

Émergence et évolution de la parenté

sous la direction de
JEAN LASSÈGUE

*Actes
de la
recherche
à l'Ens*

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Présentation

JEAN LASSÈGUE*

Les journées scientifiques qui se sont déroulées à la station biologique de Foljuif en novembre 2002 ont permis de faire se rencontrer des anthropologues et des linguistes, des éthologues, des philosophes et des modélisateurs¹ appartenant au groupe de recherche « Modélisation de l'émergence du langage » (MEL)².

Très concrètement, tout d'abord, l'objectif de ces journées était de réunir des chercheurs britanniques et français, appartenant à des disciplines variées (anthropologie, linguistique, modélisation en sciences humaines et sociales, philosophie), pour permettre des échanges transdisciplinaires et transnationaux. Il s'agissait, d'une part, de mieux comprendre la place revenant au langage dans l'ensemble des activités symboliques et, d'autre part, de mieux circonscrire la pertinence de la modélisation dans l'abord de l'émergence du langage. Ces deux questions, traitées de façon connexe depuis la formation du groupe MEL, méritaient, de par leur importance et leur difficulté, une étude particulière, d'où l'idée d'y consacrer des journées spéciales.

Je présente ici les différentes interrogations qui furent les miennes en proposant au groupe MEL la tenue de cette réunion. Ni les membres du groupe ni nos invités n'étaient évidemment tenus d'y répondre explicitement, et chacun a pu se situer librement par rapport à celles-ci, sans avoir à y couler de force sa pensée.

I. L'activité de langage resituée dans les activités symboliques

Par « activité symbolique » en général, on entendait, au moins à titre provisoire, tout foyer d'activité rendant possible l'organisation de conduites collectives et permettant leur anticipation sous la forme de transaction sur des valeurs.

Le langage faisait assurément partie de cet ensemble d'activités – une certaine tradition linguistique, de Humboldt à Bühler et Cassirer, en témoigne –, même si l'on semblait perdre, au premier abord, les façons plus fines de le caractériser, à partir des problématiques structurale, transformationnelle ou de la linguistique cognitive. Il semblait cependant plus efficace de replonger l'activité de langage dans le champ anthropologique plus vaste des activités symboliques si l'on voulait avoir une meilleure idée de son émergence. Pourtant, ne compliquait-on pas alors inutilement le problème, puisqu'il ne s'agissait plus seulement de progresser dans la question de l'émergence du langage, mais aussi dans celle de l'émergence des activités symboliques en général ? Pour bien saisir la nécessité de ce qui apparaissait au premier abord comme un détour, il fallait revenir à l'état de la question et prendre simultanément en compte quatre ordres de faits.

I. 1. État actuel de la question de l'apparition et de la généralisation des activités symboliques

Premièrement, *l'apparition des activités symboliques et celle des humains modernes ne coïncident pas*. Même si l'on néglige le fait, assez bien avéré aujourd'hui, de l'existence de traits culturels chez les grands singes³ et d'autres

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animaux, c'est sur une période d'au moins 250 000 ans – et donc *bien avant* l'apparition des humains modernes, datée d'environ 140 000 ans – que s'est progressivement mis en place un certain nombre d'activités symboliques⁴, en particulier trois d'entre elles : l'usage de pigment d'ocre rouge, le façonnage de pointes de flèches et les marques sur les parois rocheuses⁵.

Deuxièmement, en l'absence de comparaison possible entre humains modernes et autres espèces d'humains, toutes disparues aujourd'hui, on remarque l'existence d'un palier entre les primates et les humains modernes quant à l'usage du langage : *le langage est bien, aujourd'hui, spécifique à l'humain moderne*⁶. C'est donc dans l'immense intervalle excluant les primates en amont et incluant les humains modernes en aval (c'est-à-dire entre 2,5 millions d'années et 190 000 ans), intervalle composé de branches variées d'humains au sein du genre *Homo*, que le langage s'est développé en tant qu'activité symbolique, au sein d'autres activités symboliques.

Troisièmement, dans cet intervalle, l'émergence du langage *n'est pas corrélée à la croissance du cerveau mais à celle de la culture matérielle*. La phase de croissance du cerveau la plus rapide a eu lieu entre 2,5 et 1,5 million d'années en Afrique et semble avoir contribué à séparer le genre *Homo* des autres primates bipèdes, sans que la culture matérielle d'*Homo erectus*, une fois qu'il fut doté d'un gros cerveau (– 1,5 million d'années), évolue ensuite de façon radicale. En revanche, on assiste à un déclin relatif du volume du cerveau concomitant d'une accélération du rythme du changement dans la culture matérielle après la dernière grande glaciation (– 170 000 ans). Ce sont donc plutôt des modifications *d'ordre comportemental ou culturel*, une fois un certain volume du cerveau atteint, qui peuvent rendre compte de l'émergence du langage, à une phase sans doute relativement tardive de l'évolution du genre *Homo* (*Homo helmei* et *Homo sapiens*)⁷.

Quatrièmement, non seulement le langage est aujourd'hui spécifique à l'humain moderne mais il occupe *une place majeure* au sein de ses activités symboliques. Il est en effet difficile de concevoir une activité symbolique quelconque propre à l'humain moderne qui ne fasse pas appel à un ensemble de signes narrativement structurés, dont le prototype est le système organisé du langage. Le langage *a donc fini par jouer*, sans doute relativement tardivement, un rôle central au cœur des activités symboliques.

Il semblait raisonnable d'en conclure que la place occupée aujourd'hui par le langage dans les activités symboliques de l'humain moderne *résulte d'une reconfiguration des rapports entre activités symboliques déjà existantes liée à une évolution comportementale ou culturelle, lors d'une phase relativement tardive de l'évolution du genre Homo*.

On pouvait ainsi supposer que la reconfiguration des activités symboliques avait consisté en un va-et-vient entre une certaine configuration corporelle (non limitée au seul cerveau) à peu près stabilisée dans l'espèce et des régulations de conduites au moyen d'un usage collectivement partagé de marques⁸, à la fois externes aux individus et internes au groupe d'individus dans lesquels elles étaient adoptées⁹. C'est pour essayer de mieux comprendre la nature de cette reconfiguration – que celle-ci ait eu lieu progressivement ou subitement restant un point à éclaircir – et des changements comportementaux ou culturels qui lui sont corrélés, qu'il paraissait nécessaire d'envisager la question de l'émergence du langage dans son rapport aux autres activités symboliques.

I. 2. L'activité symbolique comme forme générale

La reconfiguration des rapports entre activités symboliques ayant fini par placer l'activité de langage au centre de celles-ci se présentait, par sa nature même, sous une forme circulaire : d'une part, des activités symboliques non directement linguistiques semblent faire naturellement partie de l'environnement sémiotique servant de condition d'émergence au langage proprement dit ; d'autre part, seul le langage, conçu comme « interprétant général¹⁰ », permet de rendre compte des aspects sémiotiques propres à ces activités symboliques, qu'elles se soient matérialisées sous des formes techniques ou esthético-religieuses¹¹. Un moyen de sortir de ce qui apparaissait comme un cercle vicieux consistait à essayer de concevoir les activités symboliques, y compris le langage, comme susceptibles de « déborder » les unes sur les autres¹² : ce serait en fait *la même activité symbolique qui se manifesterait de façon différenciée*, selon le contexte d'activité dans lequel elle s'exprime et dont le langage aurait fini par occuper le cœur.

Le problème revenait alors à se poser la question suivante : comment mettre au jour une morphologie générale qui puisse rendre compte de cet « air de famille » entre activités symboliques tout en faisant cependant place à la possibilité de leur différenciation ? C'est ce « bougé » originaire dans les activités symboliques, à la fois dans leur diversité native et dans l'évolution propre à chacune, dont il fallait mieux comprendre le mécanisme si l'on voulait approcher le problème de l'émergence du langage au sein des activités symboliques¹³. On devait donc se donner les moyens d'une réflexion portant sur les activités symboliques en général, leur diversification, le régime de leur corégulation et les médiations intervenant dans leurs rapports mutuels.

I. 3. La fonction symbolique dans le cadre structuraliste et post-structuraliste

C'est très naturellement vers l'anthropologie que le groupe MEL s'était alors tourné pour trouver des éléments de réponse à la question des rapports entre le langage et les autres activités symboliques ; c'est en effet cette discipline qui, depuis plusieurs décennies déjà, avait tenté, par la voie structuraliste, de mettre au jour, sous l'appellation de « pensée symbolique », un régime de sens qui soit commun aux diverses activités symboliques¹⁴. Il fallait donc revenir sur la façon dont le structuralisme avait caractérisé la notion de pensée symbolique et tenter de voir en particulier si la notion de système d'éléments, régis par des relations syntagmatiques et paradigmatiques¹⁵, qui en constituait le moteur, pouvait être retenue telle quelle.

C'est précisément en se fondant sur cette notion de système d'éléments qu'une analogie avait été proposée entre le mode de fonctionnement du langage et le mode de fonctionnement d'une activité symbolique capitale en anthropologie, celle de parenté : l'école structuraliste en anthropologie – et au premier chef, Lévi-Strauss – avait en effet tenté d'établir que l'échange des femmes était moins un échange de biens ayant une valeur dérivant d'une nature propre qu'une instanciation particulière de l'échange des signes n'acquérant de valeur que par opposition à d'autres signes¹⁶. Il fallait donc se demander si, aujourd'hui, avait encore le même degré de pertinence, pour penser la régulation réciproque des activités symboliques, le régime systémique de l'échange hérité du structuralisme qui continuait à perdurer sous différentes guises dans de nombreux cercles, en particulier cognitivistes, dans lesquels la notion de système avait fini par se durcir en théorie de l'« esprit », en abandonnant l'idée d'un régime ouvert et non exclusivement mentalisé de corégulation entre activités symboliques au profit d'un

système de modules mentaux de traitement de l'activité symbolique dont on supposait qu'ils étaient génétiquement prédéterminés¹⁷. Bref, la notion de système, organisée intérieurement selon les deux axes de simultanités et de successivités, pouvait-elle encore être considérée comme le mode général et unique de constitution de la fonction symbolique, comme en témoignait, jusqu'à aujourd'hui, l'analogie entre langage et parenté ?

Or la réponse à cette question *paraissait aujourd'hui négative*, tant du point de vue de la linguistique que de celui de l'anthropologie¹⁸, et l'analogie forte entre leurs régimes de sens, sous l'égide de la notion structurale de système, n'était plus, avec le recul, recevable dans les mêmes termes¹⁹. Retravailler l'analogie entre langage et parenté exigeait en particulier de repenser à nouveaux frais la nature et la portée de la notion de système telle qu'elle avait été abordée par le structuralisme, en particulier celui de Lévi-Strauss. Deux remarques pouvaient être faites à cet égard.

Premièrement, ce qui légitimait aux yeux de Lévi-Strauss l'analogie entre la linguistique et l'anthropologie de la parenté²⁰, c'est que les deux disciplines se *recouvraient partiellement* parce que la seconde possédait une partie linguistique : les relations de parenté étaient en effet non seulement des objets sociaux mais aussi des termes du lexique (père, frère, oncle, etc.). C'était donc à *partir de ce qui était linguistique en anthropologie* que l'on pouvait espérer retrouver la structure, non directement linguistique, des relations de parenté. Le raisonnement analogique de Lévi-Strauss revenait donc à ceci : de même qu'en linguistique le niveau du mot avait été dépassé pour atteindre le niveau du phonème, de même, en anthropologie de la parenté, le niveau des appellations (la nomenclature des termes) devait être dépassé pour atteindre le niveau des attitudes (l'atome de parenté). Mais alors qu'il n'était pas possible de passer des appellations aux attitudes parce que l'on ne pouvait pas reconstituer les secondes à partir des premières, en revanche, il semblait possible à Lévi-Strauss de reconstituer, à partir des attitudes, les appellations, car on se retrouvait alors comme dans le cas linguistique où l'on connaissait la fonction (dans le cas du langage : la fonction était la communication ; dans le cas des attitudes, la fonction visait à assurer la cohésion sociale) sans connaître encore le système (dans le cas du langage : les phonèmes ; dans le cas des attitudes : les atomes de parenté).

Cependant, l'analogie entre anthropologie de la parenté et linguistique n'était pas complète puisque, dans le cas linguistique, il y avait continuité entre les deux niveaux, celui du lexique et celui des phonèmes qui appartenaient tous les deux au domaine du langage alors que cette continuité n'était plus *a priori* assurée dans le cas anthropologique, puisqu'il fallait prêter une cohérence systématique aux attitudes²¹ en ayant seulement à notre disposition une cohérence linguistique, le *système* des appellations. Le rapport entre attitude et appellation se faisait donc sur la supposition que les deux termes étaient systématiques *au même titre* et que c'était précisément le systématique en eux qui rendait possible leur mise en rapport.

Deuxièmement, cette interprétation de la notion de système sous-entendait à l'origine que toute activité symbolique, du fait de son organisation en unité cohérente particulière – celui de la langue, de la parenté, du mythe –, *recelait en elle-même la totalité de son potentiel d'action*, concevable en termes de substituabilité d'éléments considérés comme équivalents²². Ultérieurement, ce point de vue avait fini par se couler dans un moule cognitiviste dans lequel on

pensait possible une modélisation de la pensée symbolique en termes de théorie de l'information, au risque de mentaliser entièrement le symbolique et le culturel²³. Mais ce type d'approche ne s'intégrait pas facilement à la problématique de l'émergence, dans la mesure où il ne permettait pas de comprendre l'apparition historique de ce régime de la substituabilité généralisée, ni son maintien au cours du temps, toujours présupposé. Il n'y avait plus en particulier de place, sauf sous le régime quasi algébrique en question, pour une *co-organisation différenciée des activités symboliques entre elles* : chacune d'entre elles, considérée comme autonome en elle-même n'entretenait plus, pour réussir à perdurer, de relation avec les autres²⁴, – sauf à envisager finalement ces rapports mutuels comme n'ayant pas de réalité ailleurs que dans l'esprit d'algébriste de celui qui les pensait. Il fallait donc se retenir de faire de la substituabilité entre éléments équivalents le seul régime interne des activités symboliques, pour envisager la construction de cette substituabilité, soit comme un régime parmi d'autres, soit même comme ayant une importance variable selon les activités symboliques et leur empiètement différencié²⁵.

Il paraissait nécessaire, pour ce faire, de revenir sur les *ritualisations* que les activités symboliques mettaient en place en vue de construire leur propre champ d'activité, toute activité symbolique possédant des phases ritualisées de stylisation se signalant aux acteurs comme norme, au sein d'un continuum de stylisations possibles allant de la simple esquisse de gestes à faire et de paroles à prononcer à la rigidité la plus extrême. Une telle analyse permettait d'insister d'une part sur l'aspect non toujours directement substituable des éléments d'un système entre eux²⁶, d'autre part sur les rôles distribués par ces ritualisations, qui n'étaient jamais interchangeables d'emblée et qui requéraient en particulier des initiations à chaque étape de la vie.

C'était donc la question de la substituabilité ou de la non-substituabilité entre éléments d'un système qui faisait le fond du problème de l'analogie entre différentes activités symboliques et dont il fallait débattre au cours de ces journées. C'est pourquoi il était souhaitable de réunir linguistes sémanticiens et anthropologues de la parenté pour soumettre à examen la question des rapports que pouvaient entretenir leurs disciplines quant à cet objet supposé commun, la notion de « fonction symbolique ».

II. Pertinence de la modélisation pour la question de l'émergence du langage

Un autre objectif de ces journées était d'évaluer la pertinence de la modélisation dans les recherches sur l'émergence du langage. Deux raisons pouvaient être invoquées à ce propos.

D'une part, une raison épistémologique : parmi les modèles possibles, c'était surtout la place à accorder aux modèles néodarwiniens qui faisait problème, dans la mesure où ceux-ci étaient particulièrement bien représentés dans les recherches sur l'émergence du langage. La présence, lors des journées, d'anthropologues français et britanniques ayant répondu à l'invitation du groupe MEL et qui étaient tous spécialistes à des titres divers des questions de parenté – tout en appartenant à des écoles de pensée foncièrement distinctes – avait ainsi d'abord un but informatif : il s'agissait de prendre connaissance des débats actuels sur la question controversée de la pertinence des modèles néodarwiniens dans un contexte anthropologique différent de celui auquel le groupe MEL était

habitué, où les modèles en question étaient présentés dans le cadre quelque peu latéral de recherches linguistiques sur l'émergence du langage.

D'autre part, une raison théorique : la théorie anthropologique de la parenté entretient en effet avec les modèles néodarwiniens un rapport de proximité, précisément dans la mesure où ce sont deux perspectives foncièrement différentes ayant cependant pour but commun d'articuler reproduction biologique et reproduction sociale²⁷. Le concept de parenté paraissait ainsi un bon candidat pour analyser les points de rupture ou de convergence entre le cadre structuraliste des uns et le cadre néodarwinien des autres, qui ne recoupait d'ailleurs pas les différences nationales.

Il y avait donc deux raisons valables de souhaiter confronter les points de vue sur la question de la parenté.

II. 1. Modèles néodarwiniens, modèles cognitivistes et fonction symbolique

Plus précisément, ce qui posait problème était l'acclimatation récente du cadre théorique néodarwinien à une théorie cognitiviste de la fonction symbolique, lointaine héritière de la tradition structuraliste. Cette acclimatation prenait la forme suivante : le génome apparaissait comme le lieu unique des variations possibles, contingent par rapport à la vie individuelle, où se trouvait programmé (et décrit dans un langage théorique fondé sur les notions d'information, de calcul et de module) le plan de l'organisme dans ses aspects morphologiques, comportementaux mais aussi cognitifs. Ces différents aspects, organisés selon un principe de moindre coût ayant pour mesure objective l'avantage reproductif, étaient censés opérer une synthèse des niveaux biologiques et sociaux au moyen d'un dispositif représentationnel ancré dans les gènes des individus²⁸.

Cette approche représentationaliste avait pour conséquence d'opposer radicalement deux modes de fonctionnement de la fonction symbolique : le premier reposait sur une conception étroite du symbole conçu comme marque arbitraire et concernait essentiellement l'aptitude à la *référence* à l'objet, conçu comme existant indépendamment du langage ; le second reposait au contraire sur une conception élargie du symbole, doté d'une propension non contrôlée au *symbolisme*, susceptible de produire des objets ayant une existence sans contrepartie objective, dont il fallait réussir à expliquer le caractère illusoire par un mécanisme cognitif approprié. Du point de vue de l'émergence de la fonction symbolique, celle-ci apparaissait alors comme ayant émergé deux fois, une première fois pour rendre compte de l'usage du symbole au sens étroit et une seconde pour expliquer son usage au sens large²⁹.

Il n'était certes pas impossible d'émettre cette hypothèse même s'il était très difficile de la vérifier à partir des faits connus³⁰ mais elle avait un inconvénient majeur immédiat en ce qu'elle mettait définitivement à mal l'idée d'une fonction symbolique pensée à partir de la notion d'*activités progressivement coritualisées*, au profit d'une théorie *mentaliste* et *anhistorique* de cette fonction. Or la notion d'activité rendait au contraire possible des rétroactions entre les sujets, leurs productions matérielles ou verbales et leur environnement écologique qui autorisaient une conception toute différente de la fonction symbolique et de son apparition au cours de l'histoire du genre *Homo*.

En fait, il paraissait souhaitable, à partir de la critique du volet représentationaliste des modèles néodarwiniens ayant la fonction symbolique pour objet, de revenir, assez curieusement... à Darwin. La référence à Darwin, dans l'usage qui en était fait dans les modèles néodarwiniens standards, paraissait en

effet quelque peu biaisée, dans la mesure où elle se limitait le plus souvent au mécanisme de la sélection naturelle et de sa conséquence directe, l'avantage reproductif, en négligeant toutes les recherches ultérieures de Darwin qui avait fait appel à la sélection de groupe pour rendre compte de l'émergence de la civilisation et plus particulièrement de la solidarité à l'égard du non apparenté biologiquement³¹. À cela, une raison au moins : les modèles néodarwiniens de l'émergence du langage ne reconnaissaient pas la pertinence de la notion de sélection de groupe parce qu'elle ne s'intégrait pas, ou mal, au cadre néodarwinien³².

Il fallait donc voir aussi dans quelle mesure les débats sur la nature et la fonction de la parenté n'exigeaient pas du même coup que soit reprise la question des liens possibles entre fonction symbolique et sélection de groupe.

II. 2. Sélection de groupe, sélection sexuelle et fonction symbolique

Dans le cadre de la sélection naturelle, une caractéristique corporelle ou comportementale se trouve sélectionnée si et seulement si elle offre un avantage à la diffusion des gènes de son porteur individuel. Il y aurait sélection de groupe si une caractéristique était sélectionnée parce qu'elle bénéficie au *groupe entier* auquel l'individu appartient, quel que soit l'avantage ou le désavantage que cet individu en retire. Généralement, la sélection naturelle (individuelle) empêche tout effet de sélection de groupe, sauf à considérer des groupes particuliers d'individus, assimilables à des colonies de clones³³. Il existe cependant des situations particulières où ce n'est pas le cas³⁴ et ces situations ont ceci d'intéressant qu'elles semblent attestées d'un point de vue biologique tout en ayant également une certaine plausibilité du point de vue social humain³⁵. En effet, pour perdurer, les situations dans lesquelles le groupe a temporairement un rôle sélectif rendent obligatoires, du point de vue externe au groupe, son isolement, sa séparation en sous-groupes puis la fusion de ces sous-groupes et, du point de vue interne au groupe, la distribution d'un certain nombre de rôles par nature environnementaux et mobiles – mais fixée par des règles locales –, double série de mouvements dont on peut se demander *s'ils ne participent pas, dans l'espèce humaine*³⁶, *à ce qui rend précisément possible les activités symboliques, en particulier les règles de parenté*. L'espèce humaine aurait alors ceci de particulier qu'elle parviendrait, au moyen de ritualisations spécifiques, *à faire durer un état où le groupe joue un rôle sélectif*, état généralement si transitoire parmi les espèces qu'il n'a pas ou peu d'efficacité biologique. Quelles seraient ces ritualisations ?

On devait tout d'abord remarquer que la question de la sélection de groupe était liée à celle de la sexuation en général³⁷. Même G. C. Williams, grand promoteur de la critique néodarwinienne de la sélection de groupe dans les années 1960 reconnaissait trente ans plus tard (en 1996) que la sexuation semblait bien un effet de sélection de groupe³⁸. On pouvait alors légitimement se demander si, une fois mis en place un régime, très général à travers les espèces, de sexuation produit par sélection de groupe, la sélection sexuelle – dont la force évolutive présente dans de très nombreuses espèces animales était fondée sur des systèmes de reconnaissance réciproque des partenaires sexuels au moyen de marques corporelles différentes selon les sexes et leur période de fécondité – n'avait pas fini par jouer un rôle particulier dans le cas humain³⁹ : les humains auraient fini par articuler spécifiquement sélection sexuelle et sélection de groupe en faisant émerger la notion de *groupe différencié selon les sexes* (et non pas

seulement d'individu de sexe opposé) par le biais de marques de reconnaissance particulières à chaque sexe susceptibles d'être collectivement interprétées, à la fois pour chaque sexe et en relation au sexe opposé⁴⁰. Comment de telles marques avaient-elles pu émerger ? C'est ici que les modèles exposés au cours de ces journées allaient se révéler décisifs.

On savait que l'anthropologie sociale avait beaucoup insisté – de R. Fox à F. Héritier – sur le fait que les règles de parenté dans l'espèce humaine étaient universellement régies par une domination masculine appelée par F. Héritier « la valence différentielle des sexes, qui aboutit à déposséder le féminin des capacités potentielles dues à son privilège exorbitant d'enfanter les deux sexes⁴¹ ». Cette valence différentielle avait un statut *sémiotique et sémantique* dans l'espèce humaine et c'était en cela qu'elle jouait d'une part un rôle dans l'émergence du langage tout en se distinguant d'autre part de ce qui se produisait chez les primates où les femelles avaient la même charge et le même privilège quant aux petits et étaient, elles aussi, soumises à la domination (physique) masculine sans que cela jouât un rôle quelconque dans l'émergence de signes linguistiques pour ces espèces. Il fallait donc se demander comment cette valence pouvait devenir *différentielle*.

Une conjecture de L. Scubla⁴² sur le statut anthropologique du sang pouvait ici servir d'exemple, sans préjuger de sa valeur universelle⁴³ : pour lui, le sang artificiellement versé par les hommes lors du sacrifice était à mettre en opposition *structurale* avec le sang naturellement versé par les femmes lors de leurs règles. Si l'on accordait que le sang pût jouer le rôle de marque, il y aurait eu, autour de celui-ci, une activité symbolique qui pouvait rendre compte de l'émergence d'une *reconnaissance* (sémiotique) de marques (le sang des femmes) doublée de leur *compréhension collective* (sémantique) sous forme de signes (le sang produit par les hommes qui se tient à la place du sang des femmes) : ces marques-signes auraient été *différentiels* par leur opposition structurale *sans être immédiatement substituables*, les hommes et les femmes ne produisant pas ces marques-signes de la même manière⁴⁴. L'articulation particulière que l'espèce humaine aurait opérée entre la sélection de groupe et la sélection sexuelle et qui pouvait avoir joué un rôle dans l'émergence du langage aurait donc tout d'abord consisté en la mise en place de rôles collectifs de type dualiste⁴⁵, celui des hommes ayant été de contrôler sémantiquement l'aspect *groupal* de ces rôles, tandis que celui des femmes aurait été de contrôler sémiotiquement leur aspect *sexuel*.

La conséquence majeure aurait alors été que la sélection sexuelle et l'environnement sémiotique et sémantique différencié selon les sexes qu'elle avait aménagé aurait permis de faire durer l'état habituellement transitoire pendant lequel la sélection de groupe était susceptible de jouer un rôle⁴⁶. On retrouvait donc la question de l'émergence de règles de parenté au cœur du problème de l'émergence du langage. Il y avait donc une certaine logique à chercher dans les co-articulations des activités symboliques entre elles l'émergence d'une forme générale spécifique à l'espèce humaine, dont le langage occuperait le centre.

Ces questions étaient évidemment trop vastes pour y répondre pendant le temps qui nous était imparti. Il m'avait semblé néanmoins utile de commencer par situer le débat et de proposer aux participants ces quelques pistes pour voir s'il était possible de les suivre ou, s'il valait mieux, au contraire, les abandonner.

Lors de ces journées, Isabelle Daillant puis Laurent Barry se sont chargés de l'introduction générale portant sur la théorie de la parenté du point de vue classique de l'anthropologie sociale, préalable indispensable pour que tous les participants travaillent ensemble à partir d'un vocabulaire minimal commun. Ensuite ont pris place trois exposés dont ce volume a gardé la trace : Camilla Power fait tout d'abord le point sur le dimorphisme sexuel et les différentes stratégies de coopération entre mâles et femelles au sein du genre *Homo* ; puis Chris Knight revient sur les débats concernant la place plus ou moins centrale que, depuis le XIX^e siècle, on fait occuper à l'avunculat selon que l'on se situe dans un régime de parenté patri- ou matrilineaire ; Nick Allen, enfin, expose ce qui lui semble être les conditions minimales pour qu'une activité symbolique de l'ordre de la parenté puisse émerger.

Ces trois exposés s'appuient sur des modèles très différents, proches de la perspective néodarwinienne pour ceux de C. Power et de C. Knight tandis que celui de N. Allen relève plus de la perspective structurale. Leur teneur n'est pas complètement étrangère pour qui connaît la littérature anthropologique francophone puisque les modèles dont leurs auteurs se réclament ont été examinés par C. Lévi-Strauss⁴⁷ ou, plus récemment et plus complètement, par M. Godelier⁴⁸. Ils ont suscité de riches discussions sur le statut des modèles, en particulier après l'exposé de David Chavalarias sur les modèles d'émergence de la coopération ainsi que sur la pertinence du cadre évolutionniste standard. Ils ont le mérite, quelles que soient les réticences manifestées lors de ces journées par certains des participants à l'égard des modèles néodarwiniens jugés trop simplistes, de reprendre à nouveau frais, et selon leur modalité propre, la question des rapports entre anthropologie de la parenté et sémantique linguistique qui faisait le fond du problème posé.

Au lecteur maintenant de se faire une idée des avancées que ces exposés ont rendu possibles.

Notes de JEAN LASSÈGUE

¹ Cinq anthropologues ont participé à ces journées : David Allen (université d'Oxford), Laurent Barry (EHESS), Isabelle Daillant (université Paris 10), Chris Knight (université d'East London) et Camilla Power (université d'East London). Les membres présents du groupe MEL étaient Pierre Cadiot (université Paris 8), David Chavalarias (École polytechnique), Jean-Louis Dessalles (ENST), Romain Laroche (ENST), Jean Lassègue (CNRS), Jean-Pierre Nadal (CNRS) et Yves-Marie Visetti (CNRS).

² Action concertée du CNRS « Origine de l'homme, du langage et des langues ».

³ Pour cette question encore controversée, voir J. Joulain, « "Le casse-noix" du chimpanzé : lecture anthropologique d'un objet simien », in F. Joulain et A.-J. Ducros (éd.), *La Culture est-elle naturelle ? Histoire, épistémologie et applications récentes du concept de culture*, Paris, Errance, 1998, p. 115-137.

⁴ Certains archéologues prêtent ainsi une activité artistique à d'autres représentants du genre *Homo*, *Homo heidelbergensis* et peut-être même *Homo erectus*. Voir R. G. Bednarik, « A figurine from the African Acheulian », *Current Anthropology*, 44, 2003, p. 405-438.

⁵ La chronologie s'établit à l'heure actuelle de la manière suivante : avant 300 000 ans, on trouve des lames, des pierres polies, des activités d'extraction et d'utilisation de pigment d'ocre rouge ; des pointes de flèche en pierre ; à partir de 140 000 ans, on trouve la trace de transports (obsidienne, outils) sur de longues distances, allant jusqu'à 300 km ; la pêche, des outils en os, des pointes ébarbées, une activité d'extraction de minerai, des pièces incisées et des pendentifs ; à partir de 80 000 ans, des microlithes, des perles et des images. Voir S. McBrearty et A. S. Brooks, « The revolution that wasn't : a new interpretation of the origin of modern behavior », *Journal of Human Evolution*, 39, 2000, p. 453-563. Ils décrivent quatorze indices de capacités cognitives dont la moitié était déjà présente il y a 140 000 ans. L'article a donné lieu à controverse mais des indices multiples l'ont corroboré depuis (cf. S. Johansson, *Origins of Language. Constraints and Hypotheses*, Amsterdam, Benjamins, 2005, p. 168 pour une revue de la littérature).

⁶ Le débat fait évidemment rage. Pour une mise au point récente voir M. Tomasello, « What makes human cognition unique ? From individual to share to collective intentionality », *Mind and Language*, 18(2), 2003, p. 121-147. Les conclusions de Tomasello sont corroborées d'un point de vue génétique par J. T. Crow qui montre l'existence d'un changement chromosomique entre les chimpanzés et les humains, changement qui serait à l'origine de la latéralité, elle-même capitale dans l'émergence du langage « ProtocadherinXY : a candidate gene for cerebral asymmetry and language », in A. Wray (éd.), *The Transition to Language*, Oxford, Oxford University Press, 2002, p. 93-112.

⁷ S. Oppenheimer, *Out of Eden. The Peopling of the World*, Londres, Robinson, 2004, p. 18.

⁸ Le terme permet de regrouper ce que classiquement on distingue en deux catégories : signe (reconnu) relevant de la sémiologie et discours (compris) relevant de la sémantique. Voir E. Benveniste, *Problèmes de linguistique générale*, t. 2, *Sémiologie de la langue*, Paris, Gallimard, p. 65.

⁹ M. Leroi-Gourhan avait déjà remarqué que l'activité symbolique résidait, pour l'humain, dans « cette propriété unique [...] de placer sa mémoire en dehors de lui-même » (M. Leroi-Gourhan, *Le Geste et la parole. La mémoire et les rythmes*, Paris, Albin Michel, 1965, p. 33-34).

¹⁰ La formule est de Benveniste (*Problèmes de linguistique générale*, op. cit.).

¹¹ Voir W. Wildgen, *The Evolution of Human Language. Scenarios, Principles and Cultural Dynamics*, Amsterdam, Benjamins, 2004.

¹² Le modèle est celui des théories phénoménologiques de la perception, dans lesquelles les sens ne sont pas radicalement distingués les uns des autres, mais renvoient au contraire à une forme à la fois unique et différenciée. Voir M. Merleau-Ponty, « Le cinéma et la nouvelle psychologie », in *Sens et Non-sens*, Paris, Nagel, 1948, p. 100-101.

¹³ Cela n'était pas sans rapport avec la problématique d'Ernst Cassirer qui avait insisté sur le caractère transitif de la notion de forme symbolique : « C'est une caractéristique commune de toutes les formes symboliques qu'elles soient applicables à radicalement n'importe quel objet », E. Cassirer, *Le Mythe de l'État*, Paris, Gallimard, p. 34.

¹⁴ L'article fondateur à ce sujet étant celui de Lévi-Strauss publié dans le premier numéro de la revue du cercle linguistique de New York : « Analyse structurale en linguistique et en anthropologie », *Word*, 1, 1945, p. 33-53, republié dans *Anthropologie structurale*, chapitre « Langage et parenté », Paris, Plon, 1960, p. 37-62. La « pensée symbolique » dans l'article de Lévi-

Strauss deviendra ultérieurement la « fonction symbolique ». Voir par exemple, M. Izard et P. Smith (éd.), *La Fonction symbolique, essai d'anthropologie*, Paris, Gallimard, 1979.

¹⁵ La relation syntagmatique étant définie comme relation de l'élément avec les autres éléments simultanément présents, la relation paradigmatique étant définie à l'inverse comme relation d'un élément avec les autres éléments mutuellement substituables.

¹⁶ En l'occurrence, la notion même de « femme » n'aurait eu véritablement de sens que dans l'opposition entre femme à échanger/femme exigible et « l'échange » se serait réduit à n'être qu'une propriété dérivant de la nature oppositionnelle des signes. Voir C. Lévi-Strauss, *Anthropologie structurale*, *op. cit.*, p. 70-71.

¹⁷ On laissait alors le soin à « l'évolution » et à la « sélection naturelle » de faire émerger les modules.

¹⁸ C'est un des enseignements du livre de M. Godelier, *Métamorphoses de la parenté*, qui reprend de façon critique les éléments du dossier. Pour M. Godelier, l'une des raisons pour laquelle l'analogie ne tient plus aujourd'hui vient de ce que s'il était légitime, dans le cas de la linguistique, de supposer des règles totalement inconscientes rendant compte de la façon dont parle les sujets, il n'en était pas de même dans le domaine de la parenté, sous peine de renoncer à comprendre ce qui fait l'objet même de l'enquête anthropologique sur la parenté, à savoir la façon dont les individus vivent explicitement leur rapport à la norme. Bref, entre la norme implicite de la langue et la norme explicite de la parenté, les modalités de production et de reproduction des échanges seraient de nature *radicalement* différente. Voir M. Godelier, *Métamorphoses de la parenté*, Paris, Fayard, 2004, p. 374.

¹⁹ L. Scubla allant même jusqu'à dire que, pour ce qui est de l'influence des travaux de linguistique sur Lévi-Strauss, ce dernier « ne doit à peu près rien aux travaux dont il se réclame ». Voir L. Scubla, « Fonction symbolique et fondement sacrificiel des sociétés humaines », *Revue du MAUSS*, 12, 1998, p. 41-65.

²⁰ Voir C. Lévi-Strauss, *Anthropologie structurale*, *op. cit.*, en part. chap. 2 : « L'analyse structurale en linguistique et en anthropologie », p. 37-62.

²¹ Lévi-Strauss parle du système des attitudes (*Anthropologie structurale*, *op. cit.*, p. 45).

²² Voir U. Eco, *Sémiotique et philosophie du langage*, Paris, PUF, 1988, p. 193 sq.

²³ Cf. L. Scubla, « Fonction symbolique et fondement sacrificiel des sociétés humaines », art. cité, p. 41-65, qui montre que le structuralisme lévi-straussien tient à « enraciner la fonction symbolique dans l'esprit humain ».

²⁴ L'aspect quelque peu rapsodique de la liste des activités symboliques donnée par Lévi-Strauss dans « Introduction à l'œuvre de Marcel Mauss » en est un des indices : « Toute culture peut être considérée comme un ensemble de systèmes symboliques au premier rang desquels se placent le langage, les règles matrimoniales, les rapports économiques, l'art, la science, la religion. », in M. Mauss, *Sociologie et anthropologie*, Paris, PUF, 1950, p. xix.

²⁵ Le cas des travaux récents sur la parenté était, de ce point de vue, exemplaire, d'une part parce qu'après Lévi-Strauss, avaient été réintégrés des dimensions mythiques et cosmiques concernant les substances par lesquelles la parenté transite (os, sang, lait, etc.) et, d'autre part, parce que la conception même de l'alliance en termes d'échange avait été remise en question (en particulier dans le cas de mariage à tendance endogame, dit « mariage arabe »). Voir par exemple M. Godelier, *Métamorphoses de la parenté*, *op. cit.*, p. 390-396.

²⁶ Certains exemples rapportés lors des journées par Pierre Cadiot et Yves-Marie Visetti et appartenant très directement à la linguistique, discipline pourtant réputée phare pour faire du régime de la substituabilité le mode généralisé de l'accès au sens, allaient dans ce sens, en particulier l'exemple des proverbes et de leur utilisation généralisée dans certaines sociétés. Voir Y.-M. Visetti et P. Cadiot, *Motifs et proverbes. Essai de sémantique proverbiale*, Paris, PUF, à paraître.

²⁷ En 1998, dans un article d'à peine sept pages sans référence, Lévi-Strauss avait adopté une position extrêmement critique à propos d'un certain nombre de modèles néodarwiniens dont se réclamaient deux des anthropologues participant aux journées de Foljuif (C. Power et C. Knight). Nous avons estimé, au contraire, que ces modèles, si critiquables qu'ils fussent, avaient au moins le mérite de poser explicitement le problème de l'articulation entre dimension biologique (et, plus précisément, sexuelle) et dimension sociale et qu'ils valaient à ce titre plus que des sarcasmes, fussent-ils proférés par l'un des maîtres de l'anthropologie française, qui les qualifiait de « robinsonnades génitales ». Voir C. Lévi-Strauss, « La sexualité féminine et l'origine de la société », *Temps modernes*, 598, mars-avril 1998, p. 66-84.

²⁸ Voir, par exemple, R. Dawkins, *The Selfish Gene*, Oxford, Oxford University Press, 1982.

²⁹ Voir, par exemple, P. Chase, « Symbolism as reference and symbolism as culture », *The Evolution of Culture. An Interdisciplinary View*, in C. Knight, R. Dunbar et C. Power (éd.), New Brunswick, Rutgers University Press, 1999, p. 34-49. L'article a le mérite de pousser jusqu'au bout la logique de l'argument, qui reste généralement implicite.

³⁰ C'est la conclusion à laquelle parvient P. Chase dans l'article cité *supra*, note 29.

³¹ Voir, par exemple, C. Darwin, *The Descent of Man*, Princeton, Princeton University Press [1871], 1981, chap. v, p. 166.

³² Voir pour la thèse néodarwinienne standard, G. C. Williams, *Adaptation and Natural Selection*, Princeton, Princeton University Press, 1966 et, pour des arguments contraires, E. Sober et D. S. Wilson, *Unto Others. The Evolution and Psychology of Unselfish Behavior*, Harvard, Harvard University Press, 1998.

³³ Voir G. C. Williams, *Adaptation and Natural Selection*, Princeton, Princeton University Press, 1966, p. 23-24.

³⁴ Il faut supposer des modèles dans lesquels des groupes composés de sous-groupes eux-mêmes composés de deux types d'individus se reproduisent de façon inégale selon le nombre d'individus du même type dans leur environnement. On s'aperçoit que, pendant des phases qui resteraient transitoires si les sous-groupes ne se réunissaient pas à nouveau puis se séparaient ensuite, le nombre d'individus ayant un taux de reproduction plus faible dans chaque groupe tend cependant à croître globalement. Voir E. Sober et D. S. Wilson, *Unto Others. The Evolution and Psychology of Unselfish Behavior*, *op. cit.*, p. 20.

³⁵ Voir E. Sober et D. S. Wilson, *ibid.*, p. 50 et p. 150 pour les cas biologiques (*sex ratio* et virulence d'un virus) ; p. 159 *sq.* pour les cas liés à la norme sociale et aux très fortes contraintes qui pèsent sur les individus dans de nombreuses sociétés archaïques.

³⁶ Vilmos Csányi, dans de trop brèves remarques, développe une argumentation assez analogue, centrée sur la question de la représentation collective précédant l'action et non sur celle de la parenté. Voir V. Csányi, « An ethological reconstruction of the emergence of culture and language during human evolution », in G. Györi (éd.), *Language Evolution. Biological, Linguistic and Philosophical Perspectives*, Francfort, Peter Lang Verlag, 2001, p. 41-53.

³⁷ Dans *The Descent of Man* (1871), Darwin rapportait déjà l'émergence d'*Homo sapiens* à un processus de sélection sexuelle.

³⁸ G. C. Williams, *Adaptation and Natural Selection*, *op. cit.* En conservant le critère du différentiel de reproduction utilisé dans le cadre de la sélection naturelle, il paraît nécessaire d'admettre que la voie de la sexuation a eu, en tant que stratégie évolutive, un plus grand succès que la voie de l'absence de sexuation, dans la faune comme dans la flore. Williams fait remarquer que les ancêtres du Pléistocène des espèces asexuées d'aujourd'hui étaient des espèces sexuées mais que les espèces asexuées du Pléistocène n'ont, aujourd'hui, presque plus de descendants : la sexuation des espèces apparaît donc comme un trait *fonctionnel* qui, en contribuant à la diversification des espèces par le maintien d'une variabilité interne et, indirectement, par ce biais, au retard dans leur extinction, *a servi les espèces sexuées en général* et non pas seulement l'avantage reproductif individuel au sein d'une espèce particulière.

³⁹ Okanoya ne limite pas le rôle de la sélection sexuelle au cas humain. Voir K. Okanoya « Sexual display as a syntactical vehicle : the evolution of syntax birdsong and human language through sexual selection », in A. Wray (éd.), *The Transition to Language*, Oxford, Oxford University Press, 2002, p. 46-63.

⁴⁰ Mais pas nécessairement de la même manière, comme le montre le modèle de la « menstruation factice » développé à l'origine par C. Knight dans *Blood Relations : Menstruations and the Origin of Culture*, Londres, Yale University Press, 1991 et repris ultérieurement par C. Power, « Beauty magic : the origins of art », in R. Dunbar, C. Knight et C. Power (éd.), *An interdisciplinary View*, *op. cit.*, p. 92-112.

⁴¹ F. Héritier, « À propos de la théorie de l'échange », *L'Homme*, 154-155, 2000.
<http://lhomme.revues.org/document24.html>

⁴² Communication personnelle.

⁴³ Il ne s'agit pas ici de discuter de la validité ou de la portée du modèle mais seulement de proposer une façon de concevoir la notion d'activité symbolique. Ce modèle peut d'ailleurs être enrichi par celui de la « menstruation factice » développé par Knight tout d'abord puis par Knight, Aiello et Power ensuite.

⁴⁴ Il n'était d'ailleurs même pas besoin que les deux sexes comprennent la même chose. Voir le modèle de la « menstruation factice » de C. Power.

⁴⁵ On verra plus bas comment le modèle de N. Allen peut se rapporter à cette hypothèse.

⁴⁶ Cette hypothèse me semble aller dans le même sens que celle formulée par Knight, même s'il serait certainement opposé à toute l'argumentation qui précède sur la sélection de groupe. Voir C. Knight, « Language as a revolutionary consciousness », in A. Wray (éd.), *The Transition to Language*, *op. cit.*, p. 138-160.

⁴⁷ Voir l'article déjà cité de C. Lévi-Strauss à propos de certains modèles néodarwiniens ayant pour thème la « perte de l'œstrus » : « La sexualité féminine et l'origine de la société », p. 66-84.

⁴⁸ Dans *Métamorphoses de la parenté*, *op. cit.*, celui-ci critique aussi bien les modèles néodarwiniens de C. Knight (p. 454) que le modèle émergentiste de N. Allen (p. 549-551).

Biological substrates of human kinship : the view from life history theory and evolutionary ecology

CAMILLA POWER

Résumé

Le but de cet article est de montrer, dans la lignée de R. Fox, que les notions de résidence, d'affiliation et d'accouplement – qui distinguent les humains des primates – doivent jouer comme autant de contraintes pesant sur le cours ultérieur du développement proprement humain, après la mise en place des règles symboliques de filiation et des catégories de parenté. D'un point de vue darwinien, c'est l'avantage sélectif qui gouverne l'histoire de la vie et même si les règles de parenté sont, de ce point de vue, des fictions, encore faut-il pouvoir dire quels sont les types de fiction qui prévalent. L'auteur veut démontrer que toute coopération entre mâle et femelle (interprétée comme contrainte pesant sur les règles ultérieures de parenté) doit reposer sur une coopération préalable entre femelles.

L'hypothèse de l'« homme chasseur », troquant la viande qu'il a chassée contre un commerce sexuel auprès d'une femelle unique et une certitude de paternité touchant sa progéniture n'est plus crédible du point de vue archéologique et c'est plutôt l'hypothèse d'un gain en prestige par une attitude de libéralité à l'égard de la collectivité toute entière qui est aujourd'hui retenue. En faisant appel aux modèles de la théorie des jeux, on se rend compte que les dons en provenance des mâles et en direction des femelles sont susceptibles de se produire d'autant plus facilement que le dimorphisme sexuel a tendance à diminuer (au moins dans le cas où les femelles ont développé des stratégies visant à punir les mâles non coopérateurs) pour des raisons de coût énergétique. On parvient à la même conclusion d'un point de vue archéologique : à partir de 2 millions d'années, la taille du cerveau d'Homo ergaster a doublé par rapport à celui des chimpanzés et le dimorphisme sexuel a diminué, principalement par une augmentation de la taille des femelles, ce qui implique une plus grande dépense d'énergie de leur part et sans doute une plus grande coopération des mâles envers elles. Si l'encéphalisation joue un rôle de pression de sélection pour les femelles, il y a deux façons pour elles de répondre aux demandes énergétiques croissantes que cette encéphalisation requiert : (i) en modifiant la façon dont elles font usage de leur énergie. Par exemple, un meilleur régime alimentaire peut permettre de réduire le coût de maintenance des boyaux, dont le coût énergétique est élevé ; (ii) en demandant de l'aide aux autres – que ce soit d'autres femelles ou des partenaires masculins potentiels. Un certain nombre de traits typiques de la vie humaine doivent être pris en considération : la durée pendant laquelle les petits humains sont complètement dépendants implique un meilleur régime alimentaire que celui des primates ; le retard dans la maturité sexuelle lié à une plus grande espérance de vie ; la ménopause et les vingt ans en moyenne qu'une femelle

humaine peut escompter vivre après son temps de reproduction. L'« hypothèse de la grand-mère », c'est-à-dire d'une coopération entre les femelles, rend compte de tous ces traits spécifiques : une grand-mère est le candidat le plus sûr pour assurer la garde des enfants longtemps dépendants ; elle permet également un plus grand succès reproductif à sa fille. L'hypothèse ne va d'ailleurs pas à l'encontre de celle de l'homme chasseur mais la réintègre autrement puisque les mâles recherchent les femelles ayant le plus grand succès reproductif et que celles-ci recherchent des protéines dont leur progéniture a particulièrement besoin : ainsi les deux hypothèses se renforcent-elles mutuellement.

This paper deals with the biological substrates and constraints on kinship affiliations through human evolution drawing on the latest modelling from life history theory and evolutionary ecology. As far as evolving hominins go, we are dealing with periods preceding prehistory, lacking any evidence for symbolic culture or kinship. For early hominins with ape-like life histories, it is reasonable to adopt terms from primatology in describing residence patterns (e. g. female philopatry where females stay and males move from natal groups ; male philopatry, males stay, females move) or allegiance (e. g. male kin-bonded with brothers as allies, or female kin-bonded with mothers, sisters, daughters as main allies). These are not equivalents to human symbolically constituted residence and descent rules. However, as Fox argues, the pragmatics of residence, affiliation and mating on the ground must precede and constrain what subsequently develops in terms of descent rules and categoric kinship systems. So, too, in human evolution, the residence and genetic kin alliances we start from determine certain possible pathways, while excluding others.

The areas of Darwinian theory most applicable to kinship studies are life history theory¹ and behavioural ecology². The first addresses how and why organisms apportion effort to maintain survival and reproduction through their lifetimes. It is based on an essential trade-off between time and energy used in somatic maintenance and that used to reproduce. For mammal species, including primates, human and non-human, it asks, how long is spent in gestation, in lactation ? How long does it take to reach adulthood and first reproduction ? How long are the intervals between reproductive events and how long will you live ? The second area, evolutionary ecology, assumes that every organism behaves in ways to promote its own survival and reproduction, but then explores the variability of strategies for maximising reproductive fitness given different ecological conditions and constraints. The kinds of evidence that can be used to test hypotheses on the evolution of human life history include comparative data across extant primate species, archaeological and fossil data. Evolutionary ecological hypotheses can potentially be tested by looking at observable fitness effects in contemporary populations. We may be the only symbolic species, but we are still also animals who are likely to act in ways that promote fitness. Ultimately, where kinship is concerned, evolutionary anthropologists should investigate the underlying fitness interests which lead to the evolution of different types of kinship system³. Symbolic systems of kinship terminology do indeed consist of communally entertained « fictions » which may contradict genetic

realities ; but there will still be Darwinian causes for one set of fictions prevailing over another.

In narratives of human social evolution, it is no longer credible to make the assumptions that shored up the old « man the hunter » hypothesis⁴. Ideologically motivated, these imposed nuclear family structures onto the Pleistocene past, reading a sex division of labour and family life into the juxtapositions of fossils, tools and animal bones in archaeological assemblages⁵. The old story ran that as Plio-Pleistocene males became increasingly successful at hunting, they brought back meat to females and juveniles at central places for foraging, tending to establish pair-bonds and trade such regular provisioning for « paternity certainty » each with their own woman. The idea that we have been quasi-hunter-gatherers for the past 2 million years has not stood up to scrutiny of the archaeological record on Pleistocene scavenging⁶, nor is it consistent with evidence from hominin fossils on sexual size dimorphism and life history schedules.

But the main challenge to the old « man the hunter » ideas comes as a matter of principle from « selfish-gene » theory. Because the sexes get genes into the next generation by different ways, they have different calculations about fitness. Females are forced into high levels of parental investment. For a male, investment in an offspring may come at a high opportunity cost if he has chances of mating elsewhere⁷. While it is likely that some form of male care or paternal solicitude is ancient in primates as a guard against risk of infanticide, this is different from male parental investment (MPI) involving provision of energy to mother or offspring. The latter is rarely if at all seen among non-human primates, and we have to account carefully for its evolution in terms of fitness benefits and costs to males. The work of evolutionary ecologists on hunter-gatherers today has undermined beliefs in MPI as the main motivation for men's hunting⁸ proposing instead that men are « showing off » and interested in extra matings. In the Hadza case, a man's kill is distributed throughout a camp, not to one specific nuclear family. If men are not trading provisioning for pair-bonds and paternity certainty, we need different models for the emergence of a social division of labour⁹.

Recently, Sarah Hrdy has broken the old « man the hunter » mould by characterising evolving human ancestors as « cooperative breeders », tending to have multiple extended family forms that were flexible in response to demographic uncertainty¹⁰. But if evolutionary anthropology has anything to say about the evolution of human forms of kinship, it is necessary to investigate the main pathways that natural selection would operate on to produce early forms of social cooperation, including divisions of labour. Critical here are the energetic costs of reproduction for each sex. These have altered during the course of human evolution particularly because of increases in brain size – the brain being an especially costly organ, the costs falling on mothers of encephalised offspring – and also increases in body size, which require extra energy for maintenance.

Cathy Key used game theory models to explore the effect of the relative reproductive costs of the sexes on their likelihood of cooperation, both within each sex and between the sexes¹¹. The main determinant of sex difference in energetic costs is body size. If males need to be much bigger than females to succeed in reproductive competition, their costs will be similar to female costs. Key found that where female costs of reproduction were high, female-female cooperation was strongly selected. Males were not likely to cooperate with females where their

costs were relatively high (i. e. their body sizes significantly bigger), but, when female costs rose relative to males, males would become « unconditionally cooperative » with females. This means they would give females benefits (such as gifts of high-energy food) even where they might receive nothing in return. So, a flow of benefits from males to females becomes more likely as sexual size dimorphism decreases. But it is important that this outcome depended on the condition that females developed strategies to « punish » non-cooperative males by long-term refusal of cooperation. There is also no necessary basis of « paternity certainty » in this model. Both males and females are likely to « trade » with more than one partner.

How can Key's abstract model be related to the actual fossil record ? Among earlier hominins, australopiths prior to 2 million years who retained significant climbing abilities, brains and bodies were relatively small, with high size dimorphism between the sexes (see Table 1).

Table 1– Indicators of life history change in hominins.

Hominin grade	Brain/body size	Date	Life history
Australopiths, Encephalised early « Homo »	Small brains/bodies – brains increase, bodies stay small	Pliocene, before 2 mya	Ape-like growth schedule and mortality
<i>H. ergaster/erectus</i>	Bigger brains and bodies	Early Pleistocene, from 1.75 mya	Intermediate between apes and modern humans; reduced mortality
<i>H. heidelbergensis</i> , ancestor to us and Neanderthals	Large brain, large robust body	Middle-Late Pleistocene, from 0.5 mya	Virtually modern human schedule/lifespan

From about 2.5 million years, some of these species began to encephalise while bodies remained quite small and apparently still highly dimorphic. This suggests increasing costs for females, indicating more pressure for female-female cooperation, while males still had high body-size costs and were less likely to be cooperative. These encephalised early *Homo* species led to the emergence of *Homo ergaster* after 2 million years, the first hominin with body proportions like us, bodies that were bigger and designed for walking not climbing. Their brains, though not more encephalised (that is a relative measure of brain to body weight) than earlier *Homo*, were absolutely twice the size of chimpanzee/australopithecine brains. Sex size dimorphism had reduced, largely because female *Homo ergaster* increased body size disproportionately more than males¹². With female costs rising relative to males, significantly more cooperation by males with females can be expected from this time. But this is based on the prior evolution of interfemale cooperation. We know from hunter-gatherer economies that male hunters do significantly contribute to and subsidise female reproductive costs¹³. Already in

early Pleistocene scavenging economies, males may have been giving females significant benefits – but we should be careful not to make assumptions about MPI. We need to consider the implications of female cooperative strategies as the necessary basis for the emergence of male-female cooperation.

Across species, brain and body size are generally closely related to life history variables such as age at first reproduction and lifespan. Therefore fossil remains can provide important evidence about the life history of those species (see Table 1). For evolving *Homo* females of the Plio-Pleistocene who came under selection pressure of, encephalization, there are two basic areas for evolutionary change to enable females to meet their costs. She can :

- alter the ways she allocates her own energy and resources, leading to change in stages of life history, patterns and rates of growth, etc ;
- get other people to help ! Possible candidates – female kin, or actual/potential mates. Which is she likely to turn to first ? Key's model says it must be her own relatives first.

Under the first category (changing the ways she uses her own energy) comes the brain/guts trade-off (which applies to both sexes). By the expensive tissue hypothesis¹⁴ it is possible for an organism to run a larger brain without increasing basal metabolic rate if expensive tissue from another part of the body is reduced. The gut is the part that can be most readily reduced, but only if the animal finds a higher-quality diet. Relative to early *Homo*, *Homo ergaster/erectus* females increased body size proportionately more than males did. Why the strong selection pressure on females ? Small gut size means higher quality diet, which requires larger foraging areas. *Homo ergaster* females had to travel further, they needed larger bodies of the right shape to give them more efficient bipedality, and thermoregulation in increasingly arid environments. Larger body size also aided females who had to carry offspring that is helpless for longer. Another benefit is that the bigger the mother in relation to the offspring, the more efficient lactation¹⁵. This means that the reduction of sexual size dimorphism critical to Key's model for emergence of male-female cooperation is driven originally by females meeting their own costs. It is not driven by changes in behaviour between the sexes (i. e. reduction of male sexual competition). But Key's model says that increasing male-female cooperation may be an outcome.

Life history changes also come into the first category, but may be affected or triggered by changes in foraging behaviour and new strategies of allocare, which shades into the second area – recruiting energy from others. How could female-female cooperation affect life history ?

Special life history characteristics of humans today are :

- a) secondary altriciality ;
- b) childhood ;
- c) delayed sexual maturity, taking the form of adolescence ;
- d) menopause.

Secondary altriciality refers to the extreme helplessness of a human infant, owing to the rapidity of brain development in first year and lack of development of motor skills or digestive function. Compared with a chimp, a human baby is retarded in development. That makes sense for the mother, because she has significantly more energy to find than a chimp mother, so she slows the whole process down¹⁶. But, the paradox is, we have a much shorter interbirth interval (IBI) than chimps. How can this be ?

Childhood is special to humans because unlike primate juveniles, human children, once weaned, are still dependent on adult help. Childhood is characterised by immature dentition ; very small guts relative to body and brain size ; and still growing brains. Children need special diets.

Delay in sexual maturity is only possible if an animal expects to live longer. A later age of first reproduction will only be favoured if adult mortality rates improve. Brain size across species correlates well with first age of reproduction and overall lifespan. So the increased brain size of *Homo ergaster* indicates delay of sexual maturity. So does body size. By Charnov's life history model, if mortality rates are reduced and lifespan increases, it pays to spend time growing a larger body, because you should reproduce more successfully when you do start.

Menopause. A classic problem of evolutionary theory. Why select for living after last reproduction ? The « grandmother » hypothesis – senior females add to their fitness by helping with daughter's offspring – offers a neat solution, but is it viable ? As originally modelled, « grandmothing » was made to compete against further mothering, i. e. carrying on having more offspring. And in terms of overall reproductive success, it cannot win, if that is the alternative. But Hawkes and her colleagues, O'Connell and Blurton Jones, argue that we should model it starting from the « chimp » baseline. A female chimp and a female human have roughly the same reproductive lifespan. The chimp starts earlier, ends a little earlier, but both have finished reproduction between 40-45. At that point, a chimp has very short life expectancy. A hunter-gatherer woman has life expectancy of 20 or more years. What we have to explain is selection for these extra years, with no selection for extra child-bearing¹⁷.

The « grandmother hypothesis » elegantly accounts for the evolutionary onset of all these special human characters¹⁸. The mother's mother is the most reliable candidate for offering allocare for weanling children. A mother who has a long-lived, vigorous mother will be able to shorten her IBIs and increase reproductive output relative to other females. This sets up selection directly for longer lifespan and greater allocation of somatic effort to maintaining the body post reproduction. The consequent reduction of mortality rates allows delay in sexual maturity. O'Connell and colleagues place these developments in a specific evolutionary context of change in climate and foraging strategies that triggered these new social strategies. With the increasing aridity of the Early Pleistocene, it became harder for weanlings especially to find accessible resources. Adults and older siblings would have to help. This climate favoured such resources as roots and tubers (underground storage organs or USOs) which became widely available (associated with geographic range of *Homo ergaster/erectus*) but could only be processed by adults. These dietary strategies imply new social strategies based in female kin-bonded structures. Available resources allowed larger groups of related females to congregate together, leading to increased protection of juveniles, and reduction of mortality (among primates, weanlings have a specially high mortality rate, because they have to go it alone).

In modelling the effects of body size on reproductive costs for chimps and hominins, Key shows that female *Homo ergaster* could not have evolved her larger body (and with it the longer lifespan) without drastically shortening IBI relative to chimps¹⁹. This is because breast-feeding is the most expensive aspect of reproduction. Shortening lactation greatly reduces costs, but it is only possible if there is someone to help with allocare. We cannot reasonably suppose that males were the first to step in. « Grandmother » hypothesis challenges the old

assumptions of « man the hunter », « provisioning for paternity », mainly on the grounds that modern hunter-gatherer men do not appear to target the meat they hunt to specific offspring. Also, although modern day hunters do produce a significant overall contribution to diet, any individual male would be too unreliable in providing the daily sustenance that children require. Observation of Hadza hunter-gatherers shows that older women can do that, providing staple resources including USOs.

What this means is that to become the animal we are today, with our special life history characteristics, our ancestors necessarily evolved in female kin-bonded social structures. We know this process commenced from the Early Pleistocene because of the evidence of body size change. Despite some problems with the model (e.g. How many females would live long enough?), it appears that « grandmother » was vital to our evolution.

So is « man the hunter/scavenger » dead and buried? Not necessarily. « grandmother » leads us to a modified view of « man the hunter ». Key's model said that, when females have relatively high costs, males become more inclined to help. But which females will they help most? The most attractive to males will be those females who are most frequently fertile, i. e. those with reduced IBIs. So, females with older female kin who take the weaned children off their hands will get more male help. Meat is especially valuable for children because of micronutrients. Females who get meat gifts from males will be able to reduce their IBIs even further and/or their children will survive better. So « grandmother » and « man the cooperative scavenger » become mutually reinforcing. Males will come to actively choose females who have senior female kin support, because their children will survive better. But we can expect mating effort rather than paternal strategies initially.

In the final phase of encephalization, from 500 000 years among *Homo heidelbergensis*, the ancestor of us and the neanderthals, female costs again rose steeply. Female coalitions adopted strategies to force males to cooperate in big-game « show-off » hunting. Males became more productive, fuelling the larger brains of the offspring. Ultimately, these female coalitionary strategies led to the emergence of ritual and symbolism²⁰. Because of the importance of strategies of female-female cooperation, there is little reason to suppose the prior structures of female kin-bonding would have been altered at this stage. Therefore we are led to the conclusion that modern humans and their immediate ancestors were biased to matrilocality, forming a basis for matrilineal priority in the earliest symbolic kinship systems.

Notes de CAMILLA POWER

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Revisiting matrilineal priority

CHRIS KNIGHT

Résumé

Au siècle dernier, jusque dans les années 1920, la plupart des anthropologues se rangeaient à l'idée que l'évolution de l'humanité était passée par des « stades » et qu'un stade matrilineaire avait précédé un stade patrilinéaire. Après une période de critique radicale, la situation a changé aujourd'hui et ce qui apparaissait comme un résultat incontesté de l'anthropologie du ^{xx}e siècle doit être précisé, en particulier en ce qui concerne la question des rapports entre avunculat (c'est-à-dire la relation particulière existant entre un homme et le fils de sa sœur et qui peut porter sur des droits de résidence, de partage des repas ou autre) et matrilinearité (filiation par les mères).

Au ^{xix}e siècle, l'antériorité matrilineaire était fondée sur deux hypothèses ethnographiques : (i) aucune société connue n'était passée d'un système patrilinéaire à un système matrilineaire ; (ii) il était possible d'interpréter le cas de l'avunculat, cas particulier à certaines sociétés seulement, comme une trace de priorité matrilineaire dans des systèmes devenus patrilinéaires.

La première hypothèse fit l'objet d'une critique vigoureuse de la part de Franz Boas : celui-ci avait découvert une société (les Kwakiutl) en train de passer d'un système patrilinéaire à un système matrilineaire sous l'influence de voisins organisés matrilineairement. Le schéma évolutionniste standard était donc inopérant. En fait, le cas étudié ne permettait sûrement pas de tirer des conclusions de cette ampleur mais Boas voulait surtout remettre complètement en question l'aspect déterministe et unidirectionnel du cadre évolutionniste hérité du ^{xix}e siècle. Le résultat fut surtout que, parmi les anthropologues américains, la génération suivante essaya d'établir une direction en sens inverse, allant du patrilinéaire au matrilineaire. Ce n'est qu'à partir du moment où l'idée de diffusion de la culture eut droit de cité en anthropologie que les aspects unidirectionnels du cadre évolutionniste furent progressivement abandonnés.

La deuxième hypothèse, portant sur l'avunculat, fut critiquée par Radcliff-Brown : sa définition de l'avunculat lui permettait de se passer des « stades » propres au cadre évolutionniste. Pour lui, le point important pour déterminer la filiation consistait à pouvoir déterminer à qui l'enfant d'une femme appartenait : s'il appartenait au groupe de sa mère, la filiation était matrilineaire, s'il appartenait au groupe de son père, elle était patrilinéaire. Dans le cas d'une filiation patrilinéaire – et en faisant l'hypothèse que le lien mère-enfant est le seul qui soit naturellement donné –, le mariage sépare la mère de son groupe de filiation d'origine, et en particulier de son frère, tandis que cet effet ne se produit pas dans le cas d'une filiation matrilineaire, le frère et la sœur devenue mère restant dans le même groupe de filiation. Ainsi la

façon dont le lien du mariage est considéré rend-elle compte de la pratique de l'avunculat sans qu'il soit nécessaire de faire intervenir des hypothèses de nature évolutionniste : si le lien mari-épouse est prépondérant dans une société, il se fait au détriment du lien frère-sœur et vice versa. Mais si le lien mari-épouse s'explique par la nécessité de l'alliance, le lien frère-sœur, lui, n'est pas rapporté par Radcliff-Brown à la matrilinearité et reste, tel quel, totalement inexpliqué.

L'exemple des sociétés africaines permet de supposer que les sociétés où se pratique l'avunculat, prioritairement patrilinéaires pour ce qui est de la filiation, doivent cependant demeurer minoritairement structurées selon un principe matrilineaire, puisque persistent en elles des groupes au sein desquels se côtoient des hommes, leurs sœurs et leurs enfants sur lesquels les pères n'ont de droits que s'ils les achètent auprès du groupe de leur femme (dans le cas de la société bantoue par exemple). Ce rachat peut être interprété comme l'étape finale d'un processus par lequel s'opère la séparation des femmes et de leurs enfants qui, sinon, resteraient dans le groupe de leur mère. En Afrique, c'est le rachat sous forme de têtes de bétail qui fait la différence entre les systèmes de filiation : dans le cas où il n'y a pas de bétail, on a une société matrilineaire-matrilocale ; à partir du moment où le bétail apparaît et sert à la compensation matrimoniale (départ de l'épouse vers la résidence du mari), on a un système matrilineaire-patrilocal ; quand la compensation devient plus importante, on trouve des systèmes patrilinéaires-patrilocaux dans lesquels le divorce n'existe pas (Harold Schneider). Reste à comprendre les raisons du maintien de liens matrilineaires dans des sociétés patrilinéaires.

Le lien frère-sœur n'est pas seulement une survivance parce que la grande licence de conduite du neveu à l'égard de son oncle – sans que ce dernier puisse faire preuve d'autorité à son égard – n'existe que dans le cadre patrilinéaire, alors que l'autorité de l'oncle sur son neveu est beaucoup plus présente dans le cadre strictement matrilineaire. L'absence d'autorité de l'oncle sur son neveu est donc bien plutôt la marque de la transition d'un système matrilineaire vers un système patrilinéaire. Encore faut-il admettre de relier la nature de l'avunculat à la filiation matrilineaire, ce que la majorité des anthropologues (y compris les plus fameux comme Evans Pritchard, Lévi-Strauss, Leach ou, dans une moindre mesure, Goody), suivant en cela Radcliff-Brown, se refusent à faire, prenant la relation frère-sœur comme un simple axiome.

Nineteenth century anthropologists widely agreed that early human society was not based on the nuclear family. Lewis Henry Morgan instead championed the matrilineal clan as the first stable institutional framework for human family life. In this, he was supported by theorists who later came to include E. B. Tylor, Friedrich Engels, W. H. R. Rivers, Émile Durkheim and Sigmund Freud. Until the 1920s, most anthropologists still accepted a « stages » view of the evolution of kinship, in which matrilineal descent systems universally preceded their patrilineal counterparts.

When Morgan's evolutionist schema was discredited early in the twentieth century, it was largely on the basis of two interventions. First, F. Boas claimed to have discovered a Vancouver Island tribe (the Kwakiutl) in the throes of transition from *patrilineal* to *matrilineal* descent, reversing Morgan's supposedly universal sequence. Second, A. R. Radcliffe-Brown offered an explanation of the « avunculate » in terms of universal psychological and sociological principles, claiming that the « matrilineal survival » interpretation of this peculiarity of kinship could therefore be dispensed with.

In this article, I make no attempt to return to the debate or adjudicate on the substantive issues involved. My more modest aim is to recall key features of the debate, highlighting the motivations and agendas involved at the time.

If there is one thing on which all schools of social anthropology are agreed, it is that the nineteenth century « mother-right » theory of early kinship is of no more than historical interest. Of all the theoretical conquests achieved by Boas, Kroeber, Lowie, Malinowski, Radcliffe-Brown and their allies during the first decades of the twentieth century, the overthrow of the Bachofen-Morgan evolutionary scheme has appeared the most secure. To the extent that twentieth century social anthropology solved the problems it set out to address, those responsible for this paradigm shift must posthumously be accorded full credit. Should we conclude, however, that twentieth century social anthropology stumbled from crisis to crisis, solving not one of the most basic problems facing it, then a search for the roots of our crisis might return us to that decisive moment in the history of our discipline.

I. The discovery of matriliney

Matrilineal exogamy was first accurately described in print by an early English adventurer, John Lederer, in an account of his travels in Eastern North America published in 1672. He was writing of the Tutelo, an eastern Siouan tribe¹ :

From four women, viz., Pash, Sopoy, Askarin and Maraskarin they derive the race of mankind ; which they therefore divide into four Tribes, distinguished under those several names. They very religiously observe the degrees of marriage, which they limit not to distance of Kindred, but difference of Tribes, which are continued in the issue of the females : now for two of the same tribe to match, is abhorred as Incest, and punished with great severity.

What Lederer refers to as a « tribe » is, in modern anthropological parlance, a « clan ». In this case, there are four matrilineal clans within the same tribe, marriage within the same clan being equally prohibited no matter how « closely » or « distantly » related the partners.

Fifty-two years later, Father Lafitau² described in glowing terms the apparent equality and even (in important respects) superiority of the female sex over the male among the Iroquois :

Nothing... is more real than this superiority of the women. It is essentially the women who embody the Nation, the nobility of blood, the genealogical tree, the sequence of generations and the continuity of families. It is in them that all real authority resides : the land, the fields and all their produce belongs to them : they are the soul of the councils, the arbiters of peace and war : they conserve the finances or the public treasury ; it is to them that slaves are given: they make the marriages, the children are in their domain and it is in their blood that the order of succession is based.

In 1767 the great Scottish historian Adam Ferguson was the first to generalize on the basis of Lafitau's and other missionaries' and explorers' reports. Writing of « savage nations » in general he remarked that³ :

[...] as the domestic cares are committed to the women, so the property of the household seems likewise to be vested in them. The children are considered as pertaining to the mother, with little regard to descent on the father's side.

This suggestion was not followed up, however, until in 1861 the Swiss jurist and historian of Roman law, Johann Jakob Bachofen, published his « Mother-Right ». Bachofen knew little at this time of ethnology beyond that contained in the literature of ancient Greece and Rome. His knowledge of the classics was sufficient, however, for him to make a persuasive-seeming case for the priority of matriliney from these sources alone. An idea of the kind of evidence he relied on is given by these two excerpts :

Of all records relating and pertaining to mother right, those concerning the Lycian people are the clearest and most valuable. The Lycians, Herodotus reports, did not name their children after their fathers like the Hellenes, but exclusively after their mothers ; in their genealogical records they dealt entirely with the maternal line, and the status of children was defined solely in accordance with that of the mother. Nicolaus of Damascus completes this testimony by telling us that only the daughters possessed the right of inheritance⁴.

Or again :

[...] I should like to cite a report from Nicolaus of Damascus' collection of strange customs, preserved by Stobaeus in his *Florilegium* : « The Ethiopians hold their sisters in particular honor. The kings leave their scepter not to their own children but to their sisters. If no heir is available, they choose the most beautiful and belligerent as their leader. » This last observation is confirmed by Herodotus and Strabo. The favoring of the sister's children is a necessary consequence of mother-right and is also to be found elsewhere⁵.

From his analysis of the myths, literature and ethnological reports of ancient Greece and Rome, Bachofen reached a conclusion which he expressed in these words⁶ :

[...] mother right is not confined to any particular people but marks a cultural stage. In view of the universal qualities of human nature, this cultural stage cannot be restricted to any particular ethnic family. And consequently what must concern us is not so much the similarities between isolated phenomena as the unity of the basic conception.

J. P. McLellan read Bachofen's book in 1866, having already the previous year published his *Primitive Marriage*⁷ in which he claimed to have proved that « the system of kinship through females » was « a more archaic system of kinship than the system of relationship through males ». It was Morgan, however, who did most to support Bachofen's case by relating it to evidence concerning the kinship systems of the Iroquois and other Indians of the United States. Morgan related matrilineal clans to the solidarity between sisters living together, and associated this in turn with the idea of « communism in living⁸ ». Describing an Iroquois long-house he wrote of its immense length, its numerous compartments and fires, the « warm, roomy and tidily-kept habitations » within the long-house, the raised bunks around the walls, the common stores and « the matron in each household, who made a division of the food from the kettle to each family according to their needs [...] » « Here », he commented, « was communism in living carried out in practical life... »⁹. In such households, he concluded, « was laid the foundation for that "mother-power" which was even more conspicuous in

the tribes of the Old World, and which Professor Bachofen was the first to discuss under the name of gynocracy and mother-right ». Morgan and Bachofen exerted a reciprocal influence on each other. While Bachofen's ideas on the priority of matriliney became incorporated into Morgan's *Ancient Society* (1877), Bachofen appreciated the comparisons drawn by Morgan between his own classical materials and the matrilineal clan system of the Iroquois. Bachofen expressed his gratitude by dedicating a book of essays to Morgan¹⁰.

Following some suggestions made by Marx, Engels wrote his *The Origin of the Family, Private Property and the State* (1884) in order to show the way in which Bachofen's and Morgan's positions gave strength to the interpretation of human evolution and history which he and Marx had evolved. For Engels, the base line from which alone the study of kinship-solidarity could begin was provided not by the model of « the family » in anything like the modern European sense but by the model of the matrilineal clan. Just how important this seemed to him can be judged from the following evaluation of Morgan's work¹¹ :

The rediscovery of the original mother-right gens as the stage preliminary to the father-right gens of the civilised peoples has the same significance for the history of primitive society as theory of evolution has for biology, and Marx's theory of surplus value for political economy. It enabled Morgan to outline for the first time a history of the family, wherein at least the classical stages of development are, on the whole, provisionally established, as far as the material at present available permits. Clearly, this opens a new era in the treatment of the history of primitive society. The mother-right gens has become the pivot around which this entire science turns ; since its discovery we know in which direction to conduct our researches, what to investigate and how to classify the results of our investigations.

This was not a hasty judgment. It should be remembered that even as early as 1844, Marx had written that the « immediate, natural and necessary relationship of human being to human being is the relationship of man to woman », and that « from this relationship the whole cultural level of man can be judged¹² ». The matrilineal clan, based on the solidarity of women within large, collective households, seemed to Marx and Engels merely a concrete embodiment of something in which they had always believed – namely, an original form of social organization in which the sexes had been equally powerful and in which not even the rudiments of private property, class-exploitation or the state had as yet evolved. Marx and Engels wrote in *The German Ideology*¹³ of « [...] property, the nucleus, the first form, of which lies in the family, where wife and children are the slaves of the husband ». Morgan's view that private property had emerged through the break-up of the originally communal matrilineal households of an earlier phase of social evolution seemed to dovetail neatly with this idea and was taken up eagerly by Engels. Morgan described how the transition to patrilineal descent had been associated with the breaking of women's own kinship bonds and their isolation from one another in the home¹⁴ :

It thus reversed the position of the wife and mother in the household ; she was of a different gens from her children, as well as her husband ; and under monogamy was now isolated from her gentile kindred, living in the separate and exclusive house of her husband. Her new condition tended to subvert and destroy that power and influence which descent in the female line and the joint-tenement houses had created.

Engels commente¹⁵ :

The overthrow of mother right was the world-historic defeat of the female sex. The man seized the reins in the house also, the woman was degraded, enthralled, the slave of the man's lust, a mere instrument for breeding children.

He continued, rooting the central features of class society in the original processes through which marriage bonds had undermined the solidarity of women in the matrilineal joint household and clan¹⁶ :

The first class antagonism which appears in history coincides with the development of the antagonism between man and woman in monogamian marriage, and the first class oppression with that of the female sex by the male.

II. Boas and the Kwakiutl case

The matrilineal priority hypothesis rested on two fundamental props :

- a number of matrilineal descent systems were known to have shifted towards patriliney in recent times. No case of a society evolving in the reverse direction was known ;
- in certain African societies with patrilineal descent, apparently inconsistent principles could be discerned. These anomalous features – such as the « special » relationship between a child and its mother's brother – could be interpreted as matrilineal survivals.

The first prop was attacked by Franz Boas. Boas, Lowie reminds us, had begun as a unilinear evolutionist¹⁷. In 1888 he had defended « the current view of a necessary precedence of matrilineal forms of family organization¹⁸ ». But then came... « the new facts ». Writing of Boas, Lowie¹⁹ explained in 1946 :

[...] his critique of evolutionary schemes is the psychological equivalent of the experimentalists' critique of « the biogenetic law ». The facts did not fit the theory, hence the theory would have to be modified or discarded... L. H. Morgan teaches that the individual family is an end-product, preceded by various stages including that of a clan organization ; Morgan, Bachofen, and Tylor teach the priority of matrilineal descent. Boas found that in the interior of British Columbia clanless tribes with a family organization and a patrilineal trend adopted from coastal neighbours a matrilineal clan organization. Diffusion thus disproved the universal validity of the formula that Boas himself had been defending in 1888.

These were large claims, and they certainly served their purpose. Less well-known is the fact that they were stretched to fit the case – as Lowie himself quite evidently realised. Although « the Kwakiutl facts are very interesting », he was admitting already in 1914²⁰, « it is highly doubtful whether they have the theoretical significance ascribed to them ». The Kwakiutl *numaym* groups were not matrilineal but « mixed », the reckoning of a child's affiliation being « indeterminate », and the descent groups not being exogamous at all. « For these reasons », Lowie regrettably concluded, « the Kwakiutl conditions do not seem to furnish a favorable test case ». Note that this did nothing to prevent Lowie from subsequently downplaying his misgivings and celebrating the Kwakiutl case as the crucial disproof of Morgan and Engels. Summing up the incident, Marvin Harris concludes that the « extreme interest » of Boas' handling of his material²¹

[...] stems from the fashion in which he and his students seized upon this case to destroy the supposed universal tendency for patrilineality to follow matrilineality and at the same time to discredit the entire historical determinist position.

On the basis of this one drastically deficient case, there gradually diffused out of Schermerhorn Hall at Columbia, through lecture, word of mouth, article and text, the unquestioned dogma that Boas had proved that it was just as likely that patrilineality succeeded matrilineality as the reverse.

Harris²² describes how, following Boas' sensational claims, a stampede against Morgan and his followers was let loose. When the Kwakiutl case was cited in the general textbook that Boas had edited, Gladys Reichard referred to it as « more convincing » than the other possible instance among the Trobrianders (« also » – as Harris puts it – « highly dubious, however ») because « the details of its cause are more positive »²³. Boas' speculations concerning a patrilineal to matrilineal transition among the Kwakiutl actually gave rise to an attempt to show that this sequence was the normal one. Thus, John Swanton²⁴ tried to demonstrate that in America it was precisely the matrilineally organized tribes, such as the Haida of the Northwest, the Zuni and Hopi among the Pueblos, and the Creek and Natchez of the Southeast, who were the culturally most « advanced » peoples, while the groups that were organized on a patrilineal and a bilateral basis were deemed « primitive ». Lowie, Kroeber and Goldenweiser²⁵ all eagerly seized upon this idea. As Murdock has shown²⁶ :

This inverted evolutionistic scheme of a bilateral-patrilineal-matrilineal succession in the forms of social organization became an established dogma in American anthropology.

To see how the « matrilineal priority » hypothesis appeared within the perspective of later twentieth century anthropology, it may be useful to turn to the words of G. P. Murdock. During the latter half of the nineteenth century, he writes²⁷, authorities such as Bachofen, Lubbock, McLennan, Morgan, Spencer and Tylor (i. e. virtually all those who shaped anthropological opinion for nearly fifty years) were generally agreed that the matrilineal clan was the original form of human social organization, that this form of society gave way to patrilineal and patriarchal institutions as the male sex gradually achieved a position of dominance, and that the emergence of bilateral (« family » in the European sense) kinship and the nuclear family marked a relatively late phase of social evolution. « The hypothesis of the priority of the matrilineate », comments Murdock²⁸.

[...] was buttressed with a number of extremely plausible arguments – the presumed ignorance of physical paternity in primitive times, the biological inevitability of the association of mother and child, the alleged non-inclusion of the father in the family under early nomadic conditions, the large number of apparent survivals of matrilineal customs in patrilineal societies and the rarity of comparable patrilineal traits among matrilineal peoples, the relative cultural backwardness of matrilineal as compared with patrilineal societies, and the complete lack of historically attested cases of a transition from patrilineal to matrilineal institutions.

Murdock continues²⁹ :

The most secure prop of the evolutionist theory of matrilineal priority, that which later anthropologists have had the greatest difficulty in removing, is the complete lack of historically attested, or even inferentially probable, cases of a direct transition from patrilineal to matrilineal descent. No such case has ever been encountered [...]

The author adds :

[...] the explanation turns out to be simple. There are no recorded cases of such a transition because it cannot occur [...] the direct transition from patrilineal to matrilineal descent is impossible.

But be this as it may, the nineteenth century believers in the priority of matrilineality appeared to be on very firm ground :

So logical, so closely reasoned, and so apparently in accord with all known facts was this hypothesis that from its pioneer formulation by Bachofen in 1861 to nearly the end of the nineteenth century it was accepted by social scientists practically without

exception³⁰.

Nonetheless, according to Murdock³¹, the Bachofen-Morgan evolutionary schema remains impossible to defend :

The expansion of ethnographic knowledge and the resulting recognition of the role of diffusion in culture change made these unilinear theories increasingly suspect during the early decades of the twentieth century, and they are now, of course, universally discredited by competent scholars.

For his part, Marvin Harris simply writes that matriarchies³² :

[...] were actually postulated as the primordial condition of mankind by various theoreticians who lived in the nineteenth century. Friedrich Engels, for example, who got his ideas from the American anthropologist Lewis Henry Morgan, believed that modern societies had passed through a matriarchal phase during which descent was reckoned exclusively in the female line and women were politically dominant over men. Many modern-day women's liberationists continue to believe in this myth [...]

III. Radcliffe-Brown and the case of the mother's brother

The avunculate is the peculiar relationship which exists in many African societies between a man and his sister's son. Such a special relationship is not a universal feature of human kinship as such. Obviously, then, it cannot be explained by reference to any assumed universal sociological principle. It is a characteristic of certain kinship systems but not others, and it is the differences which need to be explained.

The avunculate in this way differs from the mother-son relationship. We feel no special need to explain the existence of a relationship linking a mother and her child. On biological grounds we expect some kind of unity – commensal and so on – between a woman and her young son under normal conditions in virtually any conceivable society. But we do not universally expect the woman's adult brother to be included in this relationship. In modern western societies, in fact, a man is *not* united in any special way with his sister's son. It is this which, even at the most superficial and immediate level, makes us feel the need to « explain » the special features of avunculate when we encounter them.

Let us follow Radcliffe-Brown in assuming that, in the tension or choice between matriliney and patriliney in any society, the point at issue is really this : « To whom does the child belong ?³³ » If it belongs to the mother's kin, descent is matrilineal. If it belongs to the father's group, descent is patrilineal. As far as the abstract question of « descent » is concerned, that is all. However, we may further follow Radcliffe-Brown in assuming that in practice, as people actually implement their descent-rule, they find themselves forced to do other things as well³⁴.

For example, to the extent that the mother-child group forms an indivisible unit, the group possessing the child must include the mother as well. Robin Fox's words seem relevant here : « The irreducible and elementary social grouping is surely the mother and her children. Whatever else happens, this unit has to survive for the species to survive [...] Whether or not father can be persuaded to stay at home is another matter³⁵ ». We would normally expect a mother to be grouped in some way with her young or dependent child.

In patrilineal systems, not only children but their mothers, too, are in effect incorporated into the descent group of the husband/father. « In effect », because at first, of course, the exogamy rule prevents women from being members of

their husbands' kin-group. Patriliney requires a wife to change her descent group membership. Matriliney requires neither wives nor husbands to make such a change. This enables us to understand the fundamental social contrasts between patrilineal systems and matrilineal ones. Radcliffe-Brown brings out the contrast with his usual precision :

The contrast between father-right and mother-right is one of two types of marriage. A woman is by birth a member of a sibling group ; strong social bonds unite her to her brothers and sisters. By marriage she enters into some sort of relation with her husband. To provide a stable structure there has to be some sort of institutional accommodation of the possibly conflicting claims and loyalties, as between a woman's husband and her brothers and sisters. There are possible two extreme and opposite solutions, those of father-right and mother-right, and an indefinite number of compromises.

In the solution provided by mother-right the sibling group is taken as the most important and permanent unit in social structure. Brothers and sisters remain united, sharing their property, and living together in one domestic group. In marriage the group retains complete possession of a woman ; her husband acquires no legal rights at all or a bare minimum... Rights of possession over children therefore rest with the mother and her brothers and sisters. It is these persons to whom the child must go for every kind of aid and comfort, and it is they who are entitled to exercise control or discipline over the child [...]

The solution offered by father-right is opposite. Possession of a wife, and therefore of the children of her body, are surrendered by marriage to her husband and his kin [...] The mother's kin, her brothers and sisters, in this kind of marriage, have no rights over the children, who, in turn, have no rights over them. The jural bonds between a woman and her siblings are severed by her marriage³⁶.

In other words, matriliney is inseparable from brother-sister unity, while patriliney contradicts it. Insofar as father-right prevails, children are incorporated, along with the mother, in their father's descent-group, divorcing the children's mother from her brother. As Radcliffe-Brown puts it in the passage just quoted : « The jural bonds between a woman and her siblings are severed by her marriage. »

This is why the avunculate appears puzzling when encountered in association with patriliney. Under matriliney, there is no such puzzle. Brothers are united with their sisters. The bond between these brothers and their sisters' children follows naturally. In a patrilineal system, on the other hand, women are incorporated, along with their children, into their husbands' descent-groups. If, despite this, children are considered to belong in some sense to their mother's brothers, we are faced with a theoretical challenge.

All of this seems fairly straightforward until we come to Radcliffe Brown's celebrated 1924 essay, *The Mother's Brother in South Africa*. As if unaware of his writings on the subject elsewhere, Radcliffe-Brown found it necessary to deny any logical connection between brother-sister unity and matriliney. His polemical target was Henri Junod's (1912) argument that the avunculate among the Thonga of South Africa was a matrilineal feature of an otherwise largely patrilineally-organized kinship system, suggesting that these people once possessed matrilineal clans.

To Junod, it seemed obvious that the strong husband-wife bonds of the present-day Thonga owed their origin to the relatively recent lobola system. Instead of having to visit his wife in her village and work for her and her kin, a husband with sufficient cattle could be permitted to remove his bride and take her home with him, in effect exchanging her and any future offspring for the cattle. In this way, her links with her natal village and hence with her brothers were

weakened. Nevertheless, it was the indigenous view that despite lobola, the « true » ties of a woman remained with her natal village, and that this applied also to her children. It was everywhere accepted that it was « only the cattle » which took a woman and her children from her brothers and other natal kin.

In religious sentiment, the child's bond with its mother's relatives was conceived as fundamental, while that with the father and his relatives was « only on account of the oxen » :

In fact, as Mankehulu said in his picturesque language, « as regards sacrifices (timhamba), the mother's relatives mostly perform them. They are the stem. My father is the stem on account of the oxen, my mother is the true stem ; she is the god ; she makes me grow. Should she die when I am an infant, I will not live. At the village of my mother it is at the god's (*ka mamana hi ko psikwembyen*)³⁷ ».

If matriliney allocates children to the mother's kin, then in religious sentiment, this principle was still very much alive among the Thonga of Junod's time. But matriliney remained operative not only in this context of sentiment. It was also present as a constant threat motivating men to keep up the lobola system. Husbands had no automatic rights in their own offspring. On the contrary, men felt that they could acquire such rights only through considerable sacrifice – by making substantial payments to their wives' kin. Father-right clearly rested on the lobola. Wherever a woman had not been « paid for », her child would bear her lineage name and would continue to live in the mother's brother's village. Whenever Junod discussed with men the possibility of government legislation prohibiting lobola payments they would reply : « Who will guarantee to us the possession of our children if lobola is suppressed ?³⁸ »

Matrilineal principles also asserted themselves in rights of property inheritance. When a man died and his possessions were inherited, his sister's sons had first choice. Obviously, this right could hardly have been fully exercised without undermining the entire principle of patrilineal inheritance. If lobola had been paid by the man to his wife's relatives, then his own offspring were his legal heirs. The contradiction was overcome by a customary stipulation that the « true » (matrilineal) heirs should voluntarily give way to the « legal » (patrilineal) ones. The sister's sons, according to the formulation of the people themselves, were the original heirs, but they allowed the deceased's sons first choice, and then made their own choice of implements themselves. « In this way », wrote Junod,

ba nyiketa pfindla, they give over the inheritance to the legal heirs This is a most vivid representation of a right which no longer exists, having in fact become obsolete, but which asserts itself however in virtue of an old custom³⁹.

There is no need here to multiply examples. Plainly, this kinship system embodied no consistent logic. In many respects it was a system of double unilineal descent, even if the matrilineal descent groups were not property-owning lineages. Murdock summarizes the evidence that the Thonga were indeed in the throes of transition from matriliney to patriliney, just as Junod originally claimed:

1. Avunculocal residence is customary in childhood. Boys and girls, as soon as they are weaned, go to live in the village of their maternal uncle and remain there for several years, in the case of girls sometimes until they are fully grown. (This is normal in matrilineal societies with avunculocal residence, e. g. the central and western tribes of the Central Bantu).
2. When a man has no patrilineal heirs to carry on his line, he may require one of his sisters to remain in his settlement. Her children reside in his house, and the males

continue his lineage and clan. (This re-capitulates the household pattern of the matrilineal eastern Central Bantu).

3. If a man has no immediate patrilineal heirs, his sister's sons inherit his property in preference to remoter patrilineal kinsmen. Even when he has patrilineal heirs, his sisters' sons can claim certain items from his estate, e. g. his spears. (Inheritance by sisters' sons is normal in matrilineal societies.)

4. A maternal uncle has a right to a definite share in the bride-price received for a sister's daughter. (He can ordinarily claim most of it in a matrilineal society.)

5. The officiant at all sacrifices in a man's life-crisis ceremonies is his mother's brother. (In a matrilineal society this duty naturally falls to a man's maternal uncle as the head of his lineage⁴⁰.)

Murdock argues that all the Central Bantu tribes must once have been matrilineal. A number of them, he writes, still are, and these are the ones that possess few cattle. In his view, the transition to patriliney was in many cases accomplished only recently, as a direct result of the introduction of cattle. If this view is correct, then the severing of the bond between a man and his sister's children was the final result of a process by which women were separated from their natal kin by means of the lobola system. Each increase in cattle ownership within a particular tribe would have made possible a raising of the level of lobola payments, hence a corresponding intensification of the rights of husbands (and husbands' kin) over their wives. A concise formulation of this evolutionary argument has been put forward by Harold Schneider :

In Africa the most thoroughgoing patrilineal societies seem to be those in which livestock, particularly cattle, are an important asset – that is, where the number of cattle per person is about one or more. Where there is no asset that can be transferred for compensation, we find matrilineal-matrilocal systems. As compensation increases the husband is allowed to remove his wife to his father's home, so that the system becomes matrilineal-patrilocal, but with the male children moving to live with their mother's brother when they marry (avunculocal residence), since mother's brother owns them despite the patrilocality of the marriage. As compensation reaches a higher level the system shifts to patrilineal-patrilocal, and this increases in intensity as the amount paid increases until at the other extreme one gets patrilineal-patrilocal systems in which there is no divorce⁴¹.

At every intermediate stage in this process, there is always something left of the old brother-sister unity, but where it retains its force this can only be *despite* the increasing patrilocality and patrilineality of the current pattern of residence and descent.

In essential conformity with Morgan's scheme, the rise of alienable property is nowadays widely recognised to be the crucial factor cementing marital bonds at the expense of brother-sister solidarity throughout much of sub-Saharan Africa. As David Aberle put it, « the cow is the enemy of matriliney⁴². » Following in the footsteps of Murdock's cross-cultural comparative work, Mace and Holden's⁴³ (1999) phylogenetically controlled analysis confirmed a negative correlation between African matriliney and cattle owning. In their most recent analysis of matriliney as daughter-biased investment, Holden, Sear, and Mace comment, « the two factors Morgan identified, heritable wealth and paternity uncertainty, remain central to our understanding of variation in matriliney and patriliney in human social organisation⁴⁴. »

At this point, we may turn to the argument put forward by Radcliffe-Brown in his celebrated essay on the mother's brother. How does he account for the

peculiar unity between brothers and sisters which exists even in some of the most strongly patrilineal societies in Africa ?

Unfortunately, we cannot answer this question because – surprising as it may seem – Radcliffe-Brown offers no explanation. Or rather, he adopts two mutually inconsistent strategies. When discussing matriliney, he identifies this descent rule as inseparable from brother-sister unity. On the other hand, in his essay on the mother's brother in South Africa, his whole purpose is to demonstrate that an explanation can be found which does not involve matriliney. To what, then, should we ascribe the strong bond found in certain societies between brother and sister ? Radcliffe-Brown does not ascribe it to anything. Instead, he treats it as a metaphysical principle – something which, precisely because of its wide distribution, requires no explanation. Brother-sister unity is presented as a particular expression of the « equivalence of siblings », a « principle » which for unexplained sociological reasons is « most commonly adopted in primitive society⁴⁵ ».

Elsewhere, Radcliffe-Brown writes⁴⁶ : « The unit of structure everywhere seems to be the group of full siblings – brothers and sisters. » Fortes writes that this principle is

one of the few generalizations in kinship theory that, in my opinion, enshrines a discovery worthy to be placed side by side with Morgan's discovery of classificatory kinship ; and, like Morgan's, it has been repeatedly validated and has opened up lines of inquiry not previously foreseen⁴⁷.

Needham, however, observes that this principle has obviously not been « repeatedly validated » – in some societies the brother-sister group is the unit of structure, while in others it is not⁴⁸. Schneider notes that the « interdependence of brother and sister » *is* characteristic of matrilineal descent groups but is *not* characteristic of patrilineal ones⁴⁹. And Murdock attacks the whole idea of using timeless « principles » to explain social facts⁵⁰ :

In the first place, the alleged principles are mere verbalizations reified into causal forces. In the second, such concepts as « equivalence of brothers » and « necessity for social integration » contain no statements of the relationships between phenomena under varying conditions, and thus lie at the opposite extreme from genuine scientific laws. Thirdly, being unitary in their nature, they provide no basis for interpreting cultural differences ; they should produce the same effects everywhere.

The nub of Radcliffe-Brown's claim is that the avunculate reflects brother-sister solidarity but not matrilineal descent. We are informed, to begin with, that « there are certain fundamental principles or tendencies which appear in all societies, or in all those of a certain type⁵¹ ». As an example of a « certain type », Radcliffe-Brown cites « primitive society », in which, we are told, « there is a strongly marked tendency to merge the individual in the group to which he or she belongs⁵² ». Which precise « group » Radcliffe-Brown has in mind is not very clear. Is it the husband-wife group, the territorial group, the group of full siblings or what ? Radcliffe-Brown apparently means the brother-sister group. Skillfully avoiding any mention of matriliney, he argues that in societies in which « the classificatory system of kinship reaches a high degree of development », husbands and wives are grouped apart, while brothers and sisters are grouped together⁵³. The crux of the argument is that in these « certain societies » wives are not « merged » in the groups of their husbands, but are « merged » in those of their brothers.

Radcliffe-Brown views the avunculate primarily as a reflection of « sentiment ». He shows little interest in the rich institutional details provided by Junod : avunculocal residence in childhood, rights of inheritance, rights in bride-price, initiation, sacrifice and so forth. For him, the basic feature is the sentimental « indulgence » towards the sister's son on the part of the maternal uncle, together with certain corresponding sentiments on the part of the child. The central question, from this psychological perspective, is not « How or why is the institution maintained ? » but « How do the child's sentiments come to be formed ? » Radcliffe-Brown answers by (a) describing certain aspects of the social structure of so-called « primitive » societies and (b) showing that there is some fit with the growing child's sentiments.

In patrilineal societies where the avunculate is found, authority over children is in the hands of fathers, while sibling-unity remains strong. Consequently, argues Radcliffe-Brown, paternal authority is to some extent shared between fathers and their mature siblings, male and female. « Tenderness and indulgence », on the other hand, are to be expected from a child's mother, hence also from her male and female siblings. All this predetermines the formation of sentiments in the child's mind :

The pattern of behaviour towards the mother... is extended with suitable modifications to the mother's sister and to the mother's brother, then to the group of maternal kindred as a whole, and finally to the maternal gods, the ancestors of the mother's group. In the same way the pattern of behaviour towards the father is extended to the father's brothers and sisters, and to the whole of the father's group... and finally to the paternal gods⁵⁴.

We are asked to accept this as an explanation for the fact that children after weaning live with their maternal uncles, see their maternal relatives as the « true stem », belong to their mother's group if the lobola has not been paid and so on. Radcliffe-Brown's aim is not just to modify or improve Junod's interpretation but to discredit it and replace it with an entirely contrasting explanation. In his own words :

The point of the paper on the mother's brother may be said to be to contrast with the explanation by pseudo-history the interpretation of the institution to which it refers as having a function in a kinship system with a certain type of structure⁵⁵.

Murdock comments :

The indicated contrast is clear. In the eyes of the present writer, however, it takes the form of an opposition between sound historical scholarship and untrammelled sociological speculation⁵⁶.

A more generous verdict would be that Radcliffe-Brown inadvertently takes us back to matriliney by another route. In particular, his argument helps explain the difference between (a) the avunculate in a society which has not shifted to patriliney and (b) the avunculate following this transition. Junod himself was unable to explain why, among the Thonga, the avunculate so one-sidedly favoured the child. A boy could « do as he pleased with his maternal uncle, eating his food, flirting with his wife or wives and wandering in and out of his house without even asking, while the uncle himself had virtually no reciprocal rights in or authority over the boy⁵⁷ ». Such indulgence is not necessarily typical of matrilineal societies, in which the relationship is often much more balanced, a man usually having definite authority over his sister's son.

So the avunculate in a patrilineal society cannot simply be treated as a survival. The institution is transformed in the process, and no longer serves quite

the same functions. Radcliffe-Brown's contrast between « authority » and « indulgence » in this context is not without relevance. Let us assume that despite the loss of the mother's brother's authority, the child's relationship to its maternal uncle remains in many respects intact following the transition. It is easy to imagine why a newly assertive father might be less tolerant of the mother's brother's residual rights in a boy than of that same child's reciprocal rights in his maternal uncle. If this is accepted, we can perhaps understand why the transition to patriliney would soften the avunculate – removing from it the element of adult authority – while leaving the child's rights in other respects intact.

Among the Thonga, as Radcliffe-Brown points out, the father's sister is a relative very much to be respected and perhaps even feared. Why is this pattern so typical of societies in which the mother's brother has become a notably indulgent figure ? In a transitional society, a woman, despite patrilocal marriage, will still remain closely bound to her brother. Under the earlier, matrilineal, system, she would have been bound to him still more closely, but this would not have given her any authority over her brother's child. Once father-right has been consolidated, however, a woman's brother will have begun to exercise authority over his own biological offspring. To the extent that a woman remains in some sense her brother's equivalent, she must inevitably share in exercising such paternal authority.

IV. Lévi-Strauss, Leach and Goody

Is it fathers who give away their daughters in marriage, or brothers who give away their sisters ? This depends upon the descent rule. In real life, of course, both relatives may be involved, but where fathers traditionally « give away » their daughters, this can only be because these female relatives are in some sense theirs to give. If young women are considered the responsibility of their brothers and mothers' brothers, then as they are married it will not be their fathers but their male kin who give them away. Whether a bride is given away as a daughter or as a sister has therefore some importance in any discussion of the avunculate.

Lévi-Strauss obliterates this distinction by writing of marriage as a system in which one man gives another man « a daughter or sister ». This would not matter much, perhaps, were it not for the fact that the essay in which Lévi-Strauss does this is the place where he ventures his own explanation for the avunculate. For Lévi-Strauss, the avunculate just exists. Men simply do have sisters, and they are obliged to surrender them in marriage. Hence the avunculate.

Lévi-Strauss fails to explain why the avunculate predominates in certain African and other tribal societies while playing little role in, say, the industrialised west. We are simply told that the relation between a man and his sister is one aspect of the « primitive and irreducible character of the basic unit of kinship », while this itself « is actually a direct result of the universal presence of an incest taboo⁵⁸ ». Then follows the passage in which Lévi-Strauss slips in the words « daughter or sister » quite unobtrusively and as if the difference were irrelevant:

This is really saying that in human society a man must obtain a woman from another man who gives him a daughter or sister. Thus we do not need to explain how the maternal uncle emerged in the kinship structure : he does not emerge – he is present initially. Indeed, the presence of the maternal uncle is a necessary precondition for the structure to exist⁵⁹.

This argument is patently weak. First, it can have force only where women are in fact « given away » in marriage as sisters by their brothers. Where they are given away as daughters by their fathers, then the dynamic will be quite different.

Reflecting this, we find that in many societies there is no « pivotal » or « special » relationship between a boy and his maternal uncle. Meanwhile, in other cases, a boy lives with this relative for several years, or inherits his property or his widows, or can eat or use or « steal » his property or food at any time, etc. Why this difference ? We all observe incest-taboos, so the avunculate cannot conceivably be explained as « a direct result of the universal presence » of this taboo.

Homans and Schneider adopt a very similar position⁶⁰. That is, they treat brother-sister solidarity as a sociological axiom requiring no explanation. We are told, for example, that a boy behaves towards his mother's brother in the same way as he does towards his mother « as a result of the identification of the mother with her brother, who is her protector⁶¹ [...] ». The authors are here referring to « the patrilineal complex », it should be noted. But in the « patrilineal complex » (as opposed to its matrilineal counterpart), there is evidently no reason to assume that a woman's brother must act as her protector. Where this does happen, it must be explained : it is not inherent in patriliney, the underlying dynamic of which on the contrary makes the *husband* a woman's protector *at the expense* of her brother⁶². Once again, Homans and Schneider assume as axiomatic the very thing which has to be explained, managing to dissociate matriliney from the avunculate only by adopting this polemical stratagem.

Goody⁶³ takes a woman's unity with her brother as given, but then does something for which his professional colleagues have never forgiven him. He leaves the distinct impression in the reader's mind that even in patrilineal societies, matrilineal descent and the avunculate are linked. Worse, he even uses a formulation which would seem to make of the avunculate a matrilineal « survival ». He writes, for example, that among the West African Lo Wiili, the « legitimization » of a marriage gives the husband's patriclan rights in the child of this marriage. In this way, the child is « excluded » from the mother's group. Its claim to share the property of the mother's brother or to inherit from him is thereby denied. But father-right is not consistently or rigidly applied. For one thing, a woman remains united with her brother – she continues to belong to her own patriclan, to which her brother also belongs. Moreover, her child, despite exclusion from her group, « has nevertheless a shadowy claim upon the group by virtue of his mother's position⁶⁴ ». The avunculate is in this way explained.

In a discussion of the avunculate among the Lakher of India, Leach denounces Goody's position as « only marginally different » from the « totally mistaken » nineteenth century evolutionists' « doctrine » of matrilineal priority⁶⁵. Schneider subsequently supports Leach⁶⁶ :

Leach's disquiet over such terms as « shadowy claim », « submerged rights » and « residual sibling » is not entirely misplaced, in my view... The point is that if the rights of the sister's son over bits of property held by the mother's brother, including perhaps the mother's brother's wife, whom sister's son may « inherit », are rights which are based on the consanguineal tie through his mother, then these rights are transmitted exactly as is descent group membership. These are rights, that is, that are based on descent. If this is so, the problem is whether matrilineal filiation is not in this sense a « descent rule » and so all patrilineal descent systems with matrilineal filiation are by definition double unilineal descent systems. (p. 54)

The accusation here is that Goody once again links the avunculate with a trace or survival of matriliney. But ever since Radcliffe-Brown, it has been one of the central purposes of professional anthropologists throughout the West to exorcise this spectre. To me it seems that the more these anthropologists try, the less

convincing they are. The more they try to dissociate the avunculate from matriliney, the more entangled in matriliney they become. Perhaps the time has come for anthropologists to question the usefulness of the entire enterprise on which they and their teachers have been engaged, now, for the best part of a century⁶⁷.

Notes de CHRIS KNIGHT

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- ² J. T. Lafitau, *Mœurs des sauvages américaines, comparées aux mœurs des premiers temps*, Paris, Saugrain l'aîné, 1724, 1, p. 71-72.
- ³ A. Ferguson, *An Essay on the History of Civil Society* [1767], Philadelphia, A. Finley, 1819, p. 126.
- ⁴ J. J. Bachofen, *Myth, Religion and Mother-right*. (Selected writings), New Jersey, Princeton University Press, 1973, p. 70.
- ⁵ *Ibid.*, p. 135-136.
- ⁶ *Ibid.*, p. 71.
- ⁷ J. P. McLellan, *Primitive Marriage*, Edinburgh, Adam and Charles Black, 1865, p. 123 ; quoted in S. Tax, « From Lafitau to Radcliffe-Brown », in F. Eggan (ed.), *op.cit.*, p. 445-481 ; p. 455.
- ⁸ L. H. Morgan, *Houses and House-Life of the American Aborigines*, Chicago and London, University of Chicago Press, 1881, p. 126.
- ⁹ *Ibid.*, p. 126-128.
- ¹⁰ See M. Harris, *The Rise of Anthropological Theory*, London, Routledge, 1969, p. 189.
- ¹¹ F. Engels, *The Origin of the Family, Private property and the State* [1884], New York, Pathfinder Press, 1972, p. 181-182.
- ¹² K. Marx, « Economic and philosophical manuscripts » [1844], in D. McLellan (ed.), *Karl Marx : Selected Writings*, Oxford, Oxford University Press, 2nd ed., 2000, p. 83-121 ; p. 96.
- ¹³ K. Marx and F. Engels, « The German ideology » [1846], in D. McLellan (ed.), *ibid.*, p. 175-208 ; p. 185.
- ¹⁴ L. H. Morgan, *Houses and House-Life of the American Aborigines*, *op. cit.*, p. 128.
- ¹⁵ F. Engels, *The Origin of the Family, Private Property and the State*, *op. cit.*, p. 68.
- ¹⁶ *Ibid.*, p. 75.
- ¹⁷ R. Lowie, « Evolution in cultural anthropology : a reply to Leslie White », *American Anthropologist*, 48, 1946, p. 223-233 : reprinted in *Lowie's Selected papers in Anthropology*, C. du Bois (ed.), Berkeley, University of California Press, 1960, p. 418.
- ¹⁸ F. Boas, *Race, Language and Culture*, New York, Macmillan, 1940, p. 635.
- ¹⁹ R. Lowie, « Evolution in cultural anthropology : a reply to Leslie White », *art. cit.*, p. 420.
- ²⁰ R. Lowie, « Social organization », *American Journal of Sociology*, 20, 1914, p. 68-167 ; reprinted in C. du Bois (ed.), *op. cit.*, p. 28.
- ²¹ M. Harris, *The Rise of Anthropological Theory*, *op. cit.*, p. 305.
- ²² *Loc. cit.*
- ²³ G. Reichard, « Social life », in F. Boas (ed.), *General Anthropology*, New York, Heath, 1938, p. 409-486 ; p. 425.
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- ²⁷ *Ibid.*, p. 185.
- ²⁸ *Loc. cit.*
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- ³² M. Harris, *The Rise of Anthropological Theory*, *op. cit.*, p. 85.
- ³³ A. R. Radcliffe-Brown, *Structure and Function in Primitive Society*, London, Cohen and West, 1952, p. 40.
- ³⁴ *Ibid.* p. 32-48.
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- ³⁶ A. R. Radcliffe-Brown (ed.), « Introduction », in *African Systems of Kinship and Marriage*, London, Oxford University Press, 1950, p. 77-78.
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- ³⁹ *Ibid.*, p. 257.
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- ⁴¹ H. Schneider, *Economic Man. The Anthropology of Economics*, New York, Macmillan, 1974, p. 145.
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- ⁴⁵ A. R. Radcliffe-Brown, « The mother's brother in South Africa », *South African Journal of Science*, 21, 1924, p. 542-555 ; reprinted in A. R. Radcliffe-Brown, *Structure and Function in Primitive Society*, *op. cit.*, p. 15-48 ; p. 18.
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- ⁴⁷ *Loc. cit.*
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- ⁴⁹ D. M. Schneider, « The distinctive features of matrilineal descent groups. Introduction », in D. M. Schneider and K. Gough (ed.), *Matrilineal Kinship*, Berkeley, University of California Press, 1961, p. 1-29 ; p. 11.
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- ⁵¹ A. R. Radcliffe-Brown, « The mother's brother in South Africa », *art. cit.*
- ⁵² *Ibid.*, p. 25.
- ⁵³ *Ibid.*, p. 19-20.
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- ⁵⁵ *Ibid.*, p. 14.
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- ⁵⁸ C. Lévi-Strauss, *Structural Anthropology*, 2 vol., Harmondsworth, Penguin, 1968, vol. 1, p. 46.
- ⁵⁹ *Loc. cit.*
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⁶⁴ *Ibid.*, p. 78.

⁶⁵ E. Leach, *Rethinking Anthropology*, University of London, The Athlone Press, 1961, p. 16.

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⁶⁷ In this short essay, my focus has been upon the roles played by Boas, Malinowski, Radcliffe-Brown and their mid-twentieth century successors. For current debates and in particular for the modern « grandmother hypothesis » in relation to early kinship, see C. Knight and C. Power, « Grandmothers, politics and getting back to science », in E. Volland, A. Chasiotis and W. Schiefenhövel (ed.), *Grandmotherhood : The Evolutionary Significance of the Second Half of Female Life*, New Brunswick, Rutgers University Press, 2005, p. 81-98.

Tetradic theory and the origin of human kinship systems

NICK ALLEN

Résumé

Une société ne peut perdurer dans le temps que si elle se soumet aux contraintes biologiques permettant son renouvellement. L'étude de la parenté a ceci de particulier qu'elle peut servir à rendre compte de l'origine des sociétés de petite taille : (i) parce que la parenté y possède une structure quasi logique dont on peut supposer qu'elle était présente dès l'origine et (ii) parce que le nombre des individus de la société est si restreint que les distinctions entre groupes selon la parenté sont immédiatement aussi des distinctions sociales. La question que l'on doit se poser est alors : « Quel est le système de parenté le plus simple logiquement qui permette cependant de reconnaître le caractère humain de la société ? »

On remarque que toutes les sociétés humaines essayent d'établir un lien de « recrutement » entre leurs nouveaux membres et certains prédécesseurs ou catégories de prédécesseurs en faisant usage du lien qui unit parents et enfants (lien caractérisé comme lien de parenté). Les modèles les plus simples doivent donc prendre en considération une relation horizontale (de type alliance) et une relation verticale (de type consanguinité) qui s'appuient sur une règle de prohibition de l'inceste définissant les partenaires sexuels permis ou interdits. Cette règle impose que tous les partenaires, permis ou interdits, soient discernables au moyen d'une terminologie. Pour prendre en compte les relations horizontale et verticale, la terminologie la plus simple doit donc permettre de distinguer linguistiquement dans la société des groupes d'individus des deux sexes entretenant entre eux des rapports d'alliance et de consanguinité. Quatre groupes sont nécessaires : les deux premiers (correspondant à la première génération) pratiquent l'alliance par le biais de la sexualité et produisent des enfants (correspondant à la seconde génération) qui font l'objet d'un recrutement dans les groupes différents de ceux de leurs parents des deux sexes. Il y a donc huit termes de parenté de base au moins. Les relations entre les deux ensembles de groupes sont concevables sous forme d'échange : les deux premiers groupes produisent les enfants qui appartiendront aux deux derniers qui eux-mêmes entretiennent ensuite des relations d'alliance qui reproduiront dans la descendance les appartenances aux groupes de leurs parents et ainsi de suite.

Si l'on émet une hypothèse sur l'origine historique de ce modèle impliquant quatre groupes (d'où son nom de « tétradique »), on remarque que les bandes de chasseurs-cueilleurs se retrouvent lors de rituels et que c'est là que se constituent les séparations en groupes, souvent sous forme de joutes partageant la société en deux. On peut imaginer un rituel possédant trois traits : (i) on y distingue au hasard deux groupes ; (ii) des actes sexuels dans chacun des deux groupes

font partie du rituel (relation horizontale de type protomariage) et (iii) des recrutements d'enfants sont pratiqués (relation verticale de type proto-initiation) dans lesquels les nouveaux membres ne font pas partie des groupes auxquels leurs parents appartiennent. L'histoire de la parenté humaine a-t-elle commencé avec un système tétradique de ce genre ? Certains faits ethnographiques l'attestent : c'est en Australie que l'on trouve des systèmes de parenté proches de celui décrit dans la structure tétradique. Comme l'Australie a été peuplée il y a au moins 50 000 ans, on peut en conclure que c'est antérieurement que ce système s'est mis en place, peut-être dès la sortie hors d'Afrique.

The researcher curious about the origins of human society can either start with the animal kingdom, especially the great apes, and work forward in time, or start with anthropological materials and work backwards ; for insofar as materials exist contemporary with the origins, they can be interpreted only by adopting one or other of these two perspectives. Without in the least rejecting the primatological perspective the present paper confines itself to the second or retrospective approach.

The origins of society and the origins of kinship systems are not identical questions, and the relation between the two concepts will be discussed in more detail below. For the moment, suffice it to say, firstly, that in the context of small-scale societies the two concepts are very closely linked, and secondly that, for anyone interested in extrapolating backwards in time, kinship systems offer a particularly attractive domain. This is because they lend themselves to the construction of formal and logical models which are less arbitrary in their assumptions and less remote from reality than would be the case with, say, economic, political or religious systems. The quasi-mathematical aspect of kinship, which has much less to do with statistics than with the logic of relations, renders it possible to answer the question « what is the simplest logically possible kinship system ? » The same question can of course be posed about other societal systems, but there the chances of answering it with convincing reasons are smaller. Of course too, a gap exists between the propositions that X is the simplest kinship system of the general type one expects to find among humans, and that X is the original human kinship system. Nevertheless, until counter-evidence or counter-arguments are advanced, the equation of simplest and earliest remains the most economical hypothesis.

Within the arts and social sciences it is generally held that a problem of any complexity is best approached via its history ; but this is much less true of the sciences, and kinship probably belongs closer to the scientific than to the artistic pole of the anthropological continuum. In any case, I doubt whether, in the present context, an attempt to introduce the views of previous writers would be helpful¹.

I. Biological and social continuity

An animal collectivity cannot endure unless the members who die are replaced by new members who are born. In the simplest models the new members are the offspring of the old members, rather than outsiders ; thus the continuity of the group results from birth, copulation and death. These biological facts are the raw

materials that humans have elaborated into kinship systems by establishing more or less explicit rules. The rules have been expressed and transmitted in language, and may be translated into practice with varying degrees of rigour, but the simplest kinship system is the one with the simplest rules.

What sorts of rule does a kinship system need in order to be recognisably human? Probably the first such rule that comes to mind is one governing copulation – one prohibiting incest by dividing sexual partners into the classes of the forbidden and the permitted. For the purposes of the present paper we need not specify more closely the rules governing sexual partnership : the relation between biological factors (such as the prolonged helplessness of human infants) and pair bonding ; between such bonding and the human institution of marriage ; or the different forms of sexual partnership such as monogamy (serial or lifelong), plural marriage and concubinage. We need only say that a recognisably human society needs some sort of marriage rule.

It may seem that the simplest society could function with a marriage rule alone. Indeed, with some effort, one can imagine a group of humans among whom the marriage of parents produces children, but these children have no socially recognised links with previous generations ; the biological condition of child is associated with no filial role. New members of society would be simply that, or (a slightly less extreme case) they would be given some more specific social identity but on grounds having nothing to do with their parentage. It would be as if *all* children were foundlings. Such a group could indeed endure, but is it recognisable as a human society, let alone as the sort of human society one would expect in very early human history ? The societies we know of have always made some attempt to link new members of society with particular predecessors or categories of predecessors, and have always done so by making some use of the biological link between parents and children. Therefore the rules of our simplest imaginable human society will need to cover not only « horizontal » relations (marriage) but also « vertical » relations, for which « recruitment » is a conveniently general term.

To introduce the notion of kinship systems it is convenient to separate the horizontal and vertical rules, treating them as distinct dimensions (they are often alluded to under labels such as alliance and descent or affinity and consanguinity). However the two sorts of rule are not necessarily independent, and in the model on which we shall focus they form a single complex such that neither can be fully expressed without taking account of the other.

II. Kinship terminologies

To carry the argument further, I move from rules to language. A distinction is needed straightaway between the « target » language used by participants in a kinship system and the metalanguage used by analysts to discuss the target language. The analysts themselves of course participate in their own native kinship system, and the everyday language they use in that capacity is most unlikely to constitute a satisfactory metalanguage. At the very least it needs to be supplemented by a certain number of technical terms and devices.

For a start we need precise ways of talking about how target languages lexicalise the domain of relatives. Every known language contains a kinship terminology, a set of single words distinguishing different types of relative, but the distinctions and assimilations operated by the terminology vary widely

between languages across space and time². To analyse the variation we need symbols for the elementary or primary relations, and the following is one of the conventional notations.

sex-neutral		sex-specifying	
parents	P	F	M
siblings	G	B	Z
children	C	S	D
spouses	E	W	H

A relation of course usually links two things – two poles – which are not necessarily commutable. In the case of relatives, the two poles are the individual (ego) and the person whom he has as relative (alter), and one has to conceptualise the relation as starting from ego. Consequently, since we read from left to right, it makes sense to read the symbol F (for father) as if it has immediately on its left an invisible symbol for ego. Usually this virtual ego need not be written, but it becomes relevant in certain contexts : for instance, the relation of male ego to his father can usefully be distinguished from that of female ego to hers by writing $mF \neq fF$ ³. Moreover, when we move from the primary relatives, those indicated by a single capital letter, to remoter ones (secondary, tertiary, etc.), the additional links are shown to the right, i. e. further from ego – whatever the grammar of the genitive in the analyst's native language.

Using this notation, we can make some elementary empirical observations about kinship terminologies, which will give us an idea of the direction in which simplification can be sought. When seen in world-wide perspective, French, like other European terminologies, is not very typical. Thus, where French *oncle* assimilates the two sorts of parent's brother (it makes the equation $FB = MB$), most languages discriminate them ($FB \neq MB$). Very commonly too, languages assimilate same-sex siblings (ssG), so that $F = FB$, $M = MZ$, and they do not confine this to cases where the sibling link comes at the end of a chain. Thus one often finds $B = FBS = MZS$, $Z = FBD = MZD$, and so on. Expressed in sex-neutral symbols this amounts to $G = PssGC$: the terms for siblings also cover parallel cousins. However, the coverage is not limited to first cousins, since the G within the formula PssGC can itself be replaced by PssGC, and the replacement can be repeated as often as one likes. The terms covering this class of relatives can then be rendered in the metalanguage as « classificatory siblings », and if the assimilation is carried through consistently, the terminology as a whole can also be called classificatory.

Among such classificatory terminologies a good number, represented in all continents apart from Europe, have a separate term or terms assimilating paternal and maternal cross-cousins ($FZD = MBD$, or in short, PosGC, where os stands for opposite-sex), and in that case the society sometimes has a positive marriage rule, prescribing that every ego should marry a cross-cousin. Because the terminology is classificatory, the prescription does not necessarily concern a *first* cross-cousin. The logical and empirical concomitants of such kinship systems are described in text-books, and are difficult to grasp without the aid of genealogical diagrams. But let us move on.

Occasionally, and particularly in terminologies which accord with the simplest cross-cousin marriage diagrams, one also finds a tendency towards the assimilation of alternate genealogical levels. In attested terminologies such assimilation seems always to be partial or patchy, but it would be logically simpler

if it were total and consistent. The result is again best grasped with the aid of a genealogical diagram. Let us emphasise that we have now left the domain of what has been reported by ethnographers or historical linguists⁴.

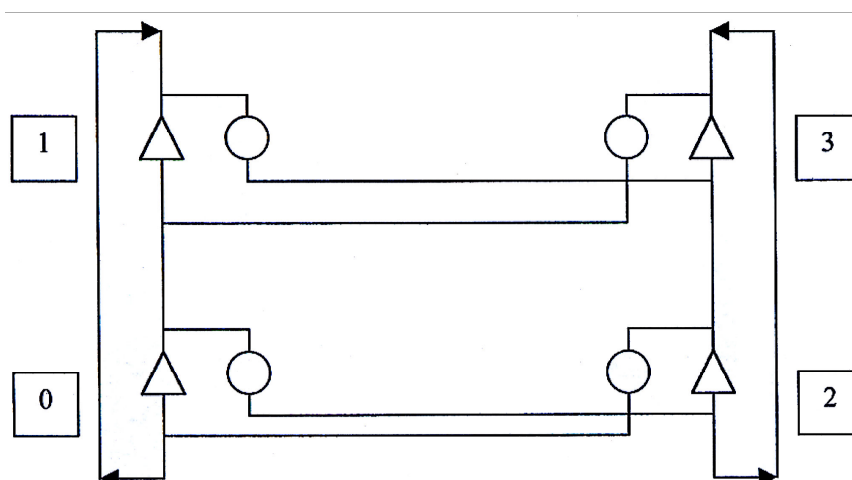


Fig. 1. Genealogical diagram for focal tetradic society.

Although the diagram uses standard genealogical conventions, it needs to be read quite differently from a family tree, where a particular triangle represents a single named individual with a datable life-span. Clearly the triangle here represents something altogether more abstract and more general. In part this is because we are dealing with a classificatory kinship system, where same-sex siblings are assimilated. Thus each triangle represents a male together with his brothers (as well as other relatives), and the same applies to the filled-in triangle representing ego. But since the triangle in quadrant 1 represents not only ego's father but also the latter's brothers, FBS too is covered by ego's triangle. One simply reads up the ordinary filiation line, then down it again.

Since we are accustomed to diagrams in which one generation is followed by another as the eye moves down the page, the cycling filiation line round the outside may initially be disconcerting. Let us concentrate first, not on interpreting its essence, but simply on how to read it. Followed from ego downwards and outwards, it leads round to ego's children, who are represented by the triangle and circle in quadrant 1 : one might supply the line with an arrow pointing in this direction, to indicate that the eye is moving in the direction in which time advances. However, the line can just as well be read in the opposite direction : trying to follow the filiation upwards from ego's father and backwards in time, one is carried round the outside and back to ego's triangle, which thus also covers ego's FF. Whether one traces it upwards or downwards, the male line simply shuttles back and forth between the triangles in quadrants 0 and 1. It is easy to see that the circle in 2 covers not only ego's wife (the relation resulting from marriage), but also his FZD and MBD (a relation that exists even before his marriage). It may be less obvious to the eye, but one will find that ego's father, like all the other males, also has as wife a PosGD. As an exercise, to increase familiarity with the notation, it may be worth working out which symbol covers FMBDSSWDH (answer in footnote⁵). The point of the exercise is to emphasise that, provided the marriage and recruitment rules are obeyed, no genealogical chain, however long, can carry one outside the diagram ; all possible relatives are accommodated within it.

Because ego can reach alter via an indefinite, indeed infinite number of genealogical paths, from one point of view the domain of relatives that the model organises is not the same as the domain of relatives as it exists in contemporary France. But from another point of view, this difference disappears. Ego's relatives are those with whom he is linked by a relation or series of relations of siblingship, filiation and marriage, and the rules of marriage and recruitment that create the multiple paths to a single alter make no difference to the definition.

It was kinship terminologies that led us to the diagram, and we can now return to them. We have already mentioned kinship systems that prescribe cross-cousin marriage and the genealogical diagrams that relate to such systems. In the simplest cases, a single symbol in the diagram corresponds to a single kinship term. Applying the same principle to our eight-symbol diagram gives us a theoretical eight-term terminology covering all possible relatives – past, present or future. The question arises whether yet further simplification is possible. One can easily postulate a single base lexeme per quadrant and distinguish the sexes by either two suffixes or a single one for the sex that is "marked" (in the linguists' sense) ; but I am uncertain whether a human society could operate with four sex-neutral kinship terms, entrusting the determination of sex, in those contexts where it was relevant, to other methods, linguistic or extra-linguistic.

III. The diagram

Let us call the diagram the « focal tetradic diagram » – tetradic because it consists of four quadrants, focal because, out of the various four-quadrant genealogical diagrams one can draw, it is on this one that, for various reasons, I focus the discussion (the most obvious variant has each quadrant consisting of a husband-wife pair, rather than a brother-sister pair). We are now in a better position to see what it is that the diagram represents. It is not enough to say that the symbols it contains represent classificatory relatives, so that, say, the circle in 3 represents a classificatory mother. The statement is true, but more fundamentally and more abstractly, what the circle represents is one component in a structure. Let us say that a structure is a totality whose components are linked by relations which remain constant when the content of the components changes (whether over time, or because of a shift of context). Over time, for ego, the individuals covered by the « classificatory mother » circle change as old ones die off and young ones are born, but the relation with the other components remains unchanged. However, we must also take account of change of context. Relatives are people, kinship terms are words – entries in a dictionary ; but when we shift from the one domain to the other, the diagram loses none of its relevance. The circle now represents one word among the (let us say) eight that make up the hypothetical terminology, and the diagram shows its semantic relations to the others. One could also say, putting the emphasis solely on meanings, that the circle represents one slot within the semantic field of relatives.

What is to be said about the rules that give order to this field ? Firstly, the rules eliminate marriage with any primary relative. Ego cannot marry his mother in quadrant 3, his daughter in 1, or his sister in 0 – or indeed their classificatory equivalents ; he can only marry within quadrant 2. Secondly, we need to consider the nature of the rules. We introduced the marriage rule by means of the empirical observation that societies quite often require ego to marry a classificatory PosGD, and we then introduced the more or less non-empirical rule

prescribing systematic assimilation of alternate genealogical levels. Applied to the male ego of our focal model, this recruitment rule entails his assimilation to FF and SS, not to mention remoter relatives, such as those in the male line from more distant even-numbered generations ; in sex-neutral language the assimilation is to ssPssP and to ssCscC. Applied to ego's wife, the rule entails her assimilation to ego's FM and DD (among others), and the model allows ego to marry these relatives, either in the minimal or in the classificatory sense (note that, being secondary relatives, FM and DD are closer than PosGD, who is tertiary). In other words the tetradic marriage rule cannot be properly stated without taking account of the recruitment rule, and the converse is also true : the category to which ego is assimilated includes not only FF but also, because of the marriage rule, MMB (osPssPosG), represented by the same triangle. The marriage and recruitment rules are interwoven., or more precisely, they are the horizontal and vertical implications of the unitary structure. Moreover, although it seems natural to introduce the structure as the *product* of rules, one can also reverse the perspective : the structure generates the rules, which are merely its expression within a jural idiom.

So far we have been talking of a kinship system envisaged as composed of ego and his relatives. However, it is common enough in small-scale societies, with total populations numbered in hundreds, for every member of society to be regarded as a relative. For simplicity of exposition let us assume that the society is demographically isolated. If the domain of relatives is co-extensive with society, the discriminations between relatives become the divisions of society, and the sets of relatives assimilated to form the various kinship categories become the groups that compose society. The metalanguage moves away from egocentric analytical terms to a different set which can be called sociocentric.

The focal tetradic diagram now becomes a representation of the social structure of a hypothetical focal tetradic society. The quadrants can now be called by the standard technical term « sections », and we can also give a name to the components of society that consist of pairs of sections. The half of society that consists of the two sections of a given parity (even or odd) is a generation moiety. Thus ego's generation moiety is made up of sections 0 + 2, 1 + 3 constituting its counterpart. For male ego, the 0 + 1 pair of sections constitutes a patrimoiety, leaving the 2 + 3 pair to form another one ; and 0 + 3 constitutes a matrimoiety, as does 2 + 1. We are here concerned as analysts with the shift from egocentric to sociocentric description of the tetradic structure, and it is immaterial whether the moieties are named by members of the hypothetical society. On the other hand, it is important to see that the matrimoieties are every bit as *real* as the patrimoieties. If the latter are more obvious to the eye, this is an artefact of the explicit male bias in the drawing of the diagram.

To appreciate this, suppose for a moment that we reverse the conventional values of triangle and circle, so that triangle signifies female (this amounts to just the same as replacing triangles by circles and vice versa). The diagram will now have a female bias : female ego's mother and children will be in quadrant 1, her father in quadrant 3, and the matrimoieties will now appear more salient than patrimoieties. But the structure – the set of relations between elements – remains unaffected by the changed interpretation of the symbols. The structure is in fact symmetrical between the sexes, but the conventional notation makes it hard to devise a single diagram that shows this. Indeed, one can easily become confused when comparing the viewpoints of male and female ego as regards relatives in the

odd level. Quadrants (parts of a diagram) and sections (parts of a society) are not the same thing. Thus I and my sister, who are in the same section, share a father, but we locate him and his section in different positions on the diagram. This is because to me he is ssP, so in quadrant 1, but to her he is osP, so quadrant 3. One can envisage the sections (enduring groups of people sharing an unchanging identity such as a name or totem) changing from one side of the diagram to the other as one changes the sex of ego.

Relations between the moieties can be neatly expressed in terms of exchange. Ego's generation moiety gives the children produced in the wombs of its members to constitute the other generation moiety, a fact that is represented by the outside cycling filiation lines ; and the prestation is reciprocated, as is shown by the ordinary filiation lines. Ego's patrimoiety gives its sisters and daughters in marriage to the other patrimoiety, and receives the corresponding counterprestation, as is shown by the four marriage lines. If we swap the reading of triangles and circles the exchange is of brothers and sons between matrimoieties. In sex-neutral language ego's ssP recruitment moiety gives osG and osC to the other recruitment moiety in exchange for their equivalents.

It is clear by now that an isomorphism exists between (a) the vocabulary, rules and behaviour in the domain of relatives and (b) the structure of society as a whole, and that this isomorphism arises because in all cases we are dealing with realisations of a single underlying structure⁶. We can now see more clearly the essence of the cycling filiation lines. They not only enable ego's generation moiety to make a return to the other for the new lives it receives, but also make it possible for the system to function perfectly, given a large enough population. An egocentric generation is a very different sort of thing from a sociocentric generation moiety : in an ordinary kinship system ego's parent's generation eventually dies off, whereas in the tetradic society ego's parental generation moiety cannot die off, being constantly replenished by the children of ego's generation moiety. Thus they in turn constantly replenish ego's generation moiety, and the risk of ego failing to find an appropriate spouse simply does not arise. The standard genealogical diagrams used to illustrate non-tetradic « elementary structures of kinship » are based on generations and, essential though they are for many analytical purposes, they cannot be perfectly realised by a demographically realistic population. From this point of view, tetradic structures of kinship are the only truly elementary ones.

IV. Logical simplicity and historical priority

Since tetradic kinship systems are so remote from our own experience, they may not be simple to grasp at first reading, but even so it should be intuitively clear how and why they are logically simple⁷. The simplicity is partly a matter of the small number of classes that suffice to classify the whole domain of relatives/society without ambiguity or overlap, but perhaps one should emphasise even more the fact that a single structure shapes a multiplicity of contexts that we are accustomed to think of as separate. Our relatives, scattered here and there across the map ; the kinship terms with which we identify them as relatives (somewhat complicated by the decline of marriage, family breakdown and new reproductive technology) ; the laws regarding incest ; the results of marriage, especially the immediate appearance of a new set of affinal relatives, who previously were simply not relatives ; the statistical tendency towards class

endogamy ; social structure seen as consisting of vaguely or arbitrarily defined socio-economic classes – some such picture presents an obvious contrast with the tidy unitary structure of tetradic society. Could one argue that this tidiness is due to the level of abstraction at which the analysis has chosen to operate, that the argument is circular since of course if one simplifies by abstracting the end point will be simple ? This would be to miss the point that a system in which the domain of relatives and the structure of society are isomorphic really is simpler than one in which separate descriptions are needed for each.

It is natural to ask whether the process of simplification can be carried further, and indeed some have speculated that human society began when two hordes met and started exchanging women. But it is not possible to reduce a tetradic society to a dual organisation while retaining the prohibition on marriage with primary relatives *as an implication of the rules of social structure*. Whatever arrangement one tries, it will always be necessary to add a rule formulated in non-classificatory language to eliminate one or other primary relative. This new rule, so contrary to the classificatory style governing the rest of the system, nullifies, indeed reverses the apparent gain in simplicity achieved by reducing four sections to two moieties. A *double* dichotomy is the furthest one can go in simplifying the domain of relatives/society, if one hopes to remain within what is humanly plausible.

One possible use for the tetradic model is as a pedagogic device. To understand how the model operates helps one think clearly about kinship systems in general, much as understanding non-Euclidean geometry or imaginary numbers can help one think about space or arithmetic. To approximate to a system such as we meet in the real world, the abstract model has naturally to be enriched with all sorts of features (spatial organisation, legal fictions and rituals such as adoption or blood-brotherhood, step-relatives, marital choice, local beliefs about reproduction, kinship and gender...), and to appreciate this is to perceive the gap that has to exist between models and reality. Similarly, to conceptualise the transformations that are needed to bring the model into line with attested systems could facilitate thinking about historical transformations of kinship systems. There is nothing wrong with this, but my focus here is on the world-historical question with which the paper started. Did human kinship systems start out as tetradic ? Can the idea that simplest equals earliest be supported by empirical evidence as well as by logic ?

Potentially the question could lead to a great deal of discussion and indeed research, and I offer here only a few brief remarks. (1) It is a text-book commonplace that small-scale tribal societies are pervaded by kinship, and this condition is met by tetradic society. (2) It has often been held that understanding the origins of incest rules would take us a long way towards understanding the origins of human society, and rules of this sort are embodied in the tetradic structure. (3) The individual features that enter into tetradic society are all ethnographically attested – notably bilateral cross-cousin marriage, terminological equations between alternate genealogical levels, societies with four sections and generation moieties ; it is only their combination in minimal form that is an invention. (4) Other forms of assimilation between alternate generations are not uncommon (for instance reincarnation of grandparents in grandchildren), and can be interpreted as continuing the assimilation originally expressed in the sections and in the terminology. (5) Such historical evidence as we have suggests that

kinship systems tend to lose features that are present in the tetradic model and gain ones that are alien to it, rather than changing in the opposite direction.

Quite a strong argument emerges if one thinks about the motivations and mechanisms of semantic change in terminologies. The tetradic terminology contains three different types of equation : alternate generation equations (associated with the vertical dimension of kinship), horizontal equations resulting from the marriage rule, and classificatory equations. Since all of these occur outside the model, critics opposed to the idea of an original tetradic terminology need to be able to offer explanations of how they arose. For instance, they need to argue that languages which at one moment distinguish the real mother from classificatory mothers (so that $M \neq MZ$) become so obsessed with the idea of ssG equivalence that they lose the distinction. Being based on the visible event of parturition, the distinction must always be relevant to some speakers in some contexts, and its retention would not be incompatible with the use of the ssG equivalence principle elsewhere in the terminology. Nevertheless, the critic must argue, one or other of the two separate words (presumably the one for MZ) becomes obsolete and disappears, so that the language now has $M = MZ$. Equally implausible changes are needed to explain, for instance, the replacement of $W \neq MBD$ or $MB \neq ZS$ with the corresponding equations. Such problems do not arise if the equations are present from the start⁸.

The only obvious alternative to a tetradic starting point for humanity is to suppose that the earliest kinship terminology covered only primary relatives and then gradually extended outwards to secondary ones, tertiary ones and so on. At its simplest this process of lexical innovation would produce a terminology without equations, which would then have to be introduced by the problematic processes mentioned in the last paragraph. Alternatively, one has to suppose that the process of extension was guided by a vision of marriage and recruitment rules, but then the problem is to explain how this vision originated in the absence of language in which to express it. Moreover, it is far from clear how the vision could guide early phases of the expansion, say from primary to secondary relations.

In any case an expansion hypothesis is unnecessary. Since kinship is usually thought of in terms of relatives and their lexical classification, that was how we started on the path that led us to the tetradic model. But other paths are possible. Someone who is already familiar with the idea of a four-section system can reach the same end-point simply by attaching to the system an isomorphic or congruent egocentric nomenclature ; and one can introduce four-section systems by talking of cross-cutting moieties and their rules of marriage and recruitment without reference to cross-cousins and the like. Approached in this way, the kinship system becomes an implication of the social structure, one among the ways in which the structure is manifested. If we can propose an origin for the social structure, we do not need a separate origin story for the terminology.

V. Origin of tetradic model

Hunter-gatherer societies spend much of their time living in small bands with a couple of dozen members, but from time to time they assemble, typically for ritual purposes. We can thus contrast phases of dispersal and concentration. It is surely more likely that a whole group will divide itself up – give itself a structure – when it is gathered together in one place than when it is dispersed, but one would like to go further and try to model the processes that led up to cross-cutting

generation and recruitment moieties. This will inevitably be speculative, but a clue is perhaps provided by the association of demographic concentration with ritual.

Rituals and similar activities quite often require the division of participants or congregation (dancers, singers, actors, contestants...) into two or more sides or teams, so the problem lies not so much in explaining holistic divisions of society, but in seeing how these divisions could be associated with rules of marriage and recruitment. Since the aim is to explain the origin of the rules, we have to make the effort to imagine that they are not already in existence.

Suppose then that the society splits into two teams, selected at random, with an identity and membership that endures from one period of concentration to the next. Suppose too that, as part of the ritual, couples within each team pair off and have intercourse. Let us say that this relationship is socially recognised, even though it is confined to the ritual context. We might even think of it as proto-marriage, and of the ritual as constituting or including a sort of proto-wedding. But so far team A does not necessarily contain the parents and children of team B : we now have somehow to incorporate the vertical dimension of kinship. So, in due course the time comes to initiate new members of society (whether or not they were conceived in the proto-marriages does not matter), and the collective initiation also takes place during a period of concentration. But the newcomers are initiated not into the team of the mother who bore them but into the other team (prototype of a recruitment rule). In this way the initial random allocation of individuals to teams gives way, in the ritual context, to child-exchanging generation moieties.

Suppose that the ritual now becomes more complicated and the two teams are each bisected, randomly, but in such a way that each individual within an established couple belongs to a different half-team, newcomers to the team being allocated randomly to one or other half. This random element can again in due course be phased out, the new participant being allotted to the half-team of his or her mother's mother⁹. I talk of mother's mother rather than father's father because the identity of an individual's mother is always clear while that of the father need not be (probably, in this case, will not be). However, it is unnecessary to decide whether matrimoieties or patrimoieties came first : the cross-cutting of generation moieties with one form of recruitment moiety necessarily generates the other form.

So far, nothing has been said about marriage or recruitment during the dispersed phase, and there is no need to assume that what happens in the concentrated phase is immediately transferred into ordinary life when the assembly breaks up into bands. The bands might for generations follow whatever more or less pre-social arrangements they had inherited from the past. During this transitional period the dispersed and concentrated phases would contrast as nature to culture, and the innovations developed in the context of ritual would only gradually spread to take over tribal life as a whole. Perhaps one can think of the aesthetic elegance of the tetradic structure as playing some part in its development and spread. Might there also have been socially sanctioned selection pressures operating against non-conformists and hence in favour of those who by virtue of their genes were most inclined to grasp the new rules of ritual and kinship and most willing to follow them ?

VI. Wider questions

This speculative little story about the origin of the tetradic structure is less important in my argument than (a) the ritual context and (b) the derivation of the classificatory terminology from the social structure. As regards (b), one can remain agnostic about the early existence of terms for primary relatives, or for some of them, especially mother, provided it is clear that these terms did not serve as a nucleus from which the terminology gradually expanded. However, there is still a deeper question, namely whether a tetradic society needs to possess a full spoken language at all. To operate the system in a minimal manner, ego only needs to know two things : into what section he can marry, and in what section his children belong. But section membership could be signalled by non-linguistic means such as haircut, body painting or ornamentation, even perhaps, within the ritual context, by posture or position in space (position on the terrain of dance ?). Thus in theory at least, language is not essential. On the other hand one might well doubt whether the organisation of teams and half-teams could really occur without something resembling spoken language.

This leads to the problem of how, in the early history of the human species, innovations in the sphere of kinship and social structure relate to innovations of other sorts, and hence to absolute dates. Since kinship systems typologically close to the focal tetradic model are well attested in aboriginal Australia, the model surely developed before the human settlement of that continent some 50 000 years ago. Furthermore, since it is so difficult to imagine a fully human society that lacks a kinship system, I suppose that the model was already familiar to those of our ancestors who dispersed from Africa twice as long ago as that. Whether one can go further back still, I do not know.

Tetradic theory may hold clues or suggestions to those approaching the problems of human origins from other perspectives. Perhaps for instance, some of those using a Darwinian framework put too much emphasis on the imperatives of subsistence and reproduction, too little on the genesis of ritual and social organisation. As regards language, perhaps the functions served by that human innovation during the concentrated phase may have differed from those of the dispersed phase, and may have been equally or even more important. In any case, no satisfactory account of human origins can ignore the domain of kinship and social structure, and no satisfactory account of this domain can ignore the tetradic framework, within which alone these two aspects of society can be perfectly congruent.

Notes de NICK ALLEN

¹ For the history of the subject see F. Zimmermann, *Enquête sur la parenté*, Paris, PUF, 1993. For my own earlier writings see « The prehistory of Dravidian-type terminologies », in M. Godelier, T. R. Trautmann and F. E. Tjon Sie Fat (ed.), *Transformations of Kinship*, Washington, Smithsonian Institution, 1998, p. 314-331, and chapters 3-4, in *Categories and Classifications: Maussian Reflections on the Social*, Oxford, Berghahn, 2000.

² Of course kinship terms may have functions other than classifying relatives, but this minimal function will suffice here. Let us also simplify by assuming that a language has only a single set of terms.

³ Following convention, and being male, I write with a consistent and explicit male bias. This explains the ordering E W H earlier in the paragraph : male ego's W is given priority over female ego's H.

⁴ I recommend drawing the diagram in the following order: the four brother-sister or sister-brother pairs, the ordinary filiation lines, the marriage lines, the cycling filiation lines ; finally fill in ego and add the quadrant numbers.

⁵ The answer is the ego triangle.

⁶ One can also envisage the structure being applied to various other domains: within the kinship domain to the patterning of attitudes towards different categories of relative (familiarity, avoidance, respect and joking), and outside it to the construction of a cosmology. In some languages (aboriginal Australia) the social structure even interacts with the grammar, affecting the choice of pronoun.

⁷ I leave the definition of simplicity informal and implicit.

⁸ The « cognatic » equation $FZ = MZ$, as in *tante*, raises different issues. Relatively uncommon across the world, it is not a classificatory equation in the sense we have been using the word. In particular, the equation is not just between the shortest pair of formulae from an indefinitely large set of ever-increasing genealogical remoteness. If the significance of the difference between the father's and mother's side of the family declines for any reason, the replacement of the distinction by the equation is not puzzling.

⁹ Logically, the allocation could as well be to the other half of the appropriate team, the half that contains not MM but FM. This will result in a tetradic structure of a non-focal type, a topic which will not be examined here.

La question de l'émergence des activités symboliques au cours de l'hominisation fait aujourd'hui l'objet d'une attention renouvelée de la part des chercheurs.

En témoignent ces *Actes des Journées* tenues à la station biologique de l'École normale supérieure en 2002, pendant lesquelles anthropologues, linguistes, modélisateurs et philosophes du groupe de travail « Modélisation de l'émergence du langage » ont, avec leurs invités britanniques, dressé un état de la question et présenté leurs travaux.

Sont reproduits ici, après une introduction en français visant à situer les débats d'un point de vue général, les exposés en anglais de trois anthropologues. Loin de présenter une unanimité de façade, ces exposés montrent au contraire la diversité des points de vue sur le front avancé des recherches contemporaines. Étudiants et chercheurs trouveront dans ces textes matière à réflexion anthropologique et philosophique à propos de la question si complexe de l'émergence de la culture.

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