertex in a graph.

1. Kinship as a biological given

Whatever discursive constructs human cultures have generated about the biological facts of reproduction – picking some aspects as culturally relevant, discarding some others – there is one intractable fact which cannot be set aside: the observation *a minima* that human beings are born from the womb of a woman who was herself born a number of years before, i.e. earlier in time (Jorion 1984a; White & Jorion 1996).

Biology teaches us that the morphogenesis of a new human being is determined by the information encrypted in its⁴ genome. Apart from the exceptional case of parthenogenesis, an individual's genome combines information from two sources: the genome of a woman, its biological mother, and the genome of a man, its biological father.

In this biological perspective, the convention we have adopted with *p-graphs* is perfectly consistent (Jorion & Lally 1983, White & Jorion 1992, White & Jorion 1996) of representing a human being as the directed edge linking the vertex standing for its own conception and that standing for the conception by it of its offspring. Indeed individuals transmit their genome from the processor which combined their parental genomes to the processor combining the genome of the biological parents of their offspring.

I will call « marriage » (*marital behaviour*) such combination of their genome by biological parents, nothing more should be read in the word which is used for convenience only.

Within the context of a strictly prescriptive marriage systems, e.g. « 5 x 7 » Murngin (see Jorion 1993), providing that

³ My first – and more highly formalised – attempt on this path was a paper I presented in 1981 at the Department of History and Philosophy of Sciences of Cambridge University, at the kind invitation of Mary Hesse: « Understanding and explanation in social anthropology. The conflict between anthropological theory and mathematical anthropology ».

⁴ I adopt here the linguistic convention of referring to a human being as « it ».

1. there is an initial historical state when all men within a *latent section* (see the phrase justified below) share an identical genome, all women in a *latent section* share a distinct but identical genome,
2. mutations are negligible,
3. male and female average generation length is consistent with the prescriptive marriage system,
4. marriage rules are strictly observed,

genomes would be strictly reproduced within the society, in terms of being identical within each latent section, that of men according to the cyclicity of male generations (e.g. Murngin: patricycle of length 5), that of women according to the cyclicity of female generations (e.g. Murngin: matricycle of length 7).

2. *Multifunctional kinship networks*

Let me now get back in time in the history of anthropology and let me remind you briefly how the network representation of kinship originated. Such initial clarification allows avoiding some quandaries which appeared later on in the field and of which Schneider established the unforgiving catalogue (Schneider 1984).

The initial studies dealt with what I will call « multifunctional kinship networks », i.e. networks which at the same time as they provide an accurate topological reduction of genealogies, provide also a template for the kinship taxonomy, for the economic, the political and the residential structures. The aboriginal Australian moiety, section and subsection systems constitute the prototypical instantiation of such multi-functionality (see a detailed examination of this multi-functionality in Verdon & Jorion 1981).

There is no historical evidence that the Indo-European societies which constitute the ancestry of the Western world ever displayed fully such multi-functionality of kinship networks. The often derided statement by Max Müller that the ancestors of the Westerners among us were never « Savages », needs to be interpreted within the framework where Savages were opposed to Barbarians: our ancestors were agriculturists and raiders, their social organisation was not that of the « horde » based on the clan but that of the cabin dwelling based on the *gens*.

Let me proceed systematically with this as kinship networks have historically shed their multi-functionality with the advent of modernity; this historical fact being a universal feature. The starting point of modelling as far as anthropological research is concerned lies in these societies where marriage is prescribed between moieties, sections or subsections which are exogamous to the extent that brothers and sisters belong to the same one and are forbidden to procreate. Spouses belong to distinct (sub-)sections (say A and B) and their children will belong to a third (sub-)section (say C) while female and male members of this third (sub-)section will marry in distinct (sub-)sections (say D and E). This being said, C, D and E may be strictly distinct from A and B, but do not have to: in a moiety system, C will be identical to either A or B; in a section system D may be identical to A or to B, etc.

Moieties (the word traditionally applies to 2 equivalence classes to marriage for men and 2 for women), sections (the word traditionally applies to 4 equivalence classes to marriage for men and 4 for women) or subsections (the word traditionally applies to 8 equivalence classes to marriage for men and 8 for women to 8-subsection systems) operate on the genealogical space a strict *partition*: each man, each woman, belongs to a (sub-)section and to one (sub-)section only, no (sub-)section is devoid of members. From now on I will refer to moieties, sections and sub-sections as « sections ».

Sections transcend the world of kinship in two ways: first – as I already suggested – because sections are multi-functional, kinship constituting also the template for the residential, economic and political orders, but secondly, because, the prescriptive alliance scheme is itself part of a broader cosmological order encompassing a global representation of the natural and the supernatural (see Jorion 1982a). Indeed sections divide in a similar manner the whole animal and botanical realms, constellations, winds and other meteorological phenomena, body fluids such as saliva, sperm, milk, vomit, mythological creatures from the dream world, etc. etc. (see von Brandenstein 1982).

For such societies, authors such as Lloyd Warner in the 1930s (Warner 1931) or Leslie Hiatt in the 1960s (Hiatt 1965) have produced compelling evidence that marriage prescriptions are not only strictly observed but strictly enforced, with spearing parties being convened to murder fugitive trespassers (see an example mentioned from Worsley in White & Jorion 1996: 296). This ensures that sections coincide with genealogies reduced as to equivalence classes in terms of kin-related marital behaviour: “kin-types”. Such coincidence provides that for every individual there is an identical injective mapping of kin types onto sections. In most of these prototypical cases the kinship terminology confirms this mapping by providing in an ego-centred manner one kinship term only for all individuals of the same sex allocated to the same section. Thus typically, an eight sub-section system corresponds to a sixteen term kinship terminology (usually further distinctions in terms of seniority apply to one’s own section).

Furthermore, such societies practising in most cases nomadic hunting and gathering, residential, economic and political groupings can be expressed unambiguously in terms of sections in the absence of any competing territory-related modes of grouping.

As the anthropological literature reveals abundantly, because of such redundancy in the various facets of the societal and cultural orders, the language of kinship networks has found in the Australian aboriginal context its abode of predilection within the anthropological literature. Indeed the redundancy of the same elementary pattern acting as a template for different types of societal and cultural behaviour allows prediction with a high degree of accuracy from one dimension to another.

It is such redundancy which has allowed anthropologists to deal similarly with Australian cases where there are no named sections, eliciting their latent existence either from evidence of a taxonomical nature, from prescribed marriage rules expressed in terms of kin categories or kin types, or from genealogies reduced as to identical kin-based marital behaviour. Following such line of inquiry, I – for instance – have (with the invaluable help of Edmund Leach) – managed to make sense of the 5 by 7 kinship terminology of the Northern Arnhem Land Murngin (Jorion 1993) from field material collected by Lloyd Warner (1931), Shapiro (1969) and Kupka (Kupka & Testart 1980).

Also, extending this time into other parts of the world such as New-Guinea, Indonesia, the New World, Africa, where cosmological categories such as sections are absent, similar inferences have been possible as to pseudo- or latent sections from taxonomical evidence, prescribed marriage rules expressed in terms of kin categories or kin types, or reduced genealogies. Following such line of inquiry, I – for instance – have (with the help of De Meur and Vuyk) managed to make sense of the apparently inconsistent marriage pattern of the Pende of Zaïre (Jorion, De Meur & Vuyk 1982).

Within the anthropological profession, such inferences of latent sections have encountered increased resistance with each degree of decreased multi-functionality of the kinship network template. Indeed each step down in terms of functionality requires an additional inference before the latent sections can be elicited.

Let us take for instance, as the first level of decreased multi-functionality those Australian cases where sections acting as cosmological categories are absent, i.e. only moieties belong to the actors' awareness. Is it legitimate to infer the existence of latent sections in such cases, all other circumstances being equal? In a conversation with Jack Goody, the Head of my Department at Cambridge, he would say « No, if they don't name them they don't know about them and therefore they do not exist ». Which needs to be understood as meaning that, not being part of the culture, there is no way they can guide behaviour.

Unless the aim of reconciling one day qualitative- and quantitative anthropology is brushed aside as inconsequential it is important that objections such as Goody's are properly addressed. Inferring the existence of un-named sections may seem to the qualitative anthropologist like reconstructing a culture according to mathematical lines, denying any relevance to the fact that a culture has given itself specific types of categories to order its universe and has ignored some others. Indeed if individuals are supposed to make decisions according to reasons they assign themselves, it is not the same to say that all male A should exclusively marry female B, and to say that all men have a choice to marry either a mother's brother's daughter, a father's sister's daughter, a mother's mother's brother's daughter's daughter or a father's father's sister's son's daughter. Even if this ends up being strictly equivalent in terms of actual behaviour.

The resistance springs from the principle that there is an essential and crucial difference between what people do purposely and what they do unconsciously. This the mathematical anthropologist can deal with by making a difference between behaviour which results from deliberate purpose and behaviour which can be characterised in terms of « it all happens as if ... » there were latent underlying sections. I guess that the main source of irritation stems from our statements betraying a particular creed: that we secretly accept that it does not make much of a difference whether or not people behave in a way because they follow a rule which they are able to enunciate if required (a principle which « guides » their behaviour) or that they do it all unwittingly. What our statements betray to the annoyance of the qualitative anthropologist is our focal interest for the algorithm. Whether or not the people who apply the algorithm are aware that they do so is for us some kind of *complementary* anecdotal information of relatively minor interest. In other words we are showing disdain for what the social or cultural actor experiences as its *freewill*.

On the other hand, what the attitude of the qualitative anthropologist reveals is an unsubstantiated belief in the impossibility for human societies and cultures to function in a way similar to that of other physical systems. This attitude finds its most accomplished form in a statement made by Frazer in *Folklore and the Old Testament*, as part of an onslaught on Rivers and his formally-minded pupils when he wrote that human societies are in no case organised in a way « like crystals »:

« Where the system is in full working order and has not fallen into obvious decay, the number of the exogamous classes is invariably two or a multiple of two, never an odd number. This suggests, what all the evidence tends to confirm, that these various groups have been produced by the deliberate and repeated bisection of a community, first into two, then into four, and finally into eight exogamous and intermarrying groups or classes; for no one as far as I know, has yet ventured to maintain that society is subject to a physical law, in virtue of which communities, like crystals, tend automatically and unconsciously to integrate or disintegrate, along rigid mathematical lines, into exactly symmetrical units » (Frazer 1918, vol. II : 231).

This is a misguided view based on unwarranted and a priori principles: it should remain unacceptable to the mathematical anthropologist. The fact is that societies do sometimes get organised in a way not dissimilar to crystals, and this, whether the members of these societies have any awareness of it or not. The argument has no more merit than to claim that it is illegitimate to write down the notes of a melody the natives are singing because these people have no notion of how to write music. A leading philosopher of science, Joseph Sneed one remarked, « After all why should the way mathematics is used to talk about people differ from the way it is used to talk about planets? » (Sneed 1979: xxiii).

The fact remains however that moving away from the prototypical Australian Aboriginal cases and moving down a path through the already mentioned cases of New-Guinea, Indonesia, the New World and Africa, the anthropologist treads a path where kinship networks become increasingly less explanatory of human behaviour. This is what I wish to explore now.

3. *The ancient empires*

As I mentioned earlier, remarking on Max Müller, as far back as we find evidence, there is nothing in the past of Western societies even remotely reminiscent of the Australian Aboriginal societal set-up. In this respect the endeavour of nineteenth-century anthropologists of accumulating evidence on « primitive » societies, as a way to complete the historical record of our own ancestors is at best inconclusive.

If anything, ancient Egypt provides us with evidence suggesting that kinship networks had even less of an impact in this early state than it has in our contemporary Western societies. Evidence gathered by Hopkins about brother / sister marriages shows convincingly that the incest prohibition which still prevails with us for this relationship, was made a mockery at least during the Ptolemaic times (Hopkins 1980). Duby suggests that such is the normal path of evolution in state-organised nations and claims that the

incest prohibition in our cultures came about only as a revival when the Church began to regulate marriage – imposing monogamy – in order to manage a logic of monastic orders (Duby 1981). It is possible to imagine that building an empire of the size of ancient Egypt – an extraordinary first in history – may have required the breakdown of loyalties accompanying kinship ties. Indeed to impose the view that is a citizen of the state, i.e. is submitted to specific regulations and is paying taxes of a particular amount – anyone being born between such and such territorial boundaries, it is indispensable to cut off the ties of affiliation which kinship provides. Kinship defines extra-territorial loyalties which transcend boundaries and survive migrations, it traces dotted lines on the social body along which a state may ever divide.

The other part of Africa, South of the Sahara desert, provides indirect evidence for the plausibility of the hypothesis – as local kingdoms never managed here to transcend the logic of what Sahlins called the « predatory » lineage (Sahlins 1961). The « colonising lineage » would be a more apt characterisation if the analogy from the animal realm were taken in earnest. Indeed in the context of the very low densities of human dwelling in sub-Saharan Africa, colonisation of wider territories remained for kinship groups an historically open option – that is until the other type of *colonisation* by European powers froze boundaries in the late nineteenth-century.

Even better than the indirect evidence offered by sub-Saharan Africa, China provides direct evidence on the difficulties of building a large empire grounded on the concept of a territory only, and transcending therefore the logic of kinship-based loyalties. China got organised within its current boundaries with the help of a bureaucracy using the Chinese character-based writing as the instrument of its administration. Schooling and recruitment of this bureaucracy through an effective examination system provided the principles underlying the administration of a large territory across the loyalties of the clan system. However the survival over the centuries and even millennia of the clan-based family delineated the lines of possible fracture which remained recurrent. Time and time again the overall territorial unity of China collapsed and reversed back to kinship-based and warlord-headed smaller kingdoms. Even nowadays in a neighbouring country, South Korean democratic elections repeat at each occasion the divides of the surviving clan system.

Whatever the actual explanations, which remain obfuscated by lost evidence, ancient Egypt and ancient China displayed ways of transmitting knowledge which have become independent from the kin-related context of the older worlds: writing and schooling made it possible to transmit knowledge across family-based loyalties. The kinship network ceased to mould the transmission of knowledge.

4. *The peasant world*

I did not mention information explicitly when dealing with the multi-functional kinship networks of Australia. To some extent in such cases, the network is pure over-determined information: cosmological information, information about marital prescriptions, descent, etc. Information about the creatures of the dream is transmitted through initiation which is typically performed by grandfathers which are usually « ontologically » identical to the novices due to the fact that they belong to the same

(sub-)section. Chris Gregory drew the attention to the fact that in some « gift » societies of New Guinea, the kinship network is the template of prices, through defining the – compulsory – gift rank index between kin types (Gregory 1982).

In a book called « the transmission of knowledge » (French original title, *La transmission des savoirs*), Geneviève Delbos and myself showed how empirical knowledge was transmitted within family- based communities of fishermen, traditional salt-producers and oyster-breeders in Brittany (Delbos & Jorion 1984). To our surprise, in such traditional environments and although sons and daughters would renew the same patterns of occupations generation after generation, there was actually hardly any knowledge properly transmitted.

What would happen would actually be the following. Whenever a child would reach the suitable age – and actually much earlier as we were led to notice – it would be introduced to becoming part of the production process. Contrary to expectation, it would not be put in a position of formal training by being shown and explained. It would instead be put at a location of strain in the productive process: where there is some actor currently missing, i.e. it is placed where it can fulfil a useful role. And instead of being told what to do, it would be asked to improvise intelligently.

This would occasionally lead to inadequate behaviour resulting in abundant scolding and very little as a matter of further explanation. That is, until the appropriate behaviour becomes a habit. The novice would therefore have to re-invent the technique for its own purpose, rather than being formally taught it. Until one day the novice is told that it knows all that needs to be known, although it would still feel inadequate. Such suddenly acquired awareness of having become an expert would come as a manner of blessing inducing exhilaration in the unexpectedly anointed novice.

Such mode of teaching would function effectively in a context where sons learn from their father and daughters from their mother. The acquisition of knowledge in this way coincides with the renewal of generations and the « You are an expert my son », would coincide with the « You are a man ».

The kinship network in cases like these can be read as carrying information flows but in a particular manner: the vertices are to be interpreted as discontinuities. It is through identification essentially that the same knowledge is reproduced generation after generation.

What we are dealing with here is peasant communities like we typically find in Europe or Asia. The economic unit here is household-based (Jorion 1982b; Delbos & Jorion 1984) and the mode of production as a whole is close to reaching the carrying capacity of the environment. This means that economic units can only expand through pushing away neighbours or absorbing them into the unit. The survival of the kin line is here subordinated to the survival of the economic unit as such. This entails that, as opposed to the colonising behaviour of the African family bending the territory to its demographic successes and failures, it is here the family and its demography which bend to the demands of too scarce a territory.

My first tutor at Cambridge in 1975 was Alan Macfarlane. I had just collected data then for fifteen months in a Breton fishing community. He had been working on

seventeenth century English parish records and diaries. From very similar facts we were drawing very different conclusions. In the ruthlessness with which the head of the family was shaping it through disinheritance, arranging marriage, adoption, etc. Macfarlane was reading the origin of individualism. What I was reading was very much the opposite – which I had seen at work in a traditional fishing community – that in certain economic circumstances people had to surrender to the iron law of the reproduction of the economic unit. This was hardly a new observation as it had already been noticed by LePlay (1870) and his pupils in their fastidious enquiries in France at the end of the nineteenth century. Chayanov (1925) in Russia studied the same phenomena and more recently then Bourdieu about the reproduction of the farm in Béarn as an economic unit (Bourdieu 1972, 1980).

I have just mentioned possible strategies for pruning and shaping kinship to the demands of the mode of production. What is probably an original contribution of the work I did with Delbos is our comparative study of the demography of two Breton villages, one fishing community, one community of traditional salt-producers, two villages distant by a mere ten miles. I have recently referred to this in a comment on Duran Bell's *Defining Marriage and Legitimacy* (Jorion 1997).

To our surprise the size and structure of the family turned out to be entirely different: in the first community, families were large and with a clear imbalance in favour of sons, in the second, families were small and with a clear imbalance in favour of daughters. The anomaly which caught our attention first was the sometimes countless string of girls in the salt-producing village where the average size of families remained however close to two children.

Parallel economic analysis had shown that in the fishing community, the optimal production unit was that of a boat with a crew of a father with three of his sons (Jorion 1982b). In the village of salt-producers, the complex labyrinth of salt ponds could not be divided and needed to be transmitted as a whole to a single son. We hesitated at testing the hypothesis that the demography reflected a birth control « strategy » of interrupting the reproductive cycle of the family as soon as such economic requisites were met. The closeness of the demographic facts to the probabilistic model turned out however to be so stunning that the hypothesis could not be discarded (Jorion 1984b: 84-89; Jorion & Delbos 1984: 79-80). It all happened « as if » reproduction stopped in Houat as soon as the family counted three sons and in Saint-Molf as soon as there was one. The strings of girls in Saint-Molf revealed simply the probabilistic mishaps of trying to secure one son. That the issue was not solved through infanticide was no doubt due to the strong Roman Catholic commitment in these villages. Uncomfortably, I would encounter however in the parish records of Houat some mentions of « enfant mort trouvé dans la lande », « baby found dead among the furze ».

The type of multi-functionality which in the section systems is that of the kinship network identified to the genealogy reduced as to homomorphisms belongs here to the economic unit: the farm or the boat. Consequently I tried for a while to reconstitute the genealogies of the boats of Houat. I immediately encountered obstacles. One is the lack of memory depth for crew composition: crew composition keeps shifting with the family cycle. There are records of boats as such as they are christened by the parish priest and

crews used to make weekly donations which would be mentioned at the Sunday mass under the name of the boat. The obstacle I encountered when I consulted the Navy records was synonymy: the range of boat names was so reduced as to lead to immediate confusion. For identification purposes boat names needed to be different at any one time within each maritime district but as soon as such common names as «Trois frères » (three brothers) or «Notre-Dame de la Garde » (a local cult to the Virgin Mary) would be freed it would be picked up again by someone. The reason why these Navy records have been inexistence for centuries is worth mentioning: a law passed by Colbert, War Minister of Louis XIV, intimated that French sailors could be drafted without notice; as a consequence sailors benefited from the first ever comprehensive social security system in France, dating thus from the seventeenth century.

Also, boats are economic units submitted to very specific constraints. I remember a remark from Edmund Leach, which unsettled me for a number of years. It was one of the first papers I gave as a graduate student at Cambridge University. When I was done Leach said «If I understand you well these people depend crucially on the migration of these fish shoals which cruise in the Bay of Biscay? ». I said, «Yes ». Then Leach mudedered me with the following words: «Then what you need is the genealogy of the fish ». And the truth of this would even sink further into me when in the early 1980s I started working with Anlo-Ewe migrant fisherman in West Africa. In this Southern part of the Atlantic, fish schools are fewer and bigger then they are in the North-Atlantic. This is the outcome of the fact that fish follow phyto-plankton blooms, which occur with a special *upwelling* of cold water through the warm coastal waters. What shapes people's lives here is the history of the *upwelling*. The way the Fante, Anlo and Xwla deal with such hardships is through highly mobile «companies ». These hire around a core of family linked specialists a number of unskilled young people. The contract is strongly reminiscent of European serfdom. Interestingly enough the contract is a very good deal for a person in his twenties, unfortunately because of its generous disposition towards debt accumulation it has also an in-built capacity of turning into indenture (Jorion 1988). The traditional salt production which Delbos had studied in Brittany is an example of even harsher dependency on climatic conditions: a string of five wet years would be enough to disrupt the industry for dozens of years to come by rendering it a too unreliable source of income.

5. *The urban environment*

The type of knowledge acquisition I have mentioned for peasant societies through identification to the father or mother is very much tied to the kind of reverence which a child would have for its parents in a traditional environment. In the 1970s the French government encouraged young people to take up again traditional salt-making. Salt-marshes constituted an ecologically unique milieu for wild life, providing fish nurseries and an efficient system of water management. The most effective way to keep the milieu in working condition was to resume salt-production. Interestingly the initiative nearly collapsed because of this type of transmission through identification. Indeed the mostly urban type of young people who volunteered in the scheme were not prepared to learn the traditional way, i.e. through trial-and-error canalised through verbal abuse. They wanted to be properly *taught* i.e. shown and explained « school-wise ».

I mentioned earlier the ancient Egyptian and Chinese systems, which used writing to initiate transmission of knowledge through schooling. The urban environment would signal the demise of the transmission of knowledge through family links. Indeed the likelihood decreased fast that successive generations would practice the same type of activity. Apprenticeship with strangers and learning through school would generalise. This would once and for all take away from the kinship network the function of transmission of knowledge. The trade corporation, ensuring inherited access to professions, would still characterise the early middle ages, allowing for a while the peasant way of transmitting knowledge to survive within the urban environment.

Conclusion

Lévi-Strauss always had an ambiguous attitude towards kinship algebra. He always welcomed suggestions from mathematicians about how to formalise the study of alliance and descent, or later transformations between myths: Weil's permutation models for the Murngin appear indeed as an appendix to a chapter in *The Elementary Structures of Kinship* (1949), and Guilbaud's reduced kinship network for the Aranda is shown as a figure in *The Savage Mind* (1962). However apart from the « canonical formula » of myth introduced in *Structural Anthropology* (1958) and only used once subsequently, Lévi-Strauss has never produced any properly mathematical modelling.

Lévi-Strauss wants his formalisations to literally emerge from the empirical facts with no prejudice whatever about what the formalisation would reveal. The last thing he would wish is to be influenced by any a priori imposition resulting from the shape of a mathematical object (Jorion 1985a). This feature of his view of theory would be striking in seminars, where he would apply the principle to guests' presentations. At question time he would go to the board and would start somewhat like this, « If I understood you well, we are dealing with a structure of this particular shape... ». Never would he utter phrases such as « permutation group », even if his modelling would in the end amount to entirely redefine what is actually a permutation group. Such exercises would always be brilliant, going way beyond what the speaker him- or herself would have noticed about the configurations present in his/her material. It would always however be « configurations » static models, never dynamics.

This being said, Lévi-Strauss takes the formalisations he has arrived at in real earnest. About these he applies a principle which I would characterise as « No fact can hurt a good model ». A few years later, in 1975, I would move on to other teachers: Leach, Fortes and Needham who hold the opposite principle: « No model can hurt a good fact » (Jorion 1985b). The three of them shared the same image of being people hostile to the network approach to kinship. Needham has written extensively on his opposition. However all three have been most encouraging of my own efforts in the field. In what way and how come?

From 1977 to 1979, Edmund Leach was my tutor while I was writing under his supervision a thesis on Malinowski. In the end he advised me not to submit it as we had uncovered too many facts which were sensitive to persons still alive. It is at that time that he showed me Langham's thesis, which became a book under the title, *The Building of British Social Anthropology, W.H.R. Rivers and his Cambridge Disciples in the*

Development of Kinship Studies, 1898-1931. The thesis was about Rivers and his students and was centred on the work that he, along with Armstrong, Barnard, Radcliffe-Brown, Deacon, Layard and Brenda Seligman had devoted to setting the basis of a science of kinship networks.

John Barnes was of course the person who was most knowledgeable about kinship networks in Cambridge at the time. He was the person who drew my attention to Armstrong's pseudo-permutation groups' catalogue of section systems published as an appendix to Armstrong's book on the *Rossel Island* gift system (Armstrong 1928). That John Barnes was encouraging was no surprise coming from a pioneer of social network studies. The interest of Leach was triggered by the help that Langham was requesting. Langham had spent some time in Britain to gather data, but he was now back in Australia and, in the process of rewriting his thesis into a book, needed additional material and checking up. Writing about Malinowski I was in an ideal position to help Langham fill the holes in his argument.

Leach was of course an authority on kinship, and more especially on the « circulating connubium » of MBD marriage but, because of the reproduction of the economic unit dynamics of both his Kachin and Pul Eliya field material, he had never paid much attention to multi-functional kinship networks such as exist in Australia. In addition he had become convinced through his field experience that all kinship was driven by the reproduction of the economic unit dynamics. In the introduction of the paper written in 1981 in collaboration with Leach and eventually published in 1993, I explain how our collaboration worked and why he never wished that I made this collaboration publicly known (Jorion 1993). Leach relished in attempts at formalising: he had got one of the very early PCs and generated with it the catalogue of all possible two-generator permutation groups up to order 32 which allowed MBD marriage. This we then sorted out in terms of plausibility of male/female generation length. Leach was aware of Warner's comment on a 5 x 7 structure: « The two main elements in Murngin kinship are the patrilineal lines and their lateral connections through the intermarriage of the five generations of the seven lines of descent » (Warner 1931: 172). Genealogies collected by Shapiro and Kupka allowed then to confirm the 7 / 5 ratio of generation lengths.

Leach's reluctance to have his name associated with this work was founded in his opposition to Lévi-Strauss' « no fact will hurt a good model » principle ». To Leach, anthropologists who were dealing with kinship networks could not help taking them too seriously: soon enough they would confuse the model for the actual thing and would defend the validity of the model against the reality of the facts. Leach told me a number of times to read Vaihinger's *Philosophy of "as if"* (Vaihinger 1924), which encompassed his epistemological vision. In Vaihinger, the model is an intellectual construct that lives in the head of a modeller only, it is no part of the world outside: it amounts to a scaffolding which can help the thinking process of understanding but should be discarded in the end. Things may look like they materialise the model but they do not: the model has been constructed for heuristic purposes only by a human mind as a schematic representation of the confused and confusing reality. There is here a strong and consistent anti-Platonist position which every epistemologically aware person needs to consider in earnest. The « scaffolding » view is expressed in White & Jorion 1996: « The kinship

structures which are mapped in this approach are not intended as any sort of complete representation of kinship “systems”, but merely as scaffoldings which help to bring into view kinship as a social field, providing a baseline for other mappings (which may be superimposed) of social processes such as communicative fields, exchange processes, transmission of learned behaviours, social rights and inheritance, political and religious succession, and the like » (White & Jorion 1996: 267).

In addition, Leach was very much concerned - like most anthropologists doing mathematical modelling at the time - that there was no critical mass of mathematically-minded anthropologists. In his review of Harrison C. White's *An Anatomy of Kinship*, Leach wrote, « The book is, in part, explicitly addressed to anthropologists, 99.9% of whom could not read it even if they wanted to » (Leach 1964: 156). Barnes wrote similarly « Hence we have the development of a technical language and a body of literature which, quite appropriately and inevitably, is incomprehensible to other social scientists, and often other anthropologists, as well as to the general public » (Barnes 1980: 297). I myself wrote comments of a similar nature, motivated essentially by the difficulty of getting these mathematical papers published. At the end of De Meur & Jorion 1980, I wrote « It should be recalled [...] that in some fields such a demonstration would be regarded as a formidable step but in anthropology not » (De Meur & Jorion 1980: 20).

Needham's support was decisive in my obtaining in 1982 a *Nuffield fellowship*, which allowed me to hire Elaine Lally for six months. Elaine was an Australian graduate student who responded to a note I posted in the Department of Mathematics at Cambridge. She had no notion of anthropology when she volunteered for the job but she went on to obtain a degree in anthropology as a consequence of our joint work. She wrote FORTRAN and Cobol versions of the algorithm for genealogy analysis, which became « P-graph » when Douglas White and I revived it jointly in 1990. « *An algorithm for the analysis of genealogies as to prior kin connection between spouses* » never found its way into print. The reviewers of *Science* claimed the paper had no originality, the reviewers of *Man* regarded as a matter-of-fact view that the journal would never publish anything as exotic as an « algorithm ». Elaine and I made a presentation of the algorithm at the Department of Statistics and Applied Mathematics at Cambridge in 1983. From there Elaine moved to a fruitful collaboration with Gisèle De Meur who had co-authored with me four papers on kinship networks. Gisèle had approached me when we were both junior professors at the Free University of Brussels in 1977. Gisèle revived my interest in Harrison White's line of approach to kinship as presented in *An Anatomy of Kinship* (1963) about which I had made a presentation in an undergraduate workshop in 1965. We used P-graph type representations consistently, starting from our 1980 Murngin paper (Jorion & De Meur 1980).

Needham had shown himself highly critical – if not vociferous – about the kinship algebraic approach in a number of papers, more particularly in his Introduction to *Rethinking Kinship and Marriage* (1971) and in the paper he had written in collaboration with Korn (Korn & Needham 1970). The reason for his encouragement was of the kind that only a great mind can afford: encouraging a line of argument for which he has no sympathy, for the sake only of the scientific venture. However Needham always made it

clear to me that the main reason for his friendly behaviour towards me and my work originated in his regard for the quality of my fieldwork, rather than my theorising; indeed when my book on the Breton fishermen of Houat was published he encouraged one of his pupils, Malcolm Chapman, to write a glowing review of it for the *Times Literary Supplement*. The day I spent with Rodney Needham at All Souls College before presenting a paper in the late afternoon at the Institute of Anthropology of Oxford is one of those days which remain as a landmark in one's life.

Fortes' attitude finds its rationale in his short book *Oedipus and Job in West African Religion* (1959). What was common to my approach and Fortes' to kinship was the striving for *explanation* and the sympathy for psychoanalytical explanation of kinship-related behaviour. I have mentioned earlier one of the underlying assumptions of the kinship networks line of approach: the relative indifference towards whether or not an actor is aware that it behaves in a way revealing *structure* when this behaviour is scrutinised in a *collective* perspective. This is also proper to psychoanalysis, which envisages any type of behaviour as having an *unconscious* motive at heart. Meyer Fortes invited me in December 1983 to share his lunch in a private room at King's College and made it clear to me that – despite any feelings I may have had to the contrary – the formal approach to kinship he had seen me developing – when informed by the psychoanalytical concern for the unconscious motive – was according to him the way of the future. A couple of weeks later Meyer fell into a coma, he died in February 1984.

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