Anthropologica

N.S. Vol. II, No. 1, 1960

LE CENTRE CANADIEN DE RECHERCHES
EN ANTHROPOLOGIE
UNIVERSITÉ D'OTTAWA

THE CANADIAN RESEARCH CENTER
FOR ANTHROPOLOGY
UNIVERSITY OF OTTAWA

Dessin - Drawing:
T. E. Ingram

Anthropologica

N.S. VOL. II, NO. I, 1960

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La notion de Société globale chez les Anthropologues

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L'expression "société globale" est de Georges Gurvitch, mais depuis que Spencer a assigné comme objet à la Sociologie l'étude comparative des "sociétés", considérées comme des systèmes sociaux autonomes se délimitant les uns les autres et assurant, chacun dans leur secteur, une intégration suprême de tous les phénomènes de solidarité sociale, la science sociale, tout en se diversifiant et en se développant, n'a pas cessé d'utiliser plus ou moins explicitement ce type d'unité sociale souveraine comme cadre de référence de ses analyses.

Le recours à un tel concept est si constant qu'il semble répondre à une espèce de nécessité logique de l'étude de la réalité sociale, qui amène à y considérer d'abord les phénomènes d'intégration sociale les plus simples et à en dégager progressivement la signification en les voyant comme des éléments de systèmes sociaux de plus en plus compréhensifs, jusqu'à un dernier système qui inclurait tous les autres et par rapport auquel chacun de ceux-ci, avec tous ses éléments, prendrait son véritable sens.

Cette démarche intellectuelle est proprement scientifique dans la mesure où elle manifeste le souci de comprendre les choses dans leur totalité et par leur totalité; cependant, appliquée à l'analyse de la réalité sociale elle nous paraît comporter deux dangers auxquels, dans la ligne de l'organicisme de Spencer, on n'a pas toujours échappé.

En privilégiant les phénomènes de solidarité et d'intégration sociale, elle peut d'abord amener à négliger ou à minimiser les phénomènes de dissociation, de tension et de résistance à l'intégration qu'implique toujours à divers degrés la vie sociale et conduire ainsi à concevoir faussement les sociétés globales à la

façon d'un organisme ou d'une fourmilière où le tout domine les parties sans réserve et sans conteste.

Le second danger qu'implique la quête à l'unité à la fois la plus compréhensive et la plus significative dans l'étude de la réalité sociale consiste à ramener les phénomènes de solidarité qui s'y manifeste à un seul type d'intégration d'ensemble et à imaginer la société globale, encore sur le modèle séduisant de l'organisme, comme un super-groupe se subordonnant tous les les autres.

Dans les deux cas la tentation est celle du fonctionnalisme intégral qui conçoit la société globale comme un système fermé à l'intérieur duquel la vie sociale est totalement unifiée soit par les déterminations de la culture, à la façon d'une certaine anthropologie, soit par l'action coordinatrice des divers sous-systèmes d'interaction sociale, à la façon d'une certaine sociologie.

Les anthropologues, pour leur part, étaient d'autant plus susceptibles de s'engager sur cette double pente que les sociétés de type tribal qu'ils ont d'abord étudiées étaient des sociétés closes et hautement intégrées, et que c'est en fonction de celles-ci qu'ils ont élaboré leur concept de base, le concept de culture.

Ce concept leur donnait d'emblée l'impression d'avoir résolu, une fois pour toutes, le problème de la société globale. L'on sait, en effet, que pour les anthropologues, la culture est à la fois le principe d'intégration et de délimitation des sociétés. C'est l'unité de la culture qui fait l'unité de la société et c'est parce qu'elles incarnent des cultures différentes, irréductibles les unes aux autres, que les sociétés forment autant de touts autonomes distincts, autant de sociétés globales.

Pour risquer une définition de la culture "totale" telle que la conçoivent généralement les anthropologues, disons qu'elle consiste pour eux dans l'ensemble cohérent des croyances, des attitudes, des symboles de communications, des procédés techniques, des schèmes de comportement, des modèles de relations et d'organisations sociales que la population d'une aire territoriale partage et vit en commun et en vertu duquel elle forme une société. La société et sa culture forment ainsi un tout homogène indissoluble, au point qu'on emploiera indifféremment les termes

de société ou de culture pour le désigner. C'est par le même processus historique qu'une population a développé une culture et s'est constituée en société. C'est par la transmission de sa culture d'une génération à l'autre qu'une société se perpétue dans son identité. Une société n'a pas d'autre structure que celle que lui fournit sa culture, et d'autre vie collective que celle qu'anime le dynamisme et qu'informent les modèles et les normes de cette même culture.

De plus, pour certains anthropologues, comme Radcliffe-Brown et Malinowski, tout dans le cadre d'une culture est fonctionnel, en ce sens que toutes les pratiques, même celles qui peuvent sembler les plus aberrantes, font partie intégrante de la culture et servent toujours, à la fois, à la conservation de la société concernée et "au bonheur biologique et mental" de ses membres.

Alors même qu'elle n'aboutit pas à ce fonctionnalisme extrême qu'a si pertinemment dénoncé Robert Merton dans "Social Theory and Social Structure", la conception anthropologique de la société globale implique un monisme qui nous paraît inadmissible.

Dans cette conception, la notion de culture est si compréhensive, pour ne pas dire si impérialiste, que l'idée de société n'y intervient, semble-t-il, que pour désigner l'ensemble des individus qui servent de support biologique à la culture.

Nous ne discuterons pas ici des rapports de la culture et de l'individu tel que les conçoivent certains anthropologues, dans les termes d'une dialectique où l'individu n'apporte que le dynamisme de son animalité et reçoit de la culture les attributs de l'humanité et de la personnalité. Il s'agit là d'une prise de position philosophique dont la critique a d'ailleurs été intelligemment faite par Mikel Dufrenne dans son ouvrage, "La personnalité de base".

Ce que nous voudrions plutôt mettre en cause, c'est la réduction du social au culturel que l'Anthropologie nous semble opérer.

Sans doute le social et le culturel sont-ils indissolublement liés; mais, à ne définir les sociétés qu'en fonction des cultures et à ne considérer la vie et les formations sociales que dans leurs

déterminations culturelles, nous paraît aboutir à une absorption indue du social dans le culturel.

Bien que culturellement conditionnées, la vie et les formations sociales n'en ont pas moins leur spécificité et leur autonomie relative par rapport à la culture dont ils dépendent et cela, même dans les sociétés les mieux délimitées et les plus intégrées culturellement.

L'impérialisme de la notion anthropologique de culture nous paraît tout particulièrement manifeste quand, dons l'analyse de la vie sociale, les anthropologues considèrent les rôles sociaux et les conduites régulières qui leur correspondent, comme deux secteurs de la culture, alors que ces deux catégories de phénomènes illustrent clairement, selon nous, la distinction du culturel et du social.

Explicitons donc les concepts qu'implique selon nous ce type d'analyse de la vie sociale et voyons comment s'y révèle la spécificité et l'autonomie relative du social par rapport au culturel.

Et d'abord il nous semble qu'on ne saurait, sans fausser toutes nos analyses, identifier le rôle social avec l'activité fonctionnelle qui s'y rapporte. Le rôle social est une norme.[1] C'est le modèle idéal de comportement auquel l'acteur social, individuel ou collectif, doit conformer sa conduite pour être fidèle à son personnage et aux exigences du scenario social dans telle ou telle formation sociale culturellement définie.

Et c'est dans ce sens qu'on peut parler de rôle du père de famille, du médecin, de l'homme d'affaire, du professeur, de l'ami ou encore du syndicat, de l'Université, de l'Eglise ou de l'Etat. Les rôles énumérés étant ici, comme dans une œuvre dramatique, autant de modèles de comportement liés, au plan culturel, à autant de personnages sociaux. Nous considérons en effet le rôle social comme étant, à ce plan, l'aspect dynamique du personnage social, concept qui nous paraît à la fois plus significatif et moins ambigu que celui de statut social qu'on lui substitue d'habitude et que nous suggérons avec Nadel,

[1] Le Petit Larousse ne définit-il pas très justement un rôle comme "la partie d'une pièce qu'un acteur *doit* jouer".

Fichter et Mikel Dufrenne[2], de réserver pour désigner la position asignée à un individu dans une structure hiérarchique définie en termes de prestige social.

Mais si, comme le personnage social, le rôle social n'est qu'un modèle, on ne saurait l'identifier, sans transgression de genre, avec l'activité fonctionnelle par laquelle l'acteur social incarnant, plus ou moins adéquatement d'ailleurs, un personnage social, tend à se conformer à ce modèle. Comme le fait remarquer très justement Homans en discutant ce point, "The design of the ship in not the ship"[3].

Il en est dans la vie sociale comme au théâtre. L'acteur qui incarne un personnage n'est pas ce personnage. Celui-ci lui préexiste comme type idéal ayant tel sexe, tel âge, telle stature, telle physionomie, tel tempérament, tel caractère, dans la pièce telle que l'a conçue et écrite l'auteur dramatique. Comme type idéal, le personnage de la pièce n'est lié à l'existence d'aucun acteur particulier. Il peut être incarné par une série indéfinie de ceux-ci soit successivement sur la même scène, soit simultanément sur plusieurs scènes. De plus aucun acteur n'incarne tout à fait de la même façon qu'un autre le même personnage et les uns l'incarnent plus parfaitement que les autres.

Or, le niveau de la pièce écrite où se situe le personnage et le niveau de la scène où se situent les acteurs qui le représentent ont leur correspondance, dans la réalité sociale, d'une part, dans le niveau culturel où se situent les personnages sociaux comme modèles ou types idéaux, d'autre part, dans le niveau proprement social où se situent les acteurs sociaux concrets qui les incarnent. Et l'irréductibilité de ces deux niveaux différents de la réalité sociale s'y manifeste de la même façon, et même d'une façon beaucoup plus claire, car ce qui n'est qu'une possibilité au théâtre est un fait dans la réalité sociale. En effet, le même personnage social dont la définition fait partie de la

[2] NADEL, S.F., "The Theory of Social Structure", p. 29. — FICHTER, Joseph H., "Sociology", p. 203 — DUFRENNE, Mikel, "La personnalité de base", p. 57.

[3] HOMANS, George C., "The human group", Harcourt, Brace and Company, 1950, p. 125.

culture, sauf s'il s'agit d'une personnage unique, comme celui de président des Etats-Unis, est effectivement incarné dans la vie sociale vécue, et de la même manière variable, par une multitude d'acteurs sociaux, lesquels peuvent en plus, à la différence des acteurs dramatiques, incarner simultanément plusieurs personnages différents.

Les mêmes constatations s'imposent au sujet du rôle social lié à un personnage social au palier culturel et de l'activité fonctionnelle de l'acteur social au palier proprement social.

Ici encore les choses se passent sensiblement de la même façon dans la réalité sociale qu'au théâtre. Le jeu d'un acteur à la scène n'est pas identique au rôle qu'il a assumé. Celui-ci est prédéterminé avec le personnage dans l'œuvre de l'auteur, et le jeu de l'acteur ne fait que s'y conformer comme à une norme préexistante. Et cette norme est comme telle, au plan idéal où elle se situe, indépendante de son jeu particulier: le même rôle dramatique pouvant être joué successivement ou simultanément par un nombre indéfini d'acteurs. De plus, alors que le rôle demeure le même dans la pièce de l'auteur, ses interprétations sur la scène varieront selon le talent des acteurs et aussi selon la conception personnelle qu'ils s'en font.

De même, dans la vie sociale, le comportement fonctionnel d'un acteur social individuel ou collectif est distinct du rôle social qu'il a à jouer pour être fidèle au personnage social qu'il incarne. Le rôle d'un acteur social préexiste à son activité fonctionnelle au plan culturel où il constitue une norme générale à laquelle il doit précisément conformer son activité. Quand un acteur social parle de "son rôle", il ne parle pas de ce qu'il fait effectivement, de l'activité fonctionnelle qu'il exerce dans telle conjoncture sociale; mais il parle de ce qu'il doit faire dans cette conjoncture; il parle du modèle culturel de comportement auquel il est personnellement soumis. Et ici, comme au théâtre, la première preuve que nous sommes en présence de deux réalités distinctes, se situant à deux plans différents, c'est que le même rôle social, tel qu'il est idéalement défini dans un groupe culturel donné, le rôle de père de famille, par exemple, est, par suite du double mécanisme de l'enculturation et du contrôle social, effectivement joué, en autant de comportements paternels différents,

qu'il y a de pères de famille dans ce groupe culturel. La seconde preuve de la distinction des deux phénomènes, et de l'irréductibilité, dans ce cas encore, du social au culturel, c'est qu'au même rôle social, fixé dans sa définition culturelle, correspondent, au niveau de la vie sociale vécue, des comportements fonctionnels qui s'y conforment plus ou moins parfaitement et, de toute façon, jamais exactement de la même manière. En effet, les acteurs sociaux interprêtent leur divers rôles sociaux dans le sens qu'ils les exécutent, mais aussi dans le sens beaucoup plus profond qu'en les assumant ils les récréent en partie par la perception personnelle qu'il en ont et le sens particulier qu'ils leur donnent. Aussi peut-on dire avec Georges Gurvitch que "dans le dynamisme de leur réalisation et de leur interprétation, les rôles sociaux impliquent une grande marge de surprise, de spontanéité et finalement de liberté humaine à tous ses degrés.[4]

Cependant, en dépit de cette marge de surprise, de spontanéité et de liberté dont parle Gurvitch, il n'en reste pas moins que les divers rôles sociaux culturellement établis dans un milieu y sont toujours observés dans une large mesure, en conséquence de leur assimilation par les acteurs sociaux au cours du processus d'enculturation et des diverses formes de contrôle social qui en garantissent l'exécution. On peut d'ailleurs en dire autant des personnages sociaux liés à ces rôles. Le mode d'être idéal qu'ils impliquent tend lui aussi à être fidèlement reproduit, et par le jeu des mêmes facteurs, dans le mode d'être réel des acteurs sociaux qui les incarnent. C'est, dans une large mesure, par suite de cette identification au personnage social qu'on dira d'un individu qu'il n'est pas le même homme au travail et à la maison, dans ses relations d'affaires et dans ses relations d'amitié, dans ses rapports avec ses subordonnés et dans ses rapports avec ses supérieurs, dans sa vie privée et dans un poste officiel.

Mais, pour nous en tenir aux comportements, il résulte du degré considérable d'observance effective des rôles sociaux, dans un milieu social culturellement homogène, une grande régularité des conduites qui en dépendent.

[4] "Déterminismes Sociaux et Liberté Humaine", 1955, p. 127.

Et ce sont ces conduites sociales standardisées que nous faisons grief à l'Anthropologie d'annexer à la culture, sans tenir compte de leur spécificité sociale et de leur irréductibilité aux modèles culturels qui les conditionnent.

Sans doute est-ce cette régularité, cette uniformité relative des comportements sociaux, qu'on peut observer dans un milieu social, qui permet d'accéder, par induction, aux modèles normatifs qui en rendent compte, et, avec eux, à la culture prévalente dans ce milieu, comme ensemble plus ou moins cohérent de ces modèles. Mais cette opération logique ne suffit pas à produire l'identification des deux sortes de phénomènes et il ne s'en suit pas, comme le prétendent les anthropologues, que les conduites sociales régulières fassent partie intégrante de la culture, qu'elles en soient le principal contenu. Une culture ne contient que des modèles et rien d'autre.

Sans doute peut-ton, en suivant la démarche logique inverse, considérer que la régularité des comportements sociaux, dans un certain milieu, dépend, entre autres modèles, des rôles sociaux qui y sont culturellement établis, et que leur structure relativement uniforme est comme une projection du culturel dans le social. Mais cette seconde opération logique ne suffit pas non plus à faire de cette uniformité relative, qui est une propriété des conduites sociales effectives, autrement que d'une certaine façon causale, une propriété de la culture. Et les anthropologues ne sont pas davantage justifiés de s'en autoriser pour faire entrer les comportements sociaux standardisés dans la culture.

Certains anthropologues ne s'embarrassent même pas de ces distinctions et de ces justifications. Ils identifient d'emblée les conduites humaines standardisées à la culture. Ainsi Ruth Benedict dans ses "Patterns of Culture", qui, en faisant de la coutume l'objet propre de l'Anthropologie, confond dans la même notion ambigue les comportements sociaux réguliers et leurs modèles culturels. La notion de "pattern" ou de schème culturel qu'utilisent les anthropologues prête d'ailleurs à la même équivoque, pour autant qu'on y trouve également inclues l'idée d'un modèle normatif de comportement, et l'idée d'une forme standardisée des comportements effectifs.

Cependant la plupart des anthropologues distinguent les deux phénomènes, bien que ce ne soit pas toujours avec beaucoup de clarté et de consistance; largement, sans doute, à cause de l'ambiguité de la notion et du terme. Ils parlent de "patterns" ou de schèmes culturels idéaux pour signifier les modèles normatifs de comportement, les standards culturels; et de "patterns" ou de schèmes culturels réels pour signifier les formes standardisées que revêtent les comportements sociaux effectifs.

Mais cette distinction ne les éclaire pas. Pour eux, les deux sortes de "patterns" ou de schèmes sont culturels au même titre, bien que ce ne soit pas de la même façon.

Ils ne voient pas qu'en dépit de leur dépendance des schèmes normatifs, les schèmes réels, les formes standardisées des comportements sociaux effectifs, appartiennent proprement à ceux-ci, qu'ils font partie de leur structure réelle et qu'ils se situent avec eux, non pas au palier culturel des modèles, mais au palier proprement social où les hommes et les groupes concrets évoluent et où se déroule la vie sociale vécue.

Aussi bien les anthropologues nous semblent-ils pratiquer un impéralisme intellectuel injustifié quand ils incorporent à la culture tout comportement humain, sous prétexte qu'un comportement humain est toujours standardisé de quelque façon; et à plus forte raison quand, à la façon de Herskovits, ils y incluent rien moins que "L'Homme et ses œuvres".

Mais peut-être sommes-nous trop catégoriques et chicanons-nous indûment les anthropologues pour ce qui serait, en définitive, une mise en persective spéciale de l'ensemble de la réalité humaine et sociale, aussi cohérente et aussi valable qu'une autre.

On pourrait le croire, si la notion de culture qu'utilisent les anthropologues fournissait effectivement, sans contradiction interne, une vue globale cohérente de tous les phénomènes dont elle prétend rendre compte. Mais il ne paraît pas qu'il en soit ainsi.

Quelle est en effet l'une des propriétés principales de la culture telle que les anthropologues eux-mêmes la définissent? C'est qu'elle est apprise et communiquée et, en tant que telle, transmissible, par les divers processus d'enculturation et d'acculturation, d'un individu à l'autre, d'une génération à l'autre, et

même, en partie, d'un groupe culturel à l'autre. Or, il est manifeste que les comportements sociaux effectifs, même standardisés, ne possèdent pas cette qualité essentielle de transmissibilité qui autoriserait à les considérer comme partie intégrante de la culture. La conduite sociale effective d'un individu peut être répétée à peu près sous la même forme par un autre individu s'il se conforme au même rôle social, mais la conduite d'un individu n'est pas transmissible à un autre individu.

La seule chose qui est transmissible et transmise, dans le cas des comportements sociaux standardisés, ce sont les modèles idéaux qui servent de normes communes à ces comportements et que les hommes se communiquent en les apprenant les uns des autres. Seuls ces modèles feraient donc partie de la culture, puisque seuls ils en ont la transmissibilité spécifique. Aussi bien, les anthropologues ne nous semblent-ils pas conséquents avec leur propre conception de la culture, quand ils considèrent ces phénomènes intransmissibles que sont le comportement des hommes et les régularités qu'il manifeste comme faisant intrinsèquement partie de la culture. Et quand avec Kluckhohn, on a justement défini une culture comme "un système de modèles de vie (designs for living) explicites et implicites, conditionnés par un processus historique, et qui, à un moment donné, a tendance à être partagé par tous les membres ou du moins par une partie des membres d'un groupe donné"[5], on devrait semble-t-il, se défendre d'y inclure, par la suite, des phénomènes qui dépendent de ces modèles de vie, mais qui en ont ni les propriétés, ni les caractères.

Que penser alors de la persistance de trop d'anthropologues à diviser la culture en culture matérielle; les réalités physiques que l'homme aménage, fabrique et utilise: et en culture non-matérielle: toutes les manifestations proprement psychologiques de la vie de l'homme en société.

Les modèles normatifs d'aménagement, de fabrication et d'utilisation de ces choses matérielles ne jouissent-ils pas seuls de cette faculté d'être appris et transmis par l'éducation et

[5] KLUCKHOHN, Clyde, *"Le Concept de Culture"* dans *Les Sciences de la Politique aux Etats-Unis,* p. 134.

l'imitation que suppose la notion de culture? Et alors comment une culture peut-elle se diviser ainsi en culture matérielle et en culture non-matérielle?

En lui donnant une extension injustifiée et en lui faisant englober des éléments qui ne lui sont pas intégrables, on introduit la contradiction dans la notion de culture et elle éclate.

Ce qui est une autre preuve, selon nous, de la spécificité et de l'autonomie relative du social et de son irréductibilité au culturel.

Cette même faculté de transmissibilité que doivent, par définition, posséder les phénomènes proprement culturels, liée à la nécessité de leur diffusion généralisée dans un milieu, nous interdit même d'inclure dans la culture tous les modèles normatifs qui conditionnent la manière d'être et d'agir des hommes. En effet, non seulement la culture n'est-elle constituée que de modèles, mais encore elle n'inclut pas tous les modèles.

C'est ainsi que les schèmes normatifs qui régissent d'une façon immédiate la structure et le fonctionnement des groupes fonctionnels[6] ne sont pas des modèles culturels, car, en tant qu'ils appartiennent en propre à ces groupes, ils ne sont ni transmissibles, ni, par conséquent, partagés entre plusieurs groupes. Ils sont des modèles exclusivement sociaux.

Ainsi l'institution universitaire, en tant que modèle idéal d'organisation de l'enseignement supérieur et de la recherche scientifique prévalant dans le monde occidental, est l'un de ces modèles normatifs qui font partie de la culture, car il en a la transmissibilité et la diffusion.

Mais la constitution, les règlements et les procédures particulières qui, plus ou moins conformément à ce modèle idéal,

[6] L'expression "groupes fonctionnels" est ici empruntée à Gurvitch pour désigner les formations sociales organisées en vue de la poursuite de buts spécifiques orientés vers la satisfaction de besoins humains définis. On pourrait encore parler ici de *groupes sociétaires* en les distinguant des *groupes communautaires,* qui eux ne doivent pas leur unité à une même fin extérieure poursuivie en collaboration, mais au partage en commun, par leurs membres, d'un même ensemble de modèles culturels et aux similitudes qui en résultent entre eux.

régissent la structure et le fonctionnement d'une université déterminée du monde occidental, sont des modèles normatifs qui appartiennent en propre à cette université et qui, comme tels, ne sont pas transférables d'une université à l'autre. Même si la constitution d'une université imite servilement celle d'une autre université, elle n'en reste pas moins exclusivement sienne.

De même la conception de la structure de l'Etat et de ses fonctions qu'implique l'idéologie démocratique est un modèle normatif culturel que peuvent se transmettre et se partager des millions d'individus et assumer comme norme idéale plusieurs Etats. Mais la constitution de l'Etat canadien lui appartient en propre, comme le principe singulier et exclusif de sa structure et de son fonctionnement.

Nous sommes donc en présence de modèles normatifs d'organisation sociale et de comportement social qui ne sont pas culturels, mais exclusivement sociaux, bien qu'ils soient conditionnés par des modèles culturels.

Et nous considérons que, faute de distinguer ces modèles normatifs proprement sociaux des modèles culturels, les anthropologues, par une nouvelle inconséquence, font déborder encore une fois la culture dans le domaine proprement social, s'empêchant ainsi de voir dans les groupes fonctionnels des réalités autonomes ayant leur constitution et leurs normes propres, irréductibles aux normes culturelles qui les conditionnent.

Nous trouvons une preuve de cette irréductibilité dans la façon totalement différente dont s'opèrent la définition des rôles sociaux et le contrôle de leur exécution selon l'un ou l'autre type de modèles.

Ainsi les personnes faisant partie d'un groupe culturel donné, une classe sociale, par exemple, se transmettent les uns aux autres et partagent en commun une certaine conception du personnage qu'est le professeur d'université et du rôle social qu'il doit remplir dans l'ensemble de la société. Cette conception constitue un standard culturel d'après lequel le comportement général, réel ou supposé, des professeurs d'université est jugé et dont l'observance est contrôlée d'une façon diffuse par diverses sanctions d'approbation et de désapprobation.

Mais toute autre est la définition du personnage et du rôle du professeur, et tout autre aussi leur contrôle, d'après les modèles normatifs propres au groupe fonctionnel qu'est une université. Les professeurs qu'implique l'organisation de la division du travail propre à une université y sont définis comme personnages sociaux en termes de qualifications académiques précises et leurs rôles en termes d'obligations bien déterminées. Et c'est aux exigences définies de ces personnages sociaux qu'ils assument, que les professeurs, comme acteurs sociaux dans une université, doivent se conformer et par leurs qualifications personnelles et par leur activité fonctionnelle. Faute de quoi, ils seront soumis, non plus seulement aux sanctions diffuses de l'opinion, mais à des sanctions institutionnelles, elles aussi bien définies et bien précises.

Et peut-être faudrait-il, dans le prolongement de cette distinction, différencier le processus de socialisation du processus d'enculturation. En effet, précisément parce qu'ils sont définis en termes de tâches précises à accomplir dans le cadre d'une division du travail déterminée, les rôles sociaux impliqués dans les groupes fonctionnels comportent une spécialisation et un apprentissage particulier auxquels on pourrait réserver le nom de socialisation. La socialisation ainsi entendue serait alors distinguée du processus général d'enculturation selon lequel on acquiert la culture ambiante et qui, de soi, quoiqu'en pense Kluckhohn, ne comporte pas de spécialisation et n'exige pas d'apprentissage spécial. En effet, les rôles sociaux, tels qu'ils sont culturellement définis, font partie de la culture commune et ils sont appris de la même façon par tous ceux qui participent à cette culture et non pas uniquement par ceux qui doivent exécuter ces rôles. D'ailleurs, les sanctions sociales diffuses imposées aux déviants dans les groupes communautaires supposent précisément que chacun soit culturellement initié non seulement aux rôles particuliers qu'il doit assumer, mais à ceux de tous les autres membres du groupe qu'il est appelé à contrôler.

Sans insister sur cette dernière distinction, retenons seulement que les modèles normatifs qui régissent d'une façon immédiate les groupes fonctionnels diffèrent des modèles normatifs culturels par des caractères définis qui les empêchent de faire, avec ceux-ci, partie intégrante de la culture.

C'est peut-être l'apparente indifférenciation de ces deux sortes de modèles normatifs et de contrôle social, dans les sociétés de type tribal régies par la coutume, qui a amené les anthropologues à les confondre dans leur notion de culture. Sans doute est-ce aussi la coïncidence entre l'aire de la culture "totale" et l'aire de la coopération organisée dans les mêmes sociétés qui les a entraînés à confondre dans leur notion de société globale l'ethnie, comme communauté fondée sur le partage en commun d'une culture "totale", avec la société politique qui, en intégrant toutes les formations sociétaires dans les limites d'un territoire, constitue un groupe fonctionnel global, ordonné à la satisfaction d'ensemble des besoins de ses membres, tels qu'ils sont d'ailleurs culturellement définis.

Il est bien certain, en effet, que dans une société de type tribal, ces deux sortes de groupements se recouvrent, délimités qu'ils sont tous les deux par les liens exclusifs du sang.

Mais est-il besoin de rappeler que l'identité de leurs membres ne rend pas identiques deux groupements différents. En sorte que même dans les sociétés tribales, l'ethnie se distingue du groupe politique.

Cependant, la coïncidence spatiale des deux formations sociales, dans les sociétés de type tribal, permettait aux anthropologues, à toutes fins pratiques, de se servir de la notion de culture "totale", comme principe de délimitation d'autant de sociétés globales autonomes et auto-suffisantes.

Mais que devient la notion de culture totale comme principe de délimitation des sociétés globales, là où l'aire de la culture totale ne coïncide plus avec l'aire de la coopération organisée; là où l'ethnie n'est pas exactement recouverte par un groupe politique?

Où se situait la société globale dans le monde grec? La trouvait-on dans la vaste communauté culturelle qu'il constituait et dont l'un des traits culturels était l'idée de cité-état conçue comme forme d'organisation sociale et politique idéale, ou la trouvait-on dans chacune des cités-états, politiquement autonome et autarcique, mais ne possédant qu'une sous-culture de la culture hellénique? Où se situait la société globale au Moyen-Age?

Et où se situe la société globale dans le monde occidental moderne? Dans les cadres de chacune des sociétés politiques qu'il contient ou dans ceux de l'immense communauté culturelle auxquels tous les occidentaux participent?

Où se situe enfin pour nous, Canadiens français, la société globale? Dans les cadres marqués par notre culture nationale ou dans ceux de l'Etat canadien?

Peut-on parler de la société globale canadienne-française? Mais aussi peut-on parler de la société globale canadienne?

Beaucoup de sociologues et d'anthropologues résolvent ces problèmes en définissant la société globale de telle façon qu'elle corresponde exclusivement à l'Etat-National, où société politique et communauté ethnique coïncident substantiellement.

Mais ils ne simplifient ainsi le problème théorique que pose la définition de la société globale, dans la complexité du monde moderne, qu'en s'évadant dans un jugement de valeur et en substituant à la réalité le rêve à peine déguisé d'un retour au type d'organisation sociale que représentait la société tribale.

Ne faut-il pas alors renoncer à définir les sociétés globales en termes objectifs absolus et leur donner la souplesse de cadres de référence variables dans leurs principes de délimitation selon les besoins de l'analyse?

Les frontières politiques, les cadres territoriaux du foyer national d'un groupe ethnique ou même ceux d'une aire de civilisation pourraient ainsi légitimement, selon le cas, marquer les limites de la société globale.

Ce qu'il faut bien voir, en conclusion, c'est que la société globale ainsi conçue n'entre pas dans la catégorie des groupes sociaux, mais qu'elle est toujours, contrairement aux conceptions monistes comme celle qui domine en Anthropologie, un complexe social plus ou moins arbitrairement délimité où coexistent, selon des rapports variables d'intégration et d'opposition, plusieurs groupes sociaux proprement dits et plus particulièrement les vastes formations sociales, irréductibles les unes aux autres, que sont les communautés ethniques et les sociétés politiques.

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Some Social and Spatial Aspects of Innovation at Zuni

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Introduction

In the course of inquiry into the theory of education of the Zuni of New Mexico[1] I discovered a phenomenon of the culture known to them as the "roads". Depicted in their most systematic form by seven of the older male informants, these "roads" or ways of life appeared on the basis of verbal report to have the quality of phenomenal objectivity. Thus, they were accorded the dignity of age and the status of spirit-given entities by all my informants who reported on the nature of the "roads" to a fair degree in a standardized manner. These ways of life, then, must be included as a part of contemporary Zuni culture and as such I will briefly describe them.

The Good Road (*onankokshi*), being the first of the four, was described by the Zuni as the *way of life* of the ideal cooperative and economically productive family man, who deeply desires an harmonious existence with his fellows, who is hard working, self-effacing and moderate in all things.[2] His success is attributed exclusively (a) to the inborn characteristics listed above which predispose him (b) to listen to and thereby learn from those described as the "inside-of-the-house" family. The family of the Good Road is described rather vaguely and the clearest depiction of statuses is that of *parents;* the function of this unit appears to be largely that of guidance.

[1] Field work was carried out over a period of seven months during 1953 and was financed by funds supplied by the Rockefeller Foundation and administered by the Laboratory of Social Relations, Harvard University. Special thanks are accorded to Dr. R.S. MacNeish and Mr. W.E. Taylor, both of the National Museum of Canada, who kindly made constructive suggestions.

[2] The similarity of implicit patterning of the Good Road to Benedict's Apollonian indicates the degree of cultural continuity that is present at Zuni. But the very different patterns of the Long Road should not be construed as something new.

Benedict once wrote that there were no disciplines exercised by the adult men of Zuni families (1948: 92). From the point of view of the Good Road this is certainly true, for with it there would be no need of disciplines. Her point, however, was that the reason was structural — disciplines were handled by agencies in the community other than the family. The Long Road (*onandashone*) is the perfect complement (according to my informants) of the Good Road specifically for purposes of finding the proper locus of disciplines, and it illustrates marked contrasts with the Good Road. This "Road" is described as the way of life of the same individual but from a different perspective, namely, that involving his relationships with a group of older men either *within* the family or closely related to it. The individual, it is implied, is inherently weak and subject therefore to intrusive influences from the environment (disease, injury and noxious but diffuse stimuli that may engender bad habits of essentially foreign origin). The individual, then, acquires a Long Road by having from an early age been subject to a process of toughening that was initiated by these older men, and carried out by them with dedication. This involved extreme coercion, and such "hardening" tactics as early morning rising, naked winter bathing, working long hours without food, and forced abstinence from smoking, drinking and other activities which, if they were to become habitual, would make one "soft inside". The Long Road is not conceived of as related to deviant behaviour; it is considered complementary to the Good Road and the two belong together in the ideal life. Of the two "roads", the former emphasizes inborn dispositions to be cooperative and provides the family as a guiding background whereas the latter indicates no concern with individual motivational states other than those related to fear and emphasizes the strictness and coercive activities of older males associated with the family.

The related Bad Road (*onanbocha*) depicts the individual as being primarily indifferent to cooperative enterprise. He is also depicted as not being able to make the neat discriminations between his own family and outsiders that marks the person of the Good Road. He is thus subject to easy exploitation from the community and beyond, and the background of the Bad

Road correspondingly becomes the community (which is vaguely described, usually as "the people", "everyone", etc.) A strong moral position is taken against the person on the Bad Road especially because of his orientation to present satisfactions. Since, however, he is described as sensitive to the wishes and actions of others (the danger being that he cannot select desirable "others") he can be controlled, and this control is brought about by the use of shame (*yatsadi*). This "shame technique" is not, however, applicable to the persons described in terms of the Short Road (*onankoni*) which is a state of illness and abnormality rather than one of deviance. Since the person of the Short Road is said to be born the way he is (the essentials of the case being that he is not responsive to the initiation of interaction) he cannot be controlled by ordinary measures that are part of the recognized system of social control; therefore, he must be subjected to the specialized techniques of the therapist.

While the degree of standardization of these phenomena and of their role in the community (and in the daily lives of the "average" Zuni) constitute problems that must yet be dealt with, there are several characteristics of the context of the production of the "roads" and their own content that suggest more pressing problems that can best be approached by diachronic analysis. The first of these pertains to the clearly *innovative* character of the "roads"[3]. I have found it surprising that in spite of the wide knowledge of the "roads" among my informants there evidently has been no record made of them by such close and perceptive observers in the past as Cushing, Stevenson, Kroeber, Bunzel and Benedict all of whom were intimate with the community at various times between 1870 and 1935. The only reasonable conclusion is that the "roads", which I regard as a category of Zuni culture, had not up to that time been developed as they appear in their present form. Second, they have a fairly definite innovative history. Thus, the concept of the *road* or *road of life* is old and

[3] Albert and Cazaneuve write:
"La notion que se font les Zuni du caractère idéal est visible aussi dans leur distinction classique entre les "quatre routes." (1956:11).
There is, in my opinion, no evidence to suggest that the *roads* as described herein, or by Albert and Cazaneuve, represent a well institutionalized phenomenon of long standing in Zuni culture.

well established at Zuni (having long ago been reported (Cushing, 1880; Bunzel, 1932) and in other pueblos, and the conception of their having been given by supernaturals who continue to exercise influence over the "road" appears to have been as true of Zuni in 1880 as it is today. What is *new* is the internal differentiation of the "road" into two distinct types and two sub-types. I shall endeavour to show later that this differentiation took place as other changes appeared in Zuni since that time. Third, it is surprising that such innovative phenomena should be receiving their impetus from a group of older men who, in other role capacities, belong in high priestly positions in the community. On two counts this is surprising: one would not generally expect the upper age group of men to be the carriers of a distinctly innovative tradition, nor would one expect this result from older men who otherwise belong to an extremely conservative tradition of activities (in the priesthoods) in what is generally thought to be an extremely conservative community. Since the "roads" are informally communicated in a family context and are not by any means accepted as guides for conduct in the real world, they might be expected to disappear as readily as they have been created. Whether or not one would make this prediction depends, I think, upon our understanding of the roles of the older men in this context.

Finally, I should like to draw attention to the content of the two "primary" *roads:* the Good Road and the Long Road. The Good Road emphasizes the familiar *Apollonian* ethic of moderation and cooperativeness and stresses harmony and integration. In commenting upon certain scholarly views of Pueblo culture, Bennett has recently summarized one of these (i.e., that which stresses the features listed above) as the *organic theory* (1956: 204-206). One could add at this point that the Zuni have vindicated this view with the statements as they appear on the Good Road. The other view of Pueblo culture, Bennett points out, stresses coercion, autocracy, implicit hostility, fear, etc., and this he calls the *repressive theory*. Again, the Zuni have been accommodating in supporting this view as well, this time with the statements as they appear on the Long Road. Considering the fact that the Zuni have chosen to combine and render inseparable these two primary "roads", it would appear

that they speak in support of Bennett himself who, in conclusion, writes:

> I am convinced that this [i.e., organic] world view actually exists — just as I am also convinced that Pueblo society achieves homogeneity by repressive measures. (*Ibid*: 211)

In this respect it should be noted that the four "roads" constitute a single unit of Zuni culture; the very fact of their presence in the culture attests to the organic character of that culture in some of its aspects, and in this case, then, the "organic world view" embodies and legitimates "repressive measures". Thus repressive measures do not just "occur" but are themselves caught up in a segregated aspect of an explicit world view. It should therefore be proper to speak of "repressive measures" in terms of authority, i.e., *repressive authority*. The production of segments of world view in explicit terms (as, for example, in the recital of folklore) enjoys acceptance by virtue of those in authority who convey its contents; in this sense I shall refer to the presence of a well delineated "world view" as *organic authority*. This applies to the Four Roads as a whole.

From the points that have emerged I should like to suggest the following hypotheses: (a) "repressive" and "organic" patterns of authority have long been a part of Pueblo organization and (b) there is a specific relationship between these two forms of authority that indicates the indispensability of roles in the community which I should like to call *"authoritative innovators"*. In order to supply evidence in support of these hypotheses, it will be pertinent to deal in extensive time depth and, therefore, to examine the archaeological record. In so doing I shall endeavour to indicate that certain objectively ascertainable growth trends in Pueblo society can be seen in relationship to the specified patterns of authority and innovation.

A note on method: a considerable amount has been written on the subject of ethnological inferences based on archaeological materials, especially pertaining to the Southwest (Haury, 1956; Hawley, 1950; MacWhite, 1956; Parsons, 1940; Reed, 1956; Reiter, 1946; Thompson, 1956; Vogt, 1956; Wendorf, 1956; Willey, 1956). In this paper the method is based on familiar assumption, namely, that human groups tend to allocate space

in a community in accordance with their size, structure and inter-relationships. This space may become rigidified in cases where changes in social organization are too rapid and too subtle to be reflected in gross building materials. In the Southwest, however, building, tearing down (and falling down) and modifying are constant activities. Thus, it is the fluidity of settlement patterns that make it possible I hope to draw reasonable inferences from community plans.

The tools of analysis are those pertaining to common principles of grouping: *similarity-difference, proximity-distance, orientation* and *relative size.*

ANASAZI CULTURE GROWTH

Anasazi culture, which belongs within the "four-corners" region of the Southwest (Reed, 1946) was identified as a tradition (first *Basket Maker* and later *Pueblo phases* of development) beginning about A.D. 200 (Martin, Quimby and Collier, 1947: 101). The growth of culture for about 1,000 years following A.D. 500 (from the *Modified Basket Makers*) will be examined here. This growth was marked by the following features or trends: rapid change; progressive enlargement of communities and their recurrent fisions; and simultaneous and mutually limiting trends toward religious centralization and dwelling proximation.

1. *Rapidity of Change*

A cursory inspection of the community plans in this region indicates clearly that by some standards[4] in North American culture the pace of change was very rapid. Even by the objective (yet surely not imposed) measure of roughly 30 generations, the change must still be regarded as rapid, for these few generations were involved in the not-inconsiderable task of bringing about the transformation of communities from the crudest of

[4] Compare communities in the Western and Eastern Subarctic.

the early Basket Maker organizations to those of the most developed post-classical Pueblos.

2. *Enlargement*

Within this same period of time Anasazi communities grew from a few more or less remote clusters of close kindred to units embodying hundreds of dwellings and the ratio of dwelling units to kivas indicates similar growth (Steward, 1937: 97-8). In time the population density became the highest in aboriginal America north of Mexico (Kroeber 1939: table 8) and was, moreover, intensified by the characteristic close-clustering settlement pattern of the Pueblos. A significant aspect of this enlargement is that relative homogeneity was maintained in communities which were accepting considerable numbers of settlers from outside the region (Kidder, 1924: 129).

3. *Recurrent Fissions*

It is well attested in the record of Southwestern archaeology that break-ups of communities were frequent throughout the latter period under consideration (*Ibid:* 128-130).

The rapidity of change and the sure (although sometimes fluctuating) pattern of enlargement of communities can be related to fissions and abandonments on a wide scale in this region. According to Titiev, the reasons underlying these fissions were internal and structural and only indirectly (if at all) related to external threats from enemies, droughts and epidemics. Steward had earlier pointed out that the method of enlargement of Southwestern communities was through the amalgamation of small (and ostensibly complete) units. These would be his *lineage units* which, when combined and compounded into Pueblo towns, brought them beyond optimal size and therefore forced their collapse. Titiev writes:

> There, I believe, we have the heart of the whole matter, for the huge pueblos failed to endure precisely because they continued to operate with social structures that were best adapted to small communities. (1944:99)

Whatever other inferences one wishes to draw concerning the relationship between these three trends in Pueblo culture

growth, two seem reasonable, namely (a) all Pueblans were permanently adjusted to conditions of continuous and fundamental changes in their communities and (b) these conditions were reflected in the quality of authority and leadership.

4. *Mutually Limiting Trends:* Centralization & Approximation

While the evidence is clear enough to establish the fact of recurrent break-ups (the inference being communal instability) there is a strong case to be made for overall stability in the basic plans of culture growth. Thus, even though community break-ups did continue, these events evidently did not bring about a halt to the changes that were going on in the area, nor did communities cease progressively to enlarge.

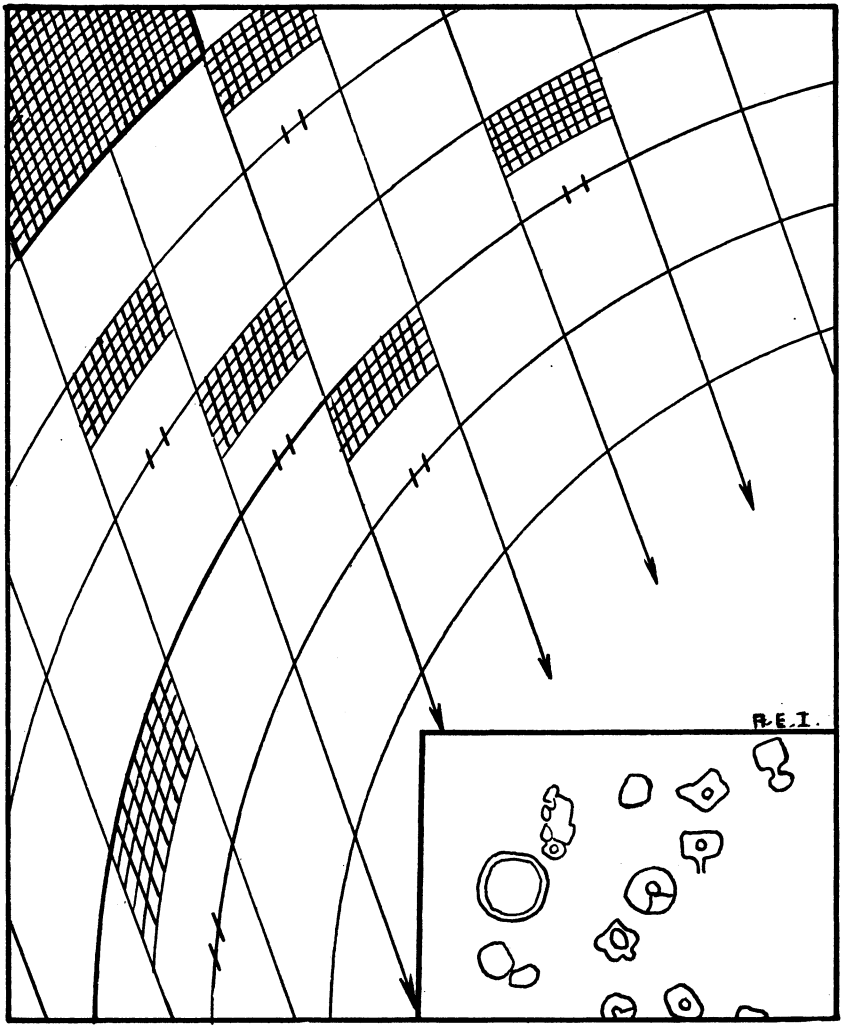
In the following sections I should like to indicate that stability in growth was expressed in part through two long-standing but mutually limiting trends, one toward the centralization of ceremonial structures and the other toward the growing approximation and communication of dwelling units. As a process, Anasazi culture growth involved then a number of trends: rapid change, extraordinary growth, recurrent dangers or actualities of civil strife, and increasing centralization and progressive dwelling communication. I shall examine these trends as part of the community plans at each definable stage of development.

THE MAJOR FEATURES OF CULTURE GROWTH

Modified Basket-Maker Pattern[5]

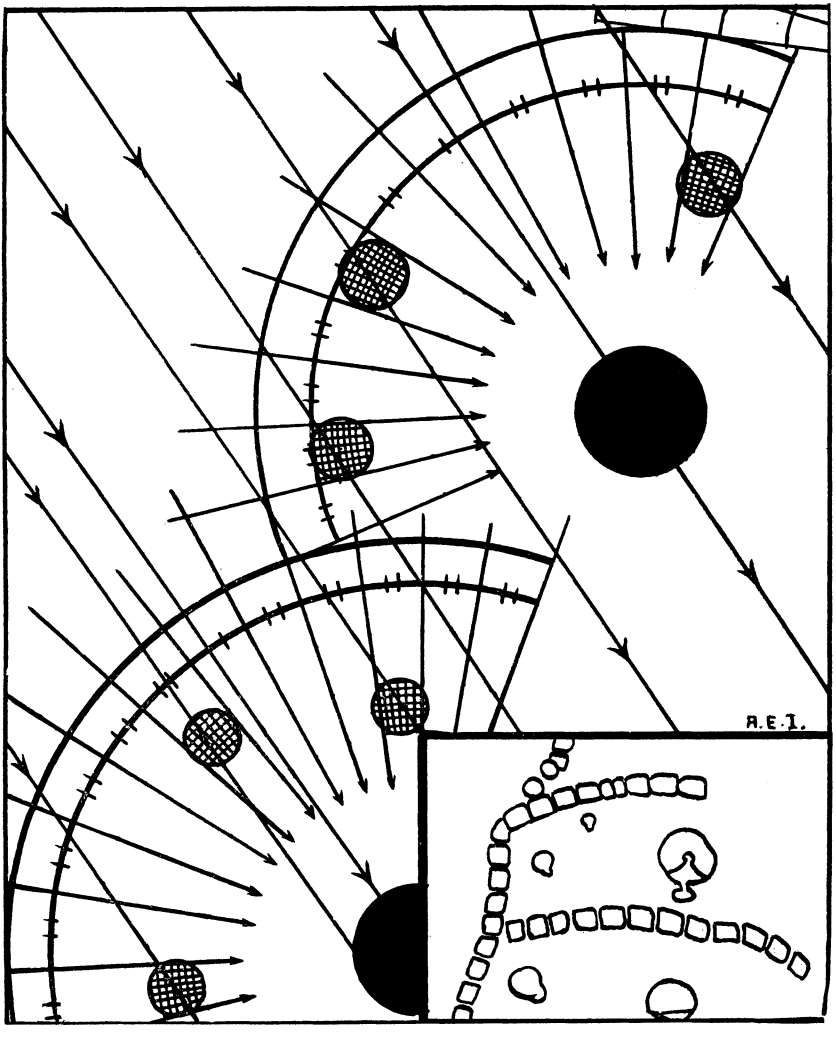
Time: about A.D. 500-700. Type sites: Kiatuthlanna, Shabik'eschee Village (represented above). Dwellings: Semi-Subterranean, variable

[5] The following drawings are taken from actual sites, but illustrate only position, orientation and ceremonial regions. It was convenient to draw these on grids, using arcs and parallel lines, depending on their application to the dynamics of integration as I view them. The first is taken from Shabik'eschee village, the second from Alkali Ridge, the third from Pueblo Bonito and the fourth from Zuni. Concerning the last, I have included geo-



shapes; most internally divided into ceremonial and non-ceremonial regions; in larger community dwellings are directionally oriented southeast. Maximum number of dwellings: 17. Architectural Relations: in most cases not easy to define: *in at least one,* however, a rough arc forms the community.

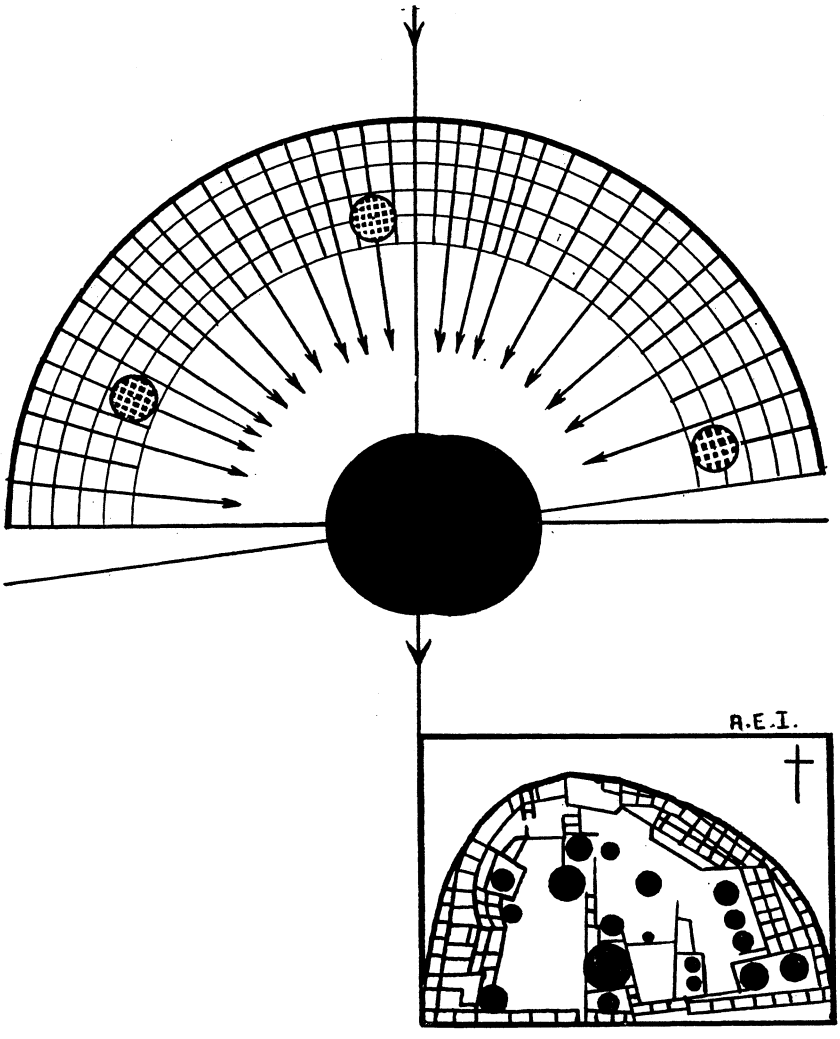
metric projections that are based both on community plans and the houses outside the community which are nevertheless oriented to it.



The Developmental-Pueblo Pattern

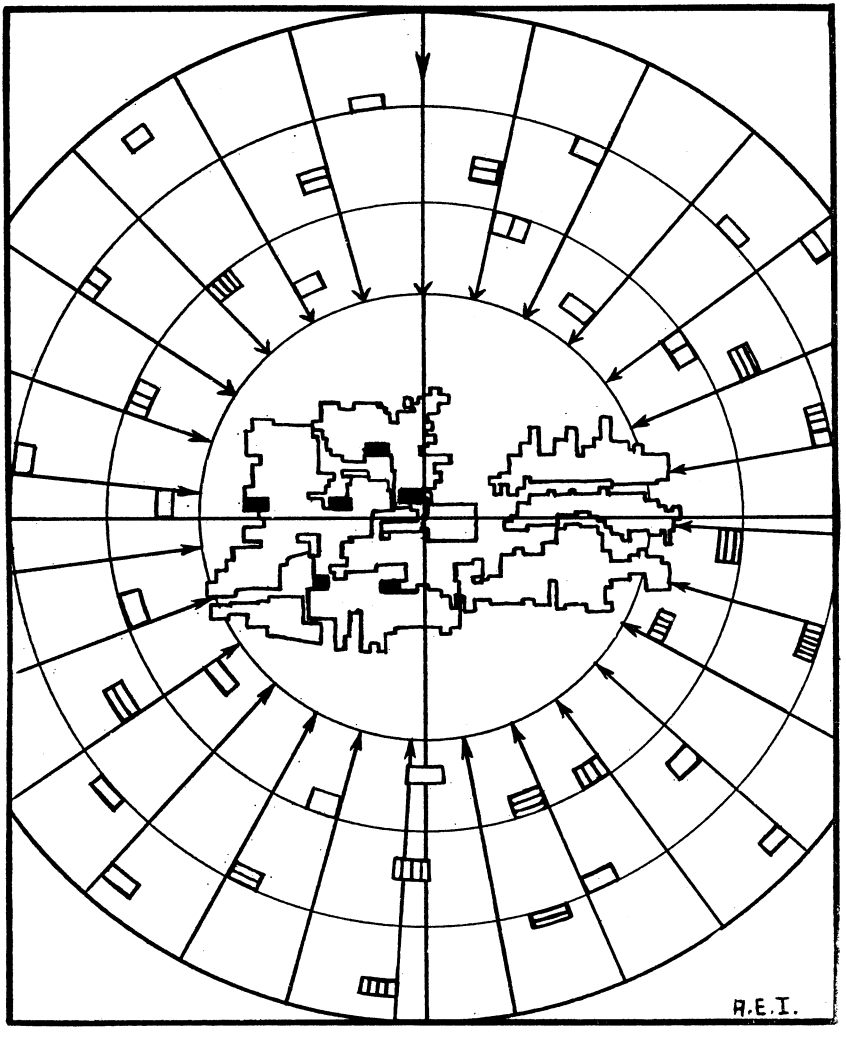
Time: about A.D. 1,000. Type Sites: La Plata, Stollsteimer, Alkali Ridge (represented here). Dwellings: surface, rectangular without internal divisions, oriented toward Great "kivas".[6] Maximum Number of dwellings: 300. Architectural Relations: Definite arcs, connected dwellings and strict formal distinctiveness between dwellings and "kivas".

[6] When "kiva" is written between quotation marks, I intend to indicate that these are proto-kivas or pit-houses serving more specialized religious functions.



Classical Pueblo Pattern

Time: About A.D. 1200. Type Sites: Aztec, Kiatuthlanna, Village of the Great Kivas, Chettro Kettel, Pueblo Bonito (represented here). Dwellings: Surface, rectangular without internal divisions, but multi-roomed. Maximum Number of Dwellings: perhaps 500. Architectural Relations: Square, semi-circular and block communities appear. Dwellings are multiply connected and communications are beginning to appear in lines of 12 to 15 rooms and possibly more. Formal distinctiveness between dwellings and kivas. Some local kivas are surrounded but Great Kivas are spatially segregated from other units in the community.



Post-Classical and Modern Patterns

Time: following A.D. 1500. Type sites: Kin-tiel, Hawikuh, Oraibi Zuni (illustrated here). Dwellings: surface, rectangular, multi-roomed and enlarged and without internal divisions. No visible orientations.

Maximum number of dwellings: probably 4-500.

Architectural Relations: dwellings often intercommunicating and without any formal distinctiveness from kivas. No spatial segregation of kivas from dwellings ("clan" kivas of Oraibi an exception) and the Great Kivas have altogether disappeared. Although the old polarization to a center has disappeared, the phenomenon of "drifting" (note the large number of houses on peripheries) indicates definite polarization with, however, the community as a whole now being the center.

Comments

Of the *Modified Basket-Maker* villages, one is especially interesting, for while its architecture was in keeping with the period, it grew in community size beyond many others. This was Shabik'eschee village, a unit which was clearly greater than the sum of its parts. It was probably more than simply a "unilateral, localized, exogamous and land owning" community (Steward, 1937: 101) for although this was a community whose dwellings (with their sacred regions) indicate a fair degree of control over supernatural relations by the household, at the same time there *was* a center for the group that is indicated by the "kiva" and plaza. Evidence is thus supplied in architecture of what Reiter called "the town hall concept" (1946: 282-3). It is clear, on the whole, that this was the first level at which at least one multi-structured unit occurred which had a form and an apparent center and in which both "household" and "community" could be identified according to some of Chang's criteria (1958: 302-3).

When considering the community plans of the *Developmental-Pueblo* villages (and those that are subsequently to be dealt with here) they should be regarded in relation to each other. One may note, then, that the loose form of the community has become more clear-cut between the period that ends the Modified Basket Maker phase and that which begins the Pueblo phases. There are definite local "kivas" and three Great "Kivas" in the four arcs at Alkali Ridge. There are arcs and a Great "Kiva" development at the La Plata site as well. While the common orientation of dwellings implied to some degree in the former Basket-Maker community a consensus as to how a house should be appropriately placed .(and therefore we may infer some degree of communal control) the individual and clearly marked sacred regions in the dwellings of these earlier communities imply the presence of local or family control over its own ceremonial organization. Developmental Pueblo dwellings now having lost both their sacred regions and their individual alignments, and their having been grouped around local Great "Kivas", stand in a different relationship to the community. Clearly if there are sacred regions at Alkali Ridge these regions

belong to the arc as a whole and religious expression was therefore a performance of a segment of the community that went well beyond the familly and probably beyond the lineage. What that segment might have been is not clear — there is certainly no indication that the "clan kivas" (as they are sometimes called) united lineages or clan segments in any sort of unity distinct from or in conflict with larger segments of the community. If such conditions prevailed one would expect to find some separations as to grouping; one finds, on the contrary, no such thing. In fact, the trend from dwelling *proximity* to their *simple connectedness* has become clearly manifest and this same trend will continue, finally to obliterate any possible subdivisions in Pueblan communities that are based on a lineage or clan arrangement. With their presumed method of building on the peripheries of arcs, with their insistence upon simple connectedness, and with the resulting gradual obliteration of sub-group lines, the possibility of the development of segregated dwellings — those that clearly show privileged access to the facilities of the centers (i.e., the Great "kivas") is disappearing. The most conservative and enduring pattern of front-to-back (or front *and* back) in Basket Maker and Pueblo communities (including but not surpassing Pueblo III) emphasized by Reed 1956: 11) is clearly illustrated in both settlement patterns and involves the community as a whole in both cases. But in the Modified Basket Maker community the dwellings *shared* in this back-front orientation (i.e., the front and back of the dwellings faced as did the front and back of the community) whereas at Alkali Ridge and the other Developmental Pueblo communities this was not true: rather, the importance of the orientations of Pueblo dwellings is that they *all faced their centers.*

Although a polarization of segments of communities around central ceremonial structures had taken place during the Developmental period, there was no indication of polarization around a single central region prior to the Great Pueblo period. This was now taking place, particularly at Pueblo Bonito, Tyonyi, Aztec and Chettro Kettle. While the Great Kiva with its central plaza (in some places apparently organized in pairs) has reached its maximum development in size and in centralizing the polarized blocks of dwellings, the further development in

the intimacy of relations between dwellings marked communities everywhere. Their having become *multiply connected,* indeed surrounded, the limits of the household can no longer be defined according to Chang's first criterion of identification (Chang, *Op. Cit.*). The dwellings have also surrounded many of the lesser kivas and similarly do not permit us to associate lesser kivas with specific groups of dwellings (since we cannot find a group of dwellings) (Wendorf, 1956: 19). Structures resembling Great Kivas, which Wendorf calls "specialized ceremonial structures" (*Ibid*: 20) appeared in a few places, indicating integration beyond the domain of any single community. Still, there is little or no indication that Southwestern communities would develop those features Steward calls "formative" (1949: 19-20)) and which indicate growth toward an era of "regional florescence" with its city states. One reason why this could not occur is probably supplied in the environment itself. The other I would like to explore below.

Post-Classical and Modern Pueblos were still very large, but they were becoming amorphous by standards so far applied to form and grouping. Reed has noted the parallel alignments in the communities, but it does not seem likely that these were clan blocks (Reed, 1956: 19-20), especially in view of Mindeleff's survey of Hopi (1891: 82) and Kroeber's of Zuni (1917: 103-115) both of which indicate great dispersal of clan segments throughout both communities. Two interesting developments have taken place since the end of the Classical Pueblo era. First, Great Kivas have disappeared. In relation to this development all of the gross formal characteristics that separate dwelling from kiva have also disappeared: they are not separated from each other in space; there is now no formal distinction, for the kivas as well as the dwellings are rectangular, and the kivas are no longer underground (at Zuni) but are on the surface of the ground as are the dwellings (except those dwellings, of course, that are on the second, third and fourth floors). In relation to this development Hawley writes:

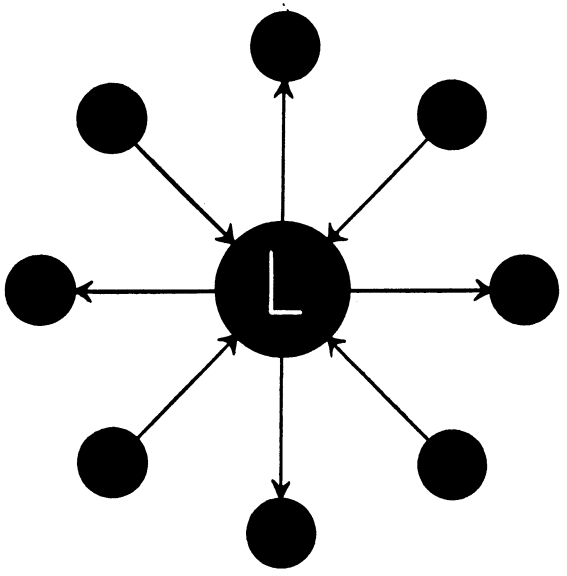
> Unless a kiva built within a house block possessed a ladder-pit or fire place with terraced back (not a universal feature) archaeologists excavating today would be unable to recognize it as a kiva. (1950:299)

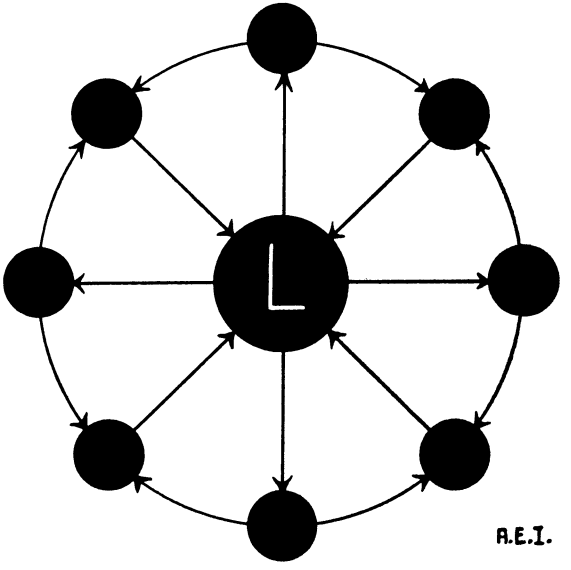
The other interesting development in culture growth is the final step in dwelling relationships: begun in the Great Pueblo period on a small scale (at the Village of the Great Kivas, Kiatuthlanna, Pueblo Bonito, for example) a system of *multiple communication* of rooms is evident, and thereafter began to enlarge as a system until, at Zuni in 1904, Stevenson remarks:

> The houses are so provided with interior doors that almost the entire older portion of the village can be put in communication without passing outside the communal structure. (p. 350)

This, then, marks the end of the two trends I have outlined above. The question remains as to how each may be interpreted and what significance should be attached to the relationship between them. As for the first, it should be noted it has generally been assumed that the Great Kiva (and presumably the Great Pit-House of Developmental Pueblo communities before it) was authoritarian (i.e., *repressive*) in the form of its relationship to the community. This assumption is reasonable on the basis of the contemporary evidence concerning priesthoods and their association with households through the kiva organization and is one of the major sources of the *repressive theory* of Pueblo society. One might go one step further, however, in drawing attention to the sociometric form of experimentally induced authoritarian leadership, the "star", which has the following characteristics: "The authoritarian leader encourages a segregated group structure in which the intercommunication among the parts is held to a minimum and wherever possible the avenues of communication are through him. In this way he ensures the indispensability of himself to the group... there is less opportunity for the development of close... relations among all group members... the withdrawal of the leader... may precipitate chaos in the group." (Krech & Crutchfield, 1948: 425).

There was an architectural similarity to this sociometric form that was manifest even at Alkali Ridge and most certainly is the mark of the communities of the Great Pueblo period where the pattern of polarization of dwellings to the center is outstanding. It is also notable that the formation of sub-groups did *not* take place along lineage or clan segment lines, at least during the





" authoritarian " leadership.

" democratic " leadership

ethnographic present. Now, the transplanting of a sociometric pattern on an architectural site while inferring the same relationship from the latter which was induced in the former may indeed be risky. It would hardly be worth presenting here were it not, also, for the fact that as the intimate system of communication between the "parts" (i.e., the dwellings) took place on a widespread scale, thus destroying the spatial form of authoritarian leardership, at this precise moment, so to speak, in archaeological time, the *Great Kivas disappeared.*[7]

If one looks now at Zuni it will become apparent to him that if there is a central position in an area this belongs to the community as a whole. Kivas are now all equally *centers* (they are not clan kivas). The "front-back" orientation of former pueblos has now completely disappeared and in relation to this peripheral households appear in considerable numbers in fairly remote positions outside the community proper, but always oriented (or polarized) *toward* the community and that section of the community from which their members originally came.

THE DYNAMICS OF INTEGRATION

The Repressive Pattern

The Pueblo people were, it appears, adjusted to the problems that are engendered by the fact that change was rapid, enlargement progressive, and efficient, centralized, coordinating structures never became inclusive in their domain as they did, for instance, in Mexico. It seems then that all problems involving the coordination of activities in the Pueblos were attacked with this set of conditions occurring as a phenomenal and social reality. These conditions were not only appreciated but must also always have been implemented, for there was great stability in the growth trends I have described. There was also, of course, the ever recurrent possibility of civil strife which occurred as late as 1906 at Oraibi (Titiev, 1944). One is therefore brought

[7] There are to-day big kivas and little kivas, as Hawley (1950) points out, but there are no Great Kivas in the Zuni-Hopi areas.

to suspect that there was a large dynamic ingredient infused into the leadership roles of the Pueblos. Bennett has performed a useful service in drawing out the two forms of integration, and what I am trying to indicate is the basis upon which a specific relationship between them prevailed. I have up to this point identified the locus of *repressive* authority with the kivas; I think this conception can now be broadened to include not only a region in the community but a type of role as well which may be regarded as a priestly role. The most dramatic activities associated with this role are to be found in socialization and specifically in the initiation ceremony with its whippings and swearing to secrecy and in the bi-annual visit of *A'Doshle* to the community for the manifest purpose of abducting, castrating, decapitating and eating recalcitrant children whom he threatens with convincing realism. Another aspect of the role is its *collective* nature which preserves individual anonymity both of the ancestors and of the priests who, as masked dancers, perform for their benefit on behalf of the community. This plurality of authority holders only serves to intensify the repressive nature of their relationship to subordinates in developing what Simmel called the quality of *objectivity* which renders irrelevant the needs of the subordinate of "tenderness, altruism and favor..." (Simmel, 1950: 224). Finally, the activities associated most directly with the role were rigidly prescribed and as rigidly controlled. They were the essence of ritual, performed (as Bunzel and Benedict have bent their efforts to emphasize) in measured fashion and never as a "spontaneous outpouring". Even the poetry associated with ceremony was strictly speaking "ritual" poetry (Bunzel, 1932) and lengthy prayers were so carefully rendered as to permit recitation in unison over a period of three to four hours at Shalako.

One can readily associate rigid and repressive authority with the recurrent break-ups that occurred in Anasazi communities — at least, one can appreciate the inability to prevent them. On the other hand, it is difficult to understand the constancy and regularity of culture growth in the Southwest either in terms of this kind of authority or in terms of haphazard events. One then is driven to the hypothesis that a more adaptive form of authority prevailed of the sort that could withstand the

threat and actuality of fission while itself remaining intact, and one that *implemented* growth.

The Organic Pattern

Bennett evidently regarded the two patterns of Pueblo organization as operating at different levels, since he refers (in the above quote) to *repressive measures,* on the one hand, and to *organic world view* on the other. The organic appearance of the community, therefore, does not necessarily reflect the tight fit of *existing* relationships between persons, groups, institutions, etc., but rather the *conceptualization* of such relationships in an overall scheme which is more peculiarly a product of the verbal medium of the culture than of its social organization. Moreover, the function of organic patterning and the authority based upon it — that is, subordination to a set of *principles* rather than to a set of priests (Simmel, *Op. Cit:* 250) — may be regarded in the context of a response to the growing problems of co-ordination in a society of ever increasing size and complexity, and a society evidently which continued to insist on maintaining its patterns of repressive authority and therefore (considering the well-known Pueblan ambivalence to assume authority roles in their communities) which had to legitimate them in an efficient way. The legitimation of priestly roles (and therefore of repressive activities) appears to have come about in at least two ways. First, the roles were accorded the respect of deep extension into the past. Zuni and other pueblos have been profoundly concerned with their creation myths the content of which is largely concerned with the role of essential guidance assigned to the priesthood (Cushing, 1896). But this comes about through the priesthood itself — it is a kind of public relations activity, if I may so phrase it, and therefore self-interested. Furthermore, the *context* in which this occurs (i.e., the ceremonial context) is itself rigid and one that is most effective in maintaining stability in a relatively static and relatively unchanging community. But Pueblo life was, as I have tried to indicate, always dynamic and the Pueblans were themselves permanently and in part successfully adapted to conditions of continuous growth and enlargement. Therefore, the larger, more inclusive, and essentially more fluid concept of authority mentioned above may be regarded

as the more effective source of the sanction for the activities of the priesthoods.

The Zuni concept of the *Directions* will serve to illustrate the way in which one form of *organic* authority legitimates and gives a place to the roles associated with *repressive* activities. According to Cushing, Zuni was conceptually divided on the basis of six divisions in space. These divisions corresponded to six directions (the four cardinal points, the upper and the lower worlds) with a seventh — the middle — which became the synthesis of all the rest. Thus, the principle represented an organization of all existing phenomena relevant to the Zuni at the time, and it served to establish a locus of relationships between individuals, activities, groups and institutions along with the environments associated with these directions and their characteristics. Each of the four cardinal points was associated with a season and each season posed problems peculiar to itself which, in turn, had to be adapted to by plants and animals associated with these directions (or points) and therefore with the seasons. Finally, in association with the plants and the animals were the various groupings in Zuni from which association they derived their sanction for authority to perform in the interest of the community. Eggan writes,

> The directional basis for grouping selected by the Zuni has the advantage of being applicable to a variety of institutions: not only clans are (or were) so organized but kivas, societies, and priesthoods as well. (1950:213)

Thus, the priesthoods were *only one among many* aspects, activities and roles in and of community life that were legitimated in the actual transmission of the content of the Directions.

The principle of the Directions did more, however, than simply legitimate for it also regulated activities by basing them upon a natural order and a natural sequence. Cushing remarks that

> ...no ceremonial is ever performed and no council ever held in which there is the least doubt as to the position which a member... shall occupy in it... (1896:370)

and he concludes,

> With such a system of arrangement as all this may be... mistake in the order of a ceremonial, a procession or a council is simply impossible, and the people employing such devices may be said to have written and to be writing their statutes and laws in all their daily relationships and utterances. (*Ibid:*372)

The Directions were informally communicated "designs" relating to the organization of the world; they were created by the older men of the community — the philosophers, the "men of thought" (Radin, 1956: 230) — and diffused generally. In this capacity (not as priests, even though in other capacities they were priests) these older men indulged unmistakably in *innovative* activity, for the beautifully integrated world which they created out of their own past they also imposed on a social world which, though changing, could be ordered as the Directions ordered it. The Directions could not have maintained *even a remote fit with communities organized around Great Kivas* (as in the former phase of development) for, according to the way in which the Directions were conceived, the Kivas and their associated priesthoods were simply one among the many elements — Zuni *and* other — that were given their place in the overall scheme[8].

Finally, the Directions represent an organic pattern that was related to the settlement pattern in such a way as to suggest that with the disappearance of the Great Kiva this (i.e., the *organic*) became the central form of authority in Pueblo communities. Whereas it is probable that *repressive* authority had been the central or dominant form in the large "classical" Pueblos, the subsequent related events involving the widespread communication of rooms, the disappearance of the Great Kivas, the loss of a distinctive form for all kivas, the loss of the "front-back" orientation and the *conceptual centralization* of the community (given in the Directions), indicate clearly the contemporary relation between the two forms of authority.

[8] Kroeber attacks this problem from the point of view of social organization, indicating that a progressive differentiation of priesthoods took place with enlargement (1919: 872).

THE ZUNI "ROADS"

Although the *Directions* appeared to be effective in coordinating the activities of the Pueblo in 1890, by 1917 they were, according to Kroeber, "so wholly mental as scarcely even to affect ritual" (1917: 97). The question then arises as to whether or not a new synthesis was developing at Zuni at the time of Kroeber's visit. As I have suggested, the concept of *road* or *road of life* as given by the spirits who continue to exercise interest and influence over it was represented at Zuni during Cushing's visit there. On this basis it seems probable that the contemporary concept of the "roads" is related to this earlier phenomenon. The four "roads", then, appear to be a Zuni innovation, but in view of the hypothesis concerning the "authoritative innovator" one would expect other and related changes to have occurred in the Pueblo.

Two related changes in community plans actually were taking place during Kroeber's visit; one of these was the enlargement of individual houses. The other concerned the general drift out of Zuni that Kroeber documents so carefully: 81 houses of 220 had been built outside the community in fairly distant surroundings (Kroeber 1917: 118). One should note that this was not an indiscriminate break-up, but a slow process of orderly decentralization, and one should note also Kroeber's finding that houses were situated along a line of extension from their quarter of origin in the community and that, regardless of position, they all oriented their doorways toward the community. One is induced therefore to speculate on the quality of authority and innovation at Zuni today.

What changes are taking place at the moment are undoubtedly too complex to be dealt with as a whole in a single paper. I would suggest, however, that *repressive* authority (*and* "repressive measures" in Bennett's meaning) is still present in the community. Both from the evidence supplied in the "roads" and from that given by Havighurst and Neugarten, it appears that this type of authority is, however, coming *more and more to be lodged within the Zuni family itself* rather than more

exclusively in the priesthood in particular or in the elders of the community at large (Li Anche, 1937: 70). Thus, as a result of their findings, Havighurst and Neugarten express surprise at

> the Zunis' low mention of the community ('everybody') as a source of praise and blame. In view of their close communal life, we might expect them to be much involved emotionally and much concerned about praise and blame from community members, as is true with the Hopi; and we might also have expected this from statements made by anthropologists that Zuni parents do not punish or threaten their children as much as in most societies, but depend on nonfamily members to do much of the scolding of children. Instead we have found just the opposite... (The family seems to have much more influence as a socializing agent with the Zuni than with any other group except Navaho Mountain.) (1955:201)

My own inquiries concerning socialization at Zuni were in agreement with these findings (McFeat, 1957: III). The following points were easily established: (a) a child's own parents are the proper first disciplinarians, (b) a boy will fear his father more than he will fear his mother, his mother's brothers, his father's brothers or his grandparents but (c) he will also fear those once upward removed by generation who are male and live within or are intimately associated with the household. (He also was positively disposed to the above). I also learned that corporal punishment, while preferably a last resort in a sequence of disciplinary steps, is considered an acceptable and effective mode of discipline.

These statements were given as fact and as opinion concerning facts. But they were poor generalizations other than their being rough indicators of change, for I could rarely at the outset establish the *context* (i.e., the conditions) relevant to a given act of discipline. The older men, again, were able in part to solve this problem in their presentation of the Four Roads which were volunteered not as facts or as re-statements of existing norms but as *idealizations* of individual careers, in the case of the two primary "roads", and they were often spoken of as "the perfect way in the life of a boy or a man". Thus, in spite of their clearly innovative character, they were not regarded by the Zuni as innovations at all, but rather as

ancient and as the gift of the spirits.[9] Whether they were aware of the fact or not, these same older men were, through their transmission of the Four Roads (and probably through many other expressions of their thoughts) attempting to bring about a new synthesis in Zuni culture. It was my judgment at the time that these same men were highly respected, and the respect accorded them appeared to include their command over (and the subject matter of) the "roads". It is for this reason that I feel justified in referring to them as "authoritative innovators".

SUMMARY AND CONCLUSIONS

I have attempted in this paper to show that culture change occurred in the Anasazi region in such a way as to manifest the following trends: rapid growth, progressive enlargement, recurrent fissions and the related developments toward centralization of ceremonial structures and proximation of dwellings. It has appeared to me as an inescapable conclusion from these facts that Pueblans were themselves permanently adjusted to conditions under which communities readily change in these directions. In spite of the recurrent fissions of communities it has appeared that the growth trends were consistent enough in their direction to imply that — despite fissions — there was overall stability in culture change. Therefore, I concluded that Pueblans were not only adjusted to change but they exercised some measure of control over the direction of change and that therefore one could best understand this change from the point of view of those in authority or positions of leadership in the communities. This implied, then, that those in leadership in the communities were in some capacities *significant innovators* as well. Since I identified (both by historical projection from the present and by formal relations) the prehistoric kivas, especially the Great Kivas, with *repressive* authority, and therefore one

[9] Since they clearly *were* innovations, it appears that a phenomenon similar to Robert's *acognitive* shift was operative (ROBERTS 1951:81).

form of leadership, I came to the conclusion that this (repressive) form could not reasonably be regarded as productive of innovative change. I suggested, therefore (again, by projection from the present) that if another form of authority must be hypothesized for the past that this be *organic* authority, and I attempted to bring some evidence directly to bear on the contention that *repressive* authority was present (especially during Classical Pueblo development) and that its chief purveyor, the Great Kiva, disappeared as intimate relationships between dwellings grew. The implication became, then, that the growing communication of the dwellings of communities as wholes (in accompaniment with the loss of front-back and the centralization of the community in an area) could be interpreted as an *organic* development based as it is upon two things: subordination to a *principle* (Simmel, *Op. Cit.*) and upon self-abnegation, moderation in all things, cooperation, etc that are seemingly appropriate with intensely clustered living which is at the same time intimate.

The complexity of communal organization and the conditions, of change, then, produced what might be called communal philosophers — men of power, authority and prestige — for whom a speculative type of role was left open. One might put the matter another way: conditions were such as always to present to this class of persons discrepancies between the world of value and the world of reality (Kroeber, 1952). Specifically, one function of the philosopher is that of demonstrating some significant and meaningful relationship between the two (*Ibid:* 159). In the theoretically perfect folk community, where value patterns and reality patterns are one and the same thing (Redfield, 1946) there would be no need to bring about an approximation between the two, therefore there would be no philosophers. Similarly, in the community of perfect *anomie* where there *could* be no bringing of the two together there would be no philosophers. But where change was regular but orderly, and where complexities tended to develop, then philosophies in the order of the *Directions* could be expected to appear and to be inclusive, even to include in its place, institutionalized repressive authority. We have Cushing's report concerning the efficacy of the *Directions* to organize life in Zuni.

But there is also Kroeber's report to the contrary only thirty or forty years later. It appeared, then, that the *Directions* emerged as an innovative response — they met and helped organize a complex situation, and they disappeared as the situation changed.

Finally, the data which I secured at Zuni and which are clearly innovative — the Four Roads — were on the one hand idealistic, "value" phenomena. I felt that in order to indicate the innovative roles of the older men who produced them and, moreover, to present these "roads" as philosophies (and therefore also reality-directed as well as value-oriented) in the sense given above, that changes should be shown to have occurred in the community which were reflected in the "roads". This I trust I have demonstrated.

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The Flexibility of Eskimo Social Organization

BY W.E. WILLMOTT

In a recent paper, Professor Honigman has discussed "flexibility" as one element of the Eastern Arctic Eskimo ethos.[1] He defines flexibility as "a relaxed mode of procedure and tolerant attitudes toward demands of living."[2] He is particularly interested in the ideational implications of this aspect of ethos, the Eskimo's "passive resignation toward disasters" and lack of elaborate theories of causation, which he relates to child-rearing practices that reinforce a flexible approach to the problems of living.

The present paper will amplify this point by giving examples of flexibility in social organization and by discussing its practical and theoretical implications. The material for this paper was gathered during the summer of 1958 at Port Harrison, P.Q., a community on the east coast of Hudson Bay where most of the 340 Eskimos live in small local groups of 30-50 individuals in 6-10 households.[3]

In an article in the *American Anthropologist,* Professor Embree has suggested that "loose integration" is a functional aspect of Thai culture[4]. It is possible to make a similar case in Eskimo culture for the equivalent concept, flexibility. Rigorous

[1] HONIGMANN, John and Irma, "Notes on Great Whale River Ethos," Anthropologica, N.S. Vol. I, 1959, pp. 106-121.

[2] *Ibid.,* p. 119.

[3] The field work was conducted for the Northern Research Coordination Centre, Department of Northern Affairs and National Resources, Government of Canada, Ottawa. I express my thanks also to the McGill-Carnegie Arctic Scholarship Committee, to Mr. Vic Valentine, chief research officer of the NRCC, and to Mr. Asen Balicki of the Anthropology Section, National Museum of Canada, Ottawa, who have discussed with me some parts of this paper.

[4] EMBREE, John F., "Thailand — a Loosely Structured Social System," *American Anthropologist,* Vol. 52 (1950), pp. 181-193.

life in the Arctic, where subsistence itself is a never-ending challenge to man's ingenuity, demands a high degree of inventiveness and at the same time prevents the establishment of large, permanent local groups where patterns may become formalized and endowed with value.

To say that flexibility is functional does not, of course, explain its presence as a part of Eskimo culture. No attempt is here made to explain its origin, but only to describe and discuss it as it exists today. The following sections will deal with flexibility in the areas of family organization, kinship terminology, community organization, and recreation. The implications of flexibility for the acculturation of the Eskimos to majority Canadian patterns will then be discussed, and the paper will conclude by raising some theoretical questions arising from this concept.

Family Organization

If it is possible to speak of a basic social unit in any society, as Professor Leighton suggests it is,[5] the household is such a unit for the Eastern Eskimo. Most activities of both men and women take place for or in the household. Nevertheless, many of its functions are variously fulfilled for its members by other households in the local group, and even its membership may change from time to time.

A new household may or may not be established by a marriage. Depending on expediency, a newly married couple may live with the parents or either spouse or may build their own snow house or tent. Locality can be described as flexible.

The normal form of marriage is permanent monogamy. This is so, not because of strong moral or ritual circumscriptions that would tend to make marriage a rigid form, but because monogamy is the best adaptation to the cultural-ecological circumstances: a man cannot live without a sewer-cook-bootsoftener, and each woman must have a hunter-trapper. The attempts of

[5] See in YATSUSHIRO, Toshio, "Economic Development and Culture Dynamics," *The Eastern Anthropolgist*, Vol. 7, no. 1 (Sep-Nov., 1953), Lucknow University, India p. 13.

the missionaries to make a value of permanent monogamy have not succeeded. This is evidenced by the large number of marriages without benefit of church (almost two-thirds of the 60 couples) and by the other forms of marriage in the area: two polygynous households, two households in which the women provide for the economic needs of several brothers, and two cases where men have sent unsatisfactory mates away and taken new wives, although the first marriage had been in the church in each case. The form of marriage could be described, therefore, as flexible monogamy.

Although the household normally provides for the daily needs of the individual — food, sleep, closing, sexual satisfaction — these needs may be satisfied in any household in the local group. Young boys may sleep in various tents if it is convenient, for instance, and a person may eat as much as he likes anywhere. When the fish are brought in from the nets, everyone around may partake of raw fish. Similarly, prayers involve whoever is in the tent at the time. From the point of view of function, then, the household can be described as a flexible unit.

The household is rendered flexible in membership by the mechanism of adoption, which distributes children between households more equitably than does nature alone. Where infant mortality is high (294 per 1 000 in the past decade) and epidemic and starvation are likely to wipe out several individuals at a time, adoption aids group survival. Death, sterility, or old age need not be reasons in this society for a couple to lack children so long as there are children in the area.

Adoption is much more prevalent among Eskimos than among other Canadians. Of the 175 children under fifteen in the area of study, 28 are adopted, or 16%. In most cases, the child is adopted because the adopting couple want a child rather than because the child does not have a home. Very few of the adoptions are registered with the RCMP, and several of them involve re-adoption by other parents.

If family forms are as flexible as here suggested, one could expect a high degree of variation and a wide range of types.

This is in fact the case, for the 65 households range in size from two to thirteen individuals; 21 include more than one nuclear family, whether paternal relative, another nuclear family, a lodger, or the child of an unmarried daughter.

Kinship Terminology

Eastern Arctic Eskimo kinship terminology is quite specific, distinguishing between affinal and consanguinal, male and female, younger and older, and often between agnate and distaff. For instance, the term for father's brother, *akkak*,[6] is different from the term for father's sister, *attsak*, parent-in-law, *sakiak*, or mother's brother, *angak*. The system of terminology could be described as bifurcate collateral.

But the use of these terms as observed in the field does not stick to their meanings as given by an interpreter. For instance, *ningaut*, daughter's husband, may also be used for cousin's husband. I found nine terms in use between cousins, none of which distinguished anything but sex and relative age. And *akkak*, father's brother, was found in use variously to describe father's brother, father's brother's son, father's sister's son, husband's brother, husband's brother's son, father's sister's husband, and mother's brother's son. Other terms are used with similar flexibility.

In fact, kinship terms appear to be used with little regard for their literal meanings, although these meanings are recognized by the Eskimos as specific. Rather, they are employed as they seem appropriate to the nature of the personal relationship between the two people. If a man looks on an older male cousin as belonging to an older generation, the term *akkak* may seem more appropriate than the literally correct term, *qatanguk*, first cousin.

[6] Eskimo words are spelled according to the orthography developed by Dr. Gilles Lefèbvre of l'Université de Montréal, which may be obtained from the Northern Research Coordination Centre, Department of Northern Affairs and National Resources, Ottawa, Canada.

Kinship terminology is also used between unrelated individuals in the same local group, especially among children. Social relationships are given kinship status in order to cement the local group together. Thus, all children of similar age in a camp may call each other *qatangutik,* a term that was variously described to me as meaning "first cousin," "any relative of the same age," or "someone born in the same camp."

Community Organization

In the area of Port Harrison, there are seven local groups. The largest of these, comprising eighteen households, in located at the "white" settlement, where, according to a rule established by the local RCMP, only those families may settle one of whose members is working for the "whites." The other six local groups are camps from five to fifty miles from the settlement, made up of from six to ten households each.

As far as we can determine, the traditional Eskimo local group was a small, flevible, nomadic band without clearly defined leadership roles, and whose membership varied somewhat from year to year as different families decided to camp together after each summer gathering.[7] This pattern has changed under the impact of varying contact situations. Around Port Harrison there are three quite distinct patterns of community organization today: 1) the organized camp, 2) the loosely conglomerate camp, and 3) the settlement. These can best be described through a historical review of their origin.

With the advent of the fur-trader — about fifty years ago in this area — camps became more stabilized in both membership and location because of the establishment of individual traplines that were maintained from year to year. The trading companies found it efficient to work through one spokesman for each group, and this man's position of leadership was assured by the policy of presenting peterhead boats and whaleboats

[7] HAWKES, E.W., *The Labrador Eskimo,* Mémoir 91 (Anthropological Series No. 14) Geological Survey, Department of Mines, Ottawa, 1916, p. 110. See also WEYER, Edward M. Jr., *The Eskimo, Their Environment and Folkways,* Yale University Press, New Haven, 1932 p. 209ff.

to spokesmen in order to assure their loyalty. Since these boats then became the primary means of summer transportation and hunting, the owner controlled the movements of the camp and assumed a position of considerable power. Three of the six camps around Port Harrison have powerful leaders who own large boats, and each has a stable population with a strong feeling of differentiation from the other camps.

As the fur trade declined, many Eskimos moved into the settlement and lived on relief, sharing the food of relatives who were employed. This centripetal tendency, which has been observed throughout the Arctic by students of Eskimos, was brought to a sudden halt in 1957 by a local RCMP (police) order that all Eskimos not employed in the settlement should relocate in camps. This order reduced the Eskimo population of the settlement from 27 households to 13, from about 135 persons to 75. Those who left the settlement moved about five miles away, where they established two loosely organized camps that fragment in winter and come together for early summer. Another camp, whose leader died without logical successor, has also become a loose conglomerate of households. These three camps appear to be the most similar to pre-contact patterns of community organization.

Those households that remain in the settlement itself are held there by the existence of the "white" establishments, not by indigenous community organization. There is no recognized leader of the Eskimo community, although the Hudson's Bay Company interpreter acts as a middleman whenever orders are given or requests received by "whites."

Although private ownership of the tools and product of the hunt was recognized in the traditional band, weapons and tools not in use by their owner could be used by anyone in the band, and food was shared to assure that no one in the band would starve as long as there was supply. These patterns were maintained during the rise of the fur trade, and are still maintained today in regard to the hunt. Wages, however, are not shared, primarily because they are part of a new economic pattern that includes majority Canadian ideas of private property. Also, they are not in a form that can be readily shared, since they are

paid as credits at the Hudson's Bay Company store and turned into goods as the wage-earner requests it. Similarly, earnings from trapping and soapstone carving are not shared but belong to the individual.

Since the camp depends on the hunt for half its food, patterns of sharing make households more interdependent than the households in the settlement, where each lives primarily from wages. Hunting is undertaken by settlement Eskimos only as a supplement to their diet, and sharing is therefore only incidental now to new pattern that is replacing it.

Eskimo community patterns have been able to change several times in a few short decades, each time adapting to the changing economic situation. The existence of several patterns within the same area and within the lifetime of these Eskimos indicates that these patterns are flexible, without strong values attached.

Recreation

What might be considered a trivial example of flexibility, but one that nevertheless makes the point clearly, is recreation. Aside from the traditional forms of recreation, the most important of which is visiting, the Eskimos have adopted various new ones from traders and other agents of civilization. From the point of view of this paper, the interesting thing about these borrowed forms of recreation is that they become less formal and more flexible, although highly structured in their original patterns. Two examples will indicate the de-structuring of games among the Eskimos.[8]

The Eskimos in the camp I studied played cards every night. The women preferred a type of 500 rummy, although the men played five-card games that were entirely new to me. The rules of 500 rummy changed from hand to hand, the person next to the dealer determining the winning score, the wild cards,

[8] A detailed description of Eskimo games and other forms of recreation may be found in my report to the Canadian Government (WILLMOTT, W.E., "An Eskimo Community," Northern Research Coordination Centre, Department of Northern Affairs and National Resources, Ottawa, 1960.

and the number of cards to be dealt on the first hand. On subsequent hands the revolving dealer decided how many cards to deal, depending on her own advantage, so that every hand became a different type of game.

Checkers are played by some Eskimos. As with other gambling games, the rules change after each game, depending on the alternate choice of the two players. Some of the observed variations were: pieces may move only forward but may jump backward as well, kings may move the entire diagonal of the board, kings may jump the entire diagonal of the board, pieces may jump backward only as the second jump of a double jump that has started forward.

Perhaps the best example of a formal pattern of recreation that has been de-structured by the Eskimos is the square dance. Learned originally from whalers and traders, Scots and French-Canadians, the dance has changed to allow any even number of couples instead of the originally prescribed four. Individuals may use a shuffle or a jig-step, and the figures follow each other according to the whim of the lead couple rather than by the original plan of the dance.

Each aspect of Eastern Arctic Eskimo social organization exhibits flexibility. The four examples given here are intended as an illustration of this characteristic, not as proof of the proposition. Its implications to the acculturation of the Eskimos and to social theory remain to be discussed.

Flexibility and Acculturation

There is no doubt that flexibility had much to do with the fact that "white" contact with Eskimos has been relatively free of conflict at Harrison. Since action patterns were not rigid nor heavily value-laden, they could adapt to the changing situation without the Eskimo feeling and overwhelming sense of loss. Changes introduced by "whites" were not viewed as threats to an Eskimo way of life, but rather as a factor of environment to which the Eskimo must adapt with the same approach he has always used in adapting to a hard and capricious physical environment.

The Eskimo's attitude toward the environment is summed up in the word. *"arunamut,"* which literally means, "because nothing can be done," and implies "therefore we must face the situation without regret."[9] Ever since the white man entered the Arctic, the Eskimo has said *"arunamut"* to all his incomprehensible antics. "White" economy, then "white" religion, and finally "white" political authority have penetrated Eskimo society, wrought far-reaching and irrevocable changes on it — often without the understanding of the Eskimos involved. Yet these changes have not been overtly opposed by the Eskimos. Rather, the social organization has adapted to the changes as it would adapt to a natural disaster or an environmental change.

The *arunamut* attitude has resulted in the unfortunate consequence that the Eskimo now accepts as an environmental axiom much that in democratic society is considered an individual's prerogative and duty. For instance, the right to choice of religion, the right to choice of residence, the duty to work, the right to bargain as a free agent, and the right and duty of education for children do not involve individual choice among Eskimos in this area.

But if this flexibility has led to domination of Eskimo society by various white agents, it may also abet the process of acculturation that the Eskimo is still undergoing. For even rapid social change need not produce conflict in a society where behavior patterns do not appear as rigid values, where the social organization is suited to easy adaptation. The problem facing the administrator in such circumstances is not one of gradualism to avoid conflict, but rather the destruction of the relationship with "whites" that involves the acceptance of "white" authority as an incontrolable part of the environment, and the reassertion by the Eskimo of control over the areas of culture that have been in "white" hands. Fundamental in this process will be the realization of economic independence of the Eskimo local group, its ability to act as a free agent in economic matters.

[9] The attitude and word *arunamut* are not new with this research, but have been recorded by many writers. See, for instance, DE COCCOLA, Raymond, and Paul KING, *Ayorama,* Oxford University Press, New York, 1956. The title of this book is derived from the same root in the dialect of the Coppermine Eskimos of the Central Arctic

Theoretical Significance

The flexibility of Eskimo society provides interesting theoretical problems for the anthropologist, which are ultimately of practical importance as well. Firstly, it raises the question of the integration of each social unit in the society. For if patterns of behavior are not standardized as values and defined to include deep emotional content, what produces solidarity and integration in the society, in the local group, or in the family?

The question of family integration is one for particularly fruitful speculation and research by social scientists. The family, or household, is the basic unit of traditional Eskimo society. One would therefore expect it to be highly integrated, with strong interdependence not only on the economic level, but on the personality level as well. But the relative ease with which children are passed from one family to another, and the apparent lack of personality damage to children resulting from even repeated adoptions, indicates that ties between parents and children are easily broken, easily made. This would suggest that neither parent nor child feels an overwhelming sense of unique relationship.

But if this is so, and since we know that personal identity is learned from relationships, how does the child gain his sense of personal identity? How does he learn to understand his status and, consequently, his relationship with other members of the society? It has been assumed by social scientists interested in socialization that this personal identity comes from the unique relationship between parent and child, especially mother and child. Is it possible that personal identity may develop without such a unique relationship that is defined as endowed with extraordinary emotional intensity? These are only premilinary queries that come to mind on what is obviously a highly complex subject.

The question of the integration of the local group provides another area of profitable research for the anthropologist. It seems evident that kinship is not the primary mechanism for integration of the organized camp, but rather the economic cooperation of the households, primarily in the use of a large

boat. These camps appear to persist, however, in places where they have ceased to function as economic units.[10] The historic evolution of the status of leader and its present demise offers an interesting developmental study of status.

Secondly, the relation between flexibility and integration provides interesting theoretical speculation for the functionalist. Flexibility "does not mean that the society is poorly integrated," Embree states:

> On the contrary, the loose integration is a functional one, allowing not only variation in individual behavior, but also in national behaviour. [In Thailand] it had a survival value which may well go back to the early days of extensive Thai migrations and which has served the nation well to this day.[11]

Embree points out that the functionality of flexibility in this case raises the theoretical question of whether it is possible to find a one-to-one correlation between "needs" and structure.[12] Although he does not mention Malinowski, this appears to be a suggestion that Malinowski's functionalism needs restudy in the light of these findings.

Malinowski has stated that function is defined "by showing that human institutions ...are related to primary, that is biological, and derived, that is cultural needs. Function means, therefore, always the satisfaction of a need."[13] By this definition, and by reading Malinowski on acculturation,[14] one gets the impression that in his view, specific needs are satisfied by specific social units, which change under impact from functionally equivalent units in another society.

But in Eskimo society it is often difficult accurately to define the functions (by Malinowski's definition) of any social

[10] Mr. Asen Balikçi reports that at both Great Whale River and Povungnituk Eskimo settlement patterns follow the lines of previously separate camps, which he calls *demes*. ("Relations inter-Ethniques à la Grande Rivière de la Baleine, baie d'Hudson, 1957," (MSS) National Museum of Canada, Ottawa, 1958, p. 23 and *passim*.)

[11] EMBREE, *loc. cit.*, p. 191.

[12] *Ibid.*, p. 192.

[13] MALINOWSKI, Bronislas, *A Scientific Theory of Culture*, University of North Carolina Press, Chapel Hill, 1944, p. 159.

[14] See, for instance, KABERRY, Phyllis, "Introduction" in MALINOWSKI, *Dynamics of Culture Change*, Yale University Press, New Haven, 1945.

unit, for they change in different circumstances. One might go further and suggest that the four-fold description of function proffered by Parsons[15] does not produce fruitful results in a situation where institutions are not clearly defined and their functions vary according to circumstances.

Summary

Eskimo social organization is characterized by what Honigman has called flevibility — a lack of rigidity and value associated with conventional ways of doing things. The flexibility may be seen in family organization, in the loose definition of the form of marriage, in the casualness of adoption, and in the informal approach to functions. It may be seen in the non-literal use of kinship terminology, which emphasizes the personal nature and closeness of the ties between two people rather than their actual kin relationship. It may be seen in the various patterns of community organization and the ease with which one changes into another. And it may be seen in the recreation patterns that become de-structured when practised by the Eskimos.

Flexibility has allowed acculturation to proceed with relative lack of conflict, and it may do so in the future. On the other hand, it produces the problem of lack of control by the Eskimos over many aspects of their society — education, religion, political authority, and especially economic organization — that are today entirely in the hands of "whites."

Finally, the flexibility of Eskimo social organization raises the interesting and researchable theoretical questions of the integration of various social units and the functional approach to institutions of Malinowski and Parsons.

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[15] See, for instance, Talcott PARSONS, and Neil J. SMELSER, Economy *and Society,* The Free Press, Glencoe, Illinois, 1951.

Du subjectivisme en psychologie sociale

par Bernard MAILHIOT, o.p.

Ces lignes voudraient servir de prise de conscience aux psychologues sociaux et montrer comment leurs recherches sont quelquefois biaisées, sinon faussées par les jugements de valeur. A cette fin nous voudrions confronter les données acquises récemment par la psychologie sur le problème de la perception sociale avec ce que nos propres recherches sur les conditionnements psychologiques des relations interethniques nous ont appris des exigences méthodologiques de l'expérimentation en psychologie sociale.

La psychologie sociale, peut-être plus que toute autre science du social, a beaucoup de peine à s'objectiver à l'égard des phénomènes qu'elle a pour tâche de nous rendre intelligibles. La sociologie, par exemple, dans la mesure où elle réussit à se garder de tout psychologisme, peut réussir à décrire, puis à expliquer le social avec des garanties optima de validité. Et cela parce que la sociologie atteint le social dans ses *dimensions formelles,* celles qui sont le plus directement accessibles à une observation systématique. Mais parce que la psychologie sociale s'essaie à repérer les *dimensions fonctionnelles* du social, elle doit la plupart du temps procéder par inférences. Et ce travail d'inférences lui est rendu d'autant plus compliqué qu'il s'opère à l'égard d'un devenir, dont le psychologue est non seulement partie intégrante, mais souvent à son insu l'un des multiples déterminants, avec lequel en tout cas il est en constante interaction. Aussi est-il peut-être de tous les chercheurs des sciences de l'homme celui qui peut le plus inconsciemment substituer à ce qui devrait être un schéma opérationnel d'expérimentation, des cadres de référence relevant d'une échelle de valeurs qui conditionnent et déterminent, sans qu'il puisse y échapper, ses propres comportements et ceux de son milieu. Bref le psycho-

logue social, pour que sa démarche soit et demeure scientifique, doit au préalable devenir lucide à l'égard de tout ce qui subjectivement le rend indissociable de son objet. Essayons de procéder à cette catharsis!

1. Faut-il rappeler qu'en psychologye sociale, comme d'ailleurs à tous les niveaux de la hiérarchie du savoir, nos intelligences créées réduisent le réel aux dimensions de nos pauvres boîtes crâniennes. Même les plus grands génies dépouillent le réel de son mystère pour qu'il devienne assimilable à leurs schèmes mentaux, nécessairement étroits et étriqués. Ils transforment fatalement les phénomènes qu'ils observent en abstractions et dégradent en petits problèmes, facilement manipulables, ce qu'ils perçoivent comme trop complexe pour une seule saisie de leur esprit. Le réductionisme, inévitable à l'intellect humain, est notre première pierre d'achoppement et constitue un subjectivisme de base. On ne peut le rectifier qu'en acceptant sa condition de créature.

L'histoire des sciences, et de la psychologie pour ce qui ici nous préocupe, peut contribuer à nous rendre conscients à l'égard de nos options initiales, de ce que nous considérons comme les postulats de notre travail de recherches. L'histoire nous apprend au terme de quels tâtonnements les vérités les plus simples ont été découvertes et à quel point les esprits même les plus grands n'échappent pas aux contextes mental et historique du milieu où ils ont élaboré leurs systèmes respectifs.

2. La psychanalyse nous a appris par contre à quel point nos structures mentales sont conditionnées et colorées par nos états émotifs, combien nos systèmes de pensée et nos plus brillantes théories répondent souvent au dedans de nous à des besoins très infantiles de sécurité et de compensation, et peuvent faire le jeu de nos mécanismes de défense les plus primitifs. Quel homme de science peut avoir la certitude que l'orientation et l'articulation de ses recherches ne sont pas affectées par ses obsessions, ses angoisses, ses moments de dépression et tout ce qu'une émotivité soi-disant adulte charroie de reliquats et de résidus, jamais assimilés ou éliminés, des traumatismes de son enfance.

La lucidité à ce plan s'acquiert péniblement. Il serait souhaitable que le psychologue social, comme tout homme de science, se livre périodiquement à une auto-analyse méthodique des motivations inconscientes qui imposent à son insu leurs déterminismes aux initiatives de son esprit. Dans certains cas, seule une psychanalyse peut procurer cette objectivation indispensable à ceux dont le métier est de faire prévaloir la vérité sur les mythes, les stéréotypes et les clichés ambiants.

3. Chaque culture a son système de valeurs, lesquelles inspirent, conditionnent ou déterminent les conduites sociales et les comportements de groupe des individus qui la composent. Ces valeurs culturelles constituent un système ou une échelle dans la mesure où elles nous offrent des patrons ou des modèles de conduites, des schémas d'adaptation. Certaines de ces valeurs sont *explicites:* on y adhère ou on les rejette consciemment. Elles sont inhérentes et indissociables des coutumes ou des modes de vie. Elles deviennent les caractéristiques manifestes du groupe qui les a adoptées. Mais souvent ces valeurs explicites sont l'expression déguisée ou déformée de valeurs *implicites*. Et ces dernières imposent des déterminismes beaucoup plus rigides à nos comportements. Privément ou collectivement, nous consentons rarement à nous avouer qu'elles servent de mobiles ultimes à nos actes, car le plus souvent elles nous demeurent inconscientes et orientent nos agirs à notre insu.

Comme membre d'une culture donnée, le psychologue social, n'échappe pas aux pressions et aux contraintes de son milieu. Mais comme il lui importe de devenir lucide sur ses propres structures mentales et affectives, aussi il lui devient nécessaire de prendre conscience des interdits, des mythes et des tabous constituant l'inconscient collectif de sa culture. L'anthropologie culturelle, qui se consacre de plus en plus à l'étude comparée des cultures existantes, lui sera alors d'un grand secours. Elle l'aidera à opérer un décalage objectif entre les valeurs explicites et les valeurs implicites de sa propre culture, sans verser pour cela dans des mécanismes d'auto-accusation ou d'auto-justification, comme il arrive souvent aux chercheurs en sciences sociales. Il ne s'agit pas pour lui de renier ou de faire l'apologie des valeurs culturelles de son milieu, mais bien de retracer les

prémisses de sa propre culture en vue de montrer comment elles commandent, une fois introjectées par ses membres, leurs attitudes, leurs perceptions et leurs conduites en groupe. Ce faisant il réussira à donner une intelligence scientifique des phénomènes de groupe qu'il observe.

4. Le phychologue social peut buter à une dernière pierre d'achoppement: élaborer ses hypothèses de travail à partir d'apriorismes, empruntés à certains engouements de son milieu professionnel. Il risque alors de considérer comme postulats à ses recherches des théories qui sont loin d'être prouvées, dont le seul mérite est d'avoir été formulées, le plus souvent sous forme d'extrapolation, par un grand nom ou un grand esprit, en terme apparemment plausibles. A côté de ses valeurs personnelles, des valeurs culturelles de son milieu, certaines valeurs professionnelles peuvent donc à leur tour venir fausser la rigueur et la validité de ses expérimentations. Ce qui n'était souvent qu'hypothèse pour leurs auteurs peut se muer sous l'influence d'émotions de groupe (les psychologues n'échappent pas à cette loi), en valeurs i.e. en vérités indiscutables ou en prémisses indispensables au travail du chercheur en psychologie sociale.

Selon l'école de pensée à laquelle il appartient, le psychologue social aura tendance à opter pour l'un ou l'autre des postulats suivants:

(a) ou il sera enclin à adhérer à l'*approche atomistique* voulant que ce qui est plus petit soit plus fondamental et essentiel que ce qui est plus grand. Ainsi ses recherches seront entreprises selon des *schémas réductionistes;*

(b) ou il considérera comme allant de soi de postuler l'existence subjective correspondante de tout ce qu'il réussira à observer soit dans le monde des stimulus ou le monde des réactions, expérimentant ainsi selon des *schémas béhavioristes;*

(c) ou enfin cédant au *penchant généticiste,* il postulera que ce qui est génétiquement antérieur est plus fondamental que ce qui est génétiquement postérieur.

Le remède le plus efficace, semble-t-il, pour s'immuniser contre une forme aussi subtile de subjectivisme, consiste pour le psychologue social à consentir à travailler en équipe. D'ailleurs les exigences de son objet lui en font un devoir. Les phénomènes de groupe étant multidimensionnels, il ne peut, au moins présentement, espérer les explorer validement qu'en faisant appel à l'usage simultané et complémentaire de multiples techniques d'observation et de sondages. Toutefois il ne doit pas se contenter de se lier à des chercheurs, devenus maîtres dans le maniement de telle ou telle technique. Il importe en plus que ses coéquipiers et lui consentent, en raison même de leurs divergences de vue et de la différence de leurs optiques, à procéder périodiquement et ensemble à une autocritique des schémas d'expérimentation qu'ils utilisent. Ainsi réussiront-ils à se libérer de tout apriorisme et à mettre à point des cadres de référence qui leur permettront de se consacrer à des observations systématiques et à des interprétations rigoureuses avec des garanties minima d'objectivité.

Ainsi que tout chercheur, le psychologue social ne peut progresser dans ses recherches que dans la mesure où il parvient à accepter les limites et les faiblesses de l'esprit humain. Même les génies les plus richement doués ne sont capables que de peu de vérité à la fois, et les vérités péniblement acquises qu'ils atteignent ne sont que des vérités provisoires et tellement fragmentaires.

Néanmoins, et ceci nous prévient de perdre cœur et de verser dans l'agnosticisme, notre intelligence est insatiable de vérité. Ce que certains psychologues ont appelé "our need for closure", explique que le plus souvent nous bouclions trop vite et qu'à cause de nos démarches hâtives nous n'aboutissions qu'à des conjectures ou à des hypothèses. Mais aussitôt notre esprit se reprend, en quête de vérités plus adéquates, cédant, relativement pour peu de temps, aux mirages et aux envoûtements de ce qui peut paraître brillant mais qui n'est pas vrai.

Le psychologue social se doit de poursuivre ses recherches sur la dynamique des groupes. S'il a soin de ne pas préjuger

des faits qu'il observe pour les accorder aux différentes échelles de valeurs auxquelles il adhère, ses théories peu à peu, dans la mesure où elles sont valides, permettront aux hommes de devenir plus attentifs aux mobiles et aux motifs qui conditionnent successivement la formation et l'évolution de leurs groupes.

Un jour viendra peut-être où, selon le souhait d'André Malraux, les hommes auront ainsi appris à dompter "les monstres de leur destin collectif"!

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Religion and Witchcraft: Spanish Attitudes and Pueblo Reactions

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It has for many years been recognized that the witch beliefs of present-day Indian inhabitants[1] of the Southwest parallel to an extraordinary degree those common in sixteenth and seventeenth century Europe. The question of parallelism versus borrowing is an important one, although not central to this paper. Our primary purpose is rather an investigation of Pueblo supernaturalism (both witchcraft and religion) as it must have appeared to Spanish colonial administrators and churchmen, and the extent which this interpretation guided the policy of ecclesiastical and civil organs of the Province of New Mexico. But it is first necessary to review some of the findings relating to Pueblo witchcraft, for it is the theme of this small study that European ideas of witchcraft provided a frame of reference for colonial action.

Parsons (1927: 116; 1939: 1068) enumerates the traits which she considers to be of Indian origin and those which she regards as adapted from the superstitions of the Spanish colonists who first came into contact with the aboriginal peoples

[1] By witchcraft we understand those supernatural practices carried out for essentially malevolent purposes. In the long run, what is regarded as witchcraft depends on the distinctions that the group makes between social and anti-social uses of supernatural power. Functionally, all forms of witchcraft have much in common, although the methods and techniques change from group to group. It is important to note that the similarities that existed between European and Southwestern witchcraft were not only in function, but also in content and form.

In dealing with the Southwest we have restricted ourselves to the settled Pueblos which lived in close and continued contact with Spanish colonists, that is, those Pueblos of the ancient Provincia de Nuevo Mexico. In dealing with these groups, some generalizations have been inevitable. Navaho and Apache conceptions of witchcraft also show a number of similarities with those of sixteenth and seventeenth century Europe, but these manifestations are outside the scope of this paper. (see for example, KLUCKHOHN 1944)

of the Southwest. She gives as elements of European folk belief such traits as the witch doll, transformation into domestic animals, the conviction that witches travel on balls of fire and that their footsteps are inaudible. The idea that the ware-animal which is wounded is subsequently found as a wounded person she classifies as European, as also all beliefs relating to the magical import of hair cuttings and nail parings.

As distinct from the above, some traits appear to be both aboriginal and Spanish and thus present before contact in the folk practices of both groups. The general concept of animal metamorphosis is apparently one such case, while the organization of witches into societies may be another: the witch societies of the Pueblos finding their counterpart in the coverns of the European witches, Sicness, crop failure, and storms are catastrophies which appear to be independently attributed to witches in both Spanish and Indian folklore.

A number of elements present in Pueblo religion — as distinct from recognized witchcraft — also struck the Spanish as smacking of witchcraft. The wearing of masks at katchina dances must have looked especially diabolical, for we know that the witches of seventeenth century Europe were reputed to array themselves in skins and ritual masks. The same is true of the whole kiva complex, the similarity between Indians performing secret rites in underground chambers and the witches' sabbat held *in loca subterranea* is apparent.

The curing societies of the Pueblos are primarily devoted to fighting witchcraft, but such groups, even if their true function had been recognized, might not have fared better than regular witches, for in orthodox opinion no leniency must be shown to theurgy. Furthermore, it is also true that some of these societies, especially the clown ones, are not always free from witchcraft. (Parsons 1927: 116)

It will perhaps suffice to mention one other factor that must have influenced Spanish authorities: the question of narcotics as part of the witches' pharmacopoeia. The European witch was believed to possess special knowledge of herbs and drugs. Some of these drugs, such as those derived from hemlock (*Conicum*

maculatum), may have been used as potions to harm the victims of the witches, others, likes belladona (*Atropa belladona*), were probably self-applied by the witches in order to produce hallucinations of flight and vascular excitement. (Wickwar 1926: 37) The effects of belladona and similar drugs are akin to the results produced by certain narcotics used among various North American groups. In northern Mexico and the Southwest the usual narcotic plant is peyote.[2]

Early Spanish sources on New Mexico comment on the demonic nature of peyote. The reports of Father Perea, agent of the Holy Office in New Mexico during the years 1631 and 1632, mention that the taking of this plant enables the person to have visions, hallucinations of flight, and predict the arrival of voyagers from New Spain. (Scholes 1935B: 220) All the evidence, therefore, indicates that Spanish missionaries and administrators in colonial New Mexico felt that they were dealing with a known phenomenon; a form of witchcraft very similar in pattern to that encountered in their homeland.

It need hardly be stressed that for the majority of sixteenth and seventeenth century Europeans witchcraft was a very real and dangerous thing. Even in Catholic Spain, a country with an enviable record of objectivity and clearheadedness in matters of witchcraft, there was never a denial of the existence of witchcraft, but rather the operation of a strict code of evidence for determining guilt.[3]

A question of great importance in understanding Spanish behaviour in the Southwest has to do with the differences in

[2] Peyote (from the Aztec peyotl) is a small cactus, *Lophophora willamsii*, which grows wild in the Rio Grande valley and to the south. Nine psychotropic alkaloids are contained in natural peyote (generally taken in the form of the dried "peyote button"); some of these are strychine-like, others, notably mescaline, hallucinogenic. (for details on the status of peyote studies see LA BARRE 1960: 45) The fact that peyote is not habit forming has led some investigators to regard it as not being a true narcotic, the question appears to be primarily one of definition. (BARBER 1959: 641)

[3] After 1614 accusations of witchcraft were seldom lodged in Spanish religious courts. This was due mainly to the efforts of an enlightened *visitador*, the Inquisitor Salazar Frias, who was responsible for investigating a witch scare in Navarre. The report which he compiled for the Suprema in 1612 became the basis of a future policy that made it impossible to lodge accusations of witchcraft without the strongest of evidence. (LEA 1907: 233)

attitude of the colonial clergy towards what was considered witchcraft and what was regarded as paganism. Although it is easy to oversimplify the evidence, it is probably fair to state that the pagan was regarded more as an individual to be pitied than one to be punished. Although wrong in his beliefs, the pagan could not be held responsible for his error and it was the duty of the missionary to convert him to Christianity by example and gentle persuasion. It is stressed many times in colonial documents that the civil and ecclesiastical authorities should not use force in bringing about this conversion. This attitude remained the official policy of the Church during the whole course of Spanish dominion in the Americas.

The witch, though, was to be dealt with in a completely different manner. For one thing, witchcraft and heresy were often equated and as such endangered both the Church and state. It was considered vitally necessary to root out such practices with the utmost severity at the command of the authorities.[4]

It should not be thought that out of the way places like New Mexico were considered less prone to witchcraft and the evils resulting from it, the very opposite in fact would be nearer the truth. Frontier areas where control — civil and religious — was less firmly established were regarded as being in special danger. In such regions, according to a newly appointed custodian of the missions (1661) "the poison is more powerful." (Scholes 1942: 98)

This important difference between witchcraft and paganism as a factor in the formation of Spanish attitudes towards the Southwestern Pueblos appears to have been insufficiently investigated. It is the thesis of this paper that many of the differences in outlook that can be recognized between missionaries in Mexico and those in the Southwest have their origins in this distinction.

[4] Not all heresy involved witchcraft, nor did all witchcraft involve heresy. Heresy should have in it an element of religious error. For instance, attributing powers to Satan (and through Satan, witches) was not heretical so long as the assumption existed that Satan could do only what God permitted. If, on the otherhand, the perverse individual assumed that Satan could exercise power in his own right, then heresy was involved (WILLIAMS, 1959: 89).

The accounts of the missionary efforts in Mexico, including the many *relaciones* compiled by both laymen and priests, the official reports of bishops and heads of monastic houses, and even the letters of viceroys to the Spanish Crown, give a completely different picture of the state of the missions, relations between missionaries and natives, and the ends to which the work of the missionaries were directed, than do similar documents dealing with the New Mexico establishments.

The Spanish in Mexico came into contact with a highly organized and flourishing religion. The hierarchical framework, impressive edifices, elaborate rituals, and so forth, must have produced a strong effect on the *conquistadores* and the religious that shortly followed after them. With certain very important reservations, the differences in the two religious systems did not preclude the drawing of camparisons between the religion of highland Mexico and the Catholic system in Spain. It should also be remembered that of all Christian countries, Spain, because of its 700 years contact with Islam in the Peninsula, had greater experience in interpreting non-christian religious systems.

It is therefore not surprising to find that just as Spanish chroniclers refered to the political and administrative organization of the Aztecs in secular terms, Sahagun and others refer to the religious officers in ecclesiastical terms. A further indication that the Spanish considered the bulk of Aztec religion heathenism rather that witchcraft may be seen in their treatment of Mexica temples. The great temples of Tenochtitlan, before their final destruction in the siege of 1521 and the aftermath of conquest, were more than once converted into centers of Christian worship. This treatment is comparable to what happened to Moorish mosques after the Spanish *reconquista,* but these, having survived the transition, and being more amenable to the necessities of Christian worship, have in some cases remained churches and cathedrals to this day.

The question of Spanish attitudes regarding the indigenous religions of Mexico has been gone into in some detail because of the fundamentally different attitudes manifested by the same people in the Southwest. In New Mexico it appears that attemps to suppress native ceremonies were accompanied by the

wholesale use of repressive measures and physical punishments. (Spicer 1954: 667; Reed 1944; 67; Scholes 1942: 16)

The evidence seems to indicate considerable toleration of native ceremonial in Mexico, and it is perhaps significant that Aztec-derived dances are still performed within the cathedral enclosure in Mexico City. On the otherhand, we read that the efforts of the clergy in New Mexico were directed to the suppression of kachina dances, masked rituals, and all forms of ceremonial expression that did not strictly conform to Catholic ritual.

The detection and burning of ceremonial paraphernalia must have taken up a good deal of the missionaries' time. In 1660 it was reported that within a short period more than 1600 masks, prayer sticks, and figures of various kinds were collected and destroyed. In the kiva of Isleta alone, twelve "diabolical masks" (Scholes 1942: 98) were found and promptly burned. The adjective "diabolical" and the mode of destruction are certainly indicative.

Apparently Spanish authorities had considerable evidence not only of what they regarded as witchcraft among the Pueblos, but, something perhaps considered a greater danger, practices of Indian origin that would be broadly described as "witchcraft" at the time seem to have been borrowed by the Spanish colonists from their Pueblo neighbours. Writing to the Holy Office in 1661, a Franciscan remarked on the evil effect that *catzina* ceremonies had on the hispanic colonists, especially those of "humble estate, such as *mestizos* and mulattoes, in whom the Faith is not firmly grounded." (Scholes 1942: 98) Commenting on this aspect of Indian-Spanish acculturation in the Southwest, a modern investigator writes that "the influence of the Indians was apparent in many ways, but none more strikingly than in the prevelence of superstition, in the practice of which they were apt teachers." (Scholes 1935A: 99)

In order to investigate such practices, and in general to elevate the moral tone of the colony and keep an eye on the administrators, the Holy Office was established in New Mexico in 1626. (Scholes 1935B: 205) As in all other Spanish

dominions in the Indies, this tribunal restricted itself to the investigation of Spaniards, although this category might at times be interpreted to include mestizos and mulattoes. But never did the Holy Office have jurisdiction over indigenous peoples. (Bandelier and Bandelier 1923: 26) Nevertheless, the documents relating to the Inquisition are important in that they give accounts not only of native life, but also shed much light on how Spanish colonists of the seventeenth century were influenced by the close proximity of the Pueblos.

One of the most energetic investigatiors for the Holy Office was Father Perea, agent for the Province in the years 1631 and 1632. He sent periodic reports to his superiors in Mexico City, and, even if some of them may strike us as unduly alarmist, he was probably correct when he wrote that in a "people [reared] from childhood subject to the custom of the Indians [and] without discipline and schools" moral disintegration and all forms of witchcraft were prevelent. (Scholes 1935B: 223)

Even if the majority of the practitioners of what was regarded as witchcraft, and those who patronized them, were to be found in the lower brackets of society, there are also indications that people of high station were not by definition free from suspicion. For example, one of the accusations made against ex-governor Sotelo, was that one day in 1627 (when he was then holder of the highest civil and military offices of the Province) he sent one of his servants to the Pueblo of San Juan to bring an Indian woman versed in magic and the black arts to Santa Fe in an attempt to save the life of a soldier who had been bewitched. (Scholes 1935B: 219)

Much of the evidence which resulted from the thorough investigations of Father Perea deals with the preparation and use of herbs, powders, and other concoctions, for the purposes of winning back a husband's love. A considerable number of the potions are clearly of European origin. This is probably true of all those that make use of food or are drunk or eaten, for it appears that this system of magic is not native to the Pueblos, although it is a recurrent theme in European folklore. (Parsons 1939: 1109) Conversely, techniques which include the manufacture from local plants of ointments and pastes to rub

on the body, can with some assurance be regarded as Indian in inspiration. The documents reveal a common knowledge on the part of the colonists of both types of formulae and a dependence on Indians, especially house servants, as a source of knowledge. (Scholes 1935B: 219)

An interesting sidelight which may be indicative of why the missionaries did not seem to differentiate at all clearly between the formal religion of the Pueblos and witchcraft is provided by examples of Spaniards taking part in *catzina* dances. Scholes sites the example of a high official, the *alcalde mayor* of the Picuris-Taos jurisdiction, who was accused of having danced the *catzina* to the scandal of both clergy and colonists. (Scholes 1949: 62) Such a performance would easily be interpreted as witchcraft, even perhaps by the very dancer in question. Apparently this was not an isolated case.

So far we have examined the problem of the interpretation of Pueblo religion and witchcraft from what we might call the Spanish viewpoint. There seems to be little doubt that the body of Indian witch beliefs are indigenous and only parallel those current in sixteenth and seventeenth century Europe. (Parsons 1939: 1067) But this similarity is certainly more than a superficial one and refers to a whole set of shared beliefs having to do with the methods, powers, and roles of those regarded as witches. These similarities in form and function would favour the borrowing of traits in both directions; Spanish elements finding their way into Pueblo witchcraft, and Pueblo traits penetrating the folk practices of the colonists. The dangers involved in this situation — given the setting in which they took place — were clearly recognized by the Spanish Church and civil authorities.

Turning from recognized witchcraft to religion, we find that because of certain elements in Pueblo religion, which also appeared to be witchcraft from the Spanish position, no clear distinction was made in many cases between witchcraft and religion, as for instance, had occured in Mexico where indentification was easier.

The failure of the Spanish to distinguish clearly these two seperate elements, what we may term the social and the anti-

social, led them to treat both alike. One important result of this situation is that it drove Pueblo religion, and with it Pueblo culture, into a more esoteric (one might almost say "underground") position and helped to implant the persecution complex which continues to be an outstanding characteristic of much of Pueblo life.

It will not to over argue the point or even pretend that this religious factor was necessarily the most important one in framing Pueblo acculturation patterns. The role of the civil authorities in Colonial times, the inherent attitudes of the Pueblos themselves to change and foreign intrusion, the somewhat mediocre personnel that seems often to have been sent to this outpost of the Spanish empire, all these and many more factors must be taken into full account in any appraisal.

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Eastern Kutchin Warfare*

BY RICHARD SLOBODIN

To early European explorers, the Kutchin bands of the Mackenzie River drainage were known as "The Quarrellers". This was an attempted translation of an almost untranslatable Athapaskan phrase,[1] meaning something like "Those Who Look Here and There." As interpreted to mean "Those Who Look for Enemies," it appeared to justify the "Quarreller" appellation.[2]

A cultural gradient from simpler forms in the east to more complex in the west has been noted for the Northern Athapaskans (Osgood 1936a:2). Among the Kutchin bands this regional variation may be observed in a narrower range. Judged by eastern Kutchin standards, the social organization of their western congeners was relatively complex, rigid, and authoritarian. This held true also for the organization of intercommunity conflict. In turn, western Kutchin organization was simpler and more loosely structured than that of tribes to the west, especially those of the Pacific coast to the southwest.

The eastern or Mackenzie basin Kutchin bands are those of the Arctic Red River and Peel River. The western bands are the Yukon Flats, Chandalar River, and "Downriver" Yukon Kutchin. The Crow River Kutchin occupy an intermediate position geographically and culturally.[3]

* This paper is based upon fieldwork among the Kutchin in 1938-39 and 1946-47, primarily with the Peel River band. The Arctic Red River, Crow River, and Yukon Flats Kutchin were also visited, and the author had some acquaintance among the Mackenzie Eskimo.

[1] As transliterated by Alexander Mackenzie and John Richardson, the phrase does not appear to be Kutchin. It may be of Hare origin.

[2] It is probably from this epithet that the French term for the eastern Kutchin, "Loucheux", derives. As commonly employed in reference to these Indians, its pronunciation is anglicized as /'lucu/.

[3] The identity and distribution of Kutchin communities at the time of contact has been reconstructed by Osgood (1934). Following current Americanist usage (Kroeber 1955: 303-305), the communities are here designated "bands".

The eastern Kutchin do not consider themselves quarrelsome, and this view is shared, in a different spirit, by western Kutchin. To the western Kutchin, the Mackenzie people were of little account in warfare, either organizationally or as individual fighters. Members of the eastern bands see themselves as quiet, equable people, not seeking trouble but ready to defend themselves if need be. This conforms with the eastern Kutchin manly ideal: one who is quiet, modest, goodhumored, even gay, but tough enough when occasion demands. Physically, members of the Peel River band are generalized as rather short, chunky, and dark-skinned — tall or light-skinned band members, of whom there are not a few, are explained as of Yukon derivation. This alleged physical type is considered to be congruent with the ideal personality type.

The eastern Kutchin attitude toward inter-community conflict, and also toward the western Kutchin, is illustrated in a favorite Peel River story placed in the period shortly prior to European contact. A turbulent Yukon Flats Kutchin war-leader, driven into exile by a dispute with his band chief, formed an outlaw group of Yukon and Crow River men which crossed the mountains to the Peel valley, bent on plunder. Encountering a number of Peel River families on the move, the outlaws represented themselves as without food and in need of help. The Peel River people made them welcome, but the arrogant behavior of the visitors put them on their guard, and they moved camp to a hill overlooking the outlaws' location. So disdainful of Peel River prowess was the Yukon war-leader that he did not intervene as the easterners took the high ground.

On the following day the war-leader, seeking an excuse to open hostilities, taunted and insulted a short, mild-looking Peel River man. Although a formidable wrestler, this man made no retort. At length the exasperated Yukon warrior ripped the small man's upper garment off and tore it to pieces. At this the offended man threw the war-leader down, whereupon the Peel River people attacked, and after a desperate fight killed all of the invaders save one aged shaman.

There is a discrepancy between the self-image reflected in this tale and the known history of mutual aggression between

eastern Kutchin and "Mackenzie" Eskimo, their neighbors to the north. This discrepancy is not entirely explained by the fact that the aggressors in the story cited above were fellow-Kutchin, for the eastern Kutchin have the same pacific image of themselves in relation to the Eskimo, although their tradition does not imply that hostilities were forced upon them by the northern enemy.

Traditions on Causes of War with the Eskimo

Eskimo and Kutchin informants agree that relations between the two peoples were at one time friendly. Kutchin tales refer to combined Eskimo-Kutchin hunting camps "long, long ago" in the Richardson Mountains west of the lower Mackenzie. Three different explanations are given in eastern Kutchin tradition for the rupture of peaceful relations; 1. a contest of hunting magic between a Kutchin and his Eskimo trading-partner; 2. a deception practised upon an Eskimo by a Kutchin; 3. the ravishing of a Kutchin girl by an Eskimo. The first two explanations imply close relations between the two peoples. The Kutchin deception cited as the second reason occurred when Eskimo and Indians were camped together. A Kutchin boy shot a bird with his small bow and arrow. An Eskimo boy took the bird, and the ensuing quarrel involved their families. To restore peace, the close kin of both boys exchanged gifts. One of the Kutchin was called upon to contribute a beaded coat, but he was too poor to provide the shell beads proper for its adornment. He had his wife use feather quills cut to resemble beads. When the Eskimo recipient discovered the substitution, he was insulted, and a feud developed.

It may be noted that only one of the three alternative Kutchin explanations for the outbreak of hostilities assigns blame to the Eskimo, for, in reference to the second story, it is not considered that blame for so grave a consequence can be placed upon a child.

Mourning and Vengeance

Although the capture of women and goods, and the enhancement of a man's prestige as a warrior were incentives to the war-path, among the eastern Kutchin they were not "causes of war", as they have been termed by Osgood (1936b: 86). From the Kutchin point of view the immediate occasion for a raid was revenge.

Once the pattern of raid and retaliation had been established, there was always a death by raiding to mourn and to revenge. When a Kutchin died from any cause, his near kin mourned for him and dreamed about him constantly. Such dreams were ominous and disturbing. The dreaming and mourning could be brought to a close, in the case of death not at the hands of the enemy, by (1) a suitable memorial potlatch or (2) the rebirth of an aspect of the deceased's soul in a baby of the opposite sex. Such reincarnation occurred only occasionally. The deceased was recognized in some attribute or mannerism of a child of the appropriate sex born soon after the death. Upon such recognition, a small potlatch for the child was provided by the mourners.

Where a Kutchin was slain by the enemy, the resulting social imbalance was only partially restored by memorial potlatch or reincarnation. Mourning and dreaming could be terminated only by a death outside of the community, usually — although not always — a vengence upon the killers.

Informants differ as to whether the killing of women and children was avenged. From the important roles played by women in some war stories, it would appear likely that their deaths were avenged in some cases at least. This seems also to be true of children with high-ranking close kin. During the period of mourning, all close consanguineal kin of the victim — parents, children, siblings, parents' siblings — wore ragged clothes and tended to remain apart from other people. The chief mourner, i.e., the most influential man among the close kin, further signalized his condition by wearing suspended from his neck the head of a raven with the the windpipe pro-

truding. He camped at a little distance from the rest of the band.

Here, as in all aspects of Kutchin life, past and present, consideration was given to the prestige of the deceased and of his immediate family, and more especially to their place in the scale of wealth-ranking. The "wealthy" had many relatives, that is, many people who were willing and anxious to assert kinship. The "poor" had few.

In Kutchin society there are three matrilineal sibs; the tribe is at the northeastern margin of the distribution area for the Tsimshian-type sib system. However, there is no indication that sib membership played a significant part in the organization of vengeance among the eastern bands.

Vendetta raiding was a warm-weather activity. During the winter Kutchin and Eskimo were usually dispersed in small groups, the Kutchin high on either side of the Continental Divide, the Eskimo along the Arctic coast or on nearby islands. With the advent of warmer weather, the Eskimo came up into the Mackenzie delta and the eastern Kutchin moved in the direction of the Mackenzie. Moreover, at this time both peoples congregated in fish camps.

Kutchin informants agree that the best time for raiding was in the spring just before breakup. The Indians believed that they could travel more swiftly than the Eskimo at that season.

At some time after the spring ingathering, the chief of the band or the leading man of the fish camp (Slobodin 1959: 104, 124) would request the principal mourner to appoint three leaders for a retaliatory raid. The head war-leader recruited the war party and arranged for war medicine to be made by shamans. The bereaved family, through the principal mourner, gave the war-leader a wolverine pelt, or two pelts if a large party was to be assembled. The fur was cut into narrow strips one of which was given to each man designated for the raid. This was worn as a circlet around the head, bestowing upon the wearer, it was hoped, some of the cunning and ruthlessness of the wolverine.

One of the oldest Peel River informants, generally reliable, described a variant method of recruitment. The chief mourner himself distributed the wolverine strips. Four strips shorter than the rest were given to the four, rather than three, men were to lead the raid. The leaders were also given skins of the ermine weasel to wear as additional circlets, so that for them the craftiness of the weasel was added to that of the wolverine.

The size of the war party is not clear; "about a hundred" was given as an estimate, but this figure is almost surely an exaggeration. Two or three dozen seems more plausible.

On the day before the departure of the raiders, the entire encampment participated in a dance in which combat was mimed. Medicine was made to give the warriors a measure of invisibility to the enemy, to steal or render ineffective the enemy's arrows, and to reduce his watchfulness. Each warrior, or his close kin, might make medicine for himself, but, in keeping with the relative lack of specialization among eastern Kutchin, shamans did not accompany the party unless chosen for their fighting ability. War-leaders were not necessarily shamans.

The Raid

During most of the raiding season there was continuous or almost continuous daylight. The war party travelled during the cool hours when the sun was low. As the shadows lengthened on the appointed day, the principal war leader gave a loud whooping signal, and, if there was snow on the ground, each man threw his trail snowshoes down and leaped upon them, setting off immediately at a jogging run. Kutchin hunters were trained in getting onto their snowshoes and setting off in a hurry; even at present a skilled hunter can do this, wriggling his snowbooted foot into the snowshoe harness as he runs. Each man carried some pemmican or dried meat, a bow, a quiverful of arrows to the number ordered by the war leaders, and a club. Bows were generally of birch, strung with twisted caribou sinew. Arrows might be equipped with points

either firmly attached or detachable. The Kutchin did not have specialized war arrows, as did some American Indians. The club was the characteristic weapon of the Kutchin. The most favored club was a section of caribou antler between two and a half and three feet in length, from which the secondary tines had been cut, and which had been boiled and straightened. Many men preferred to keep a portion of caribou skull attached to the antler, giving the club a potato-masher design.

Lances were rarely used, while slat armor, mentioned by Osgood (1936b: 87) is cited only for the Western Kutchin. For infighting and the *coup de grace,* stone picks or daggers triangular in cross section were favored.

Some of the war party, but not necessarily all, would wear a single feather stuck upright in the fur headband. These were men who possessed, or who had been given for the time, the medicine of a bird: loon, any of several species of duck, duck-hawk, eagle, whistling swan. Painting of the face or jacket with red earth pigment, also varied individually.

If the ground were still snow-covered, the fighters had large smudges or vertical bands of soot under each eye eo reduce the glare of reflected sunlight, for this was the season for snow-blindness.

The march was rapid, usually single file along main river trails until well into what might be termed the combat zone.[4] No fires were lit. A good deal of security was maintained. Watches were set while the party slept, and evidences of encampment were concealed. Thus, while resting, men frequently passed the time in shaving down arrow shafts; the shavings were then concealed under rocks. Discipline was maintained. Some war leaders punished a breach of security by sending the culprit home in disgrace, or even, some say, by shooting him.

The pattern of the raid was the surprise attack upon a sleeping encampment.

[4] Jones (1864: 327) and Osgood (1936b: 88) state that the war party killed every living thing which it encountered on the outward march. None of my informants mentioned this practice.

In a Peel River attack upon Eskimo, three to five night marches would bring a party from one of the summer camps on the Peel to an enemy camp in the Mackenzie delta or on one of the Eskimo Lakes east of the delta. Shortly after midnight before the attack, the leaders would scout the enemy and mark a point overlooking his camp; some informants state that a short stick was placed in the snow as a mark. Each of the party would then advance to the mark and take a look. In some raids the leaders assigned particular tents to certain groups of men. In turn, each of the leaders would then advance again to the marked spot. It any of them went past the spot, this was the signal for a general attack. Leaders preferred to wait until the hoot of an owl was heard, followed by the call of a ptarmigan. Some men, it was said, could talk to these birds and induce them to give the signal.

The attack consisted of rushing to the dwellings, cutting them open or trampling them down if they were skin tents, and laying about with clubs. Bow and arrow were employed as a rule, not for preliminary fire but to pick off fugitives. As they attacked, the warriors would shout "Go upriver (or south)", which, as Osgood says, has the same force as "Go to Hell!" (1936b: 87). As a rule only one of the enemy was spared intentionally, unless captive women were desired, but an impression is gained from hearing a number of raid stories that in fact few if any Kutchin or Eskimo raids succeeded in annihilating the enemy encampment. The actual attack was not highly organized in a military sense. The surrounding of a camp and closing in from all sides is described only for the western Kutchin. This is striking in view of Kutchin and Eskimo experience in surrounding caribou. Moreover, while there is mention of assigning certain attackers to certain tents, discipline in this regard was not well maintained in the heat of the attack, and frequently the raiders would concentrate on the nearest dwellings, permitting occupants of others to escape or even to counterattack.

The one male enemy who was deliberately spared was usually a man who had witnessed much of the affray. He was

called "The Survivor."[5] He was spared so that he could spread news among his people of what had happened and the reason for it. One of the leaders would point to the chief mourner and say, "This is because you killed his brother." Sometimes a further announcement would be made that there would be another raid "when the sun is higher." This was done "so that the enemy wouldn't sleep well then."

Understandably, efforts were made at even greater speed and security on the return than on the outward march. However, as the party was burdened with loot and sometimes with captives, essential speed was not always possible, and there are tales of a returning party being overtaken by the enemy.

There is no indication that raided enemy or captives were tortured, and several informants deny it. At the time of the attack, bodies of victims were disjointed (cf. Jones 1864: 327; Hooper 1853: 366). This was sympathetic magic to reduce the speed and agility of the enemy. There was some ceremonial cannibalism; e.g., drinking of spurting blood, and eating part of the liver. The current practice of drinking blood from the artery of a freshly-killed or dying caribou is reminiscent of cannibalistic blood-drinking as described in the tradition of war. Both customs were followed by the Eskimo as well.

Defense Measures

If the summer was preeminently the time for Kutchin raiding, it was the time to fear raids as well. As an informant put it, "When the people were fighting all the time with the Eskimo, they were afraid to sleep soundly. The men used to hold a sharpened bone or stick in their hand which would jab their head if they nodded. When the white men came, and the people saw their soft, comfortable eiderdowns, they said, 'It would be easy to kill people who sleep so comfortably, if we had a war with them'..."

[5] The old shaman in the story of the western Kutchin marauders may have been spared in conformity with this custom. He was not, however, referred to as "The Survivor" in the institutionalized sense.

Another informant averred, however, that two characteristics of the Eskimo gave some forewarning of their approach: if the wind was right, they could be smelled ten miles away; and, since they were afraid of the bush, they made a good deal of noise going through the forest.

Fish camps were fairly large concentrations along the banks of principal rivers at or near good fishing locations (Slobodin 1959: 102, 111-112). Youths and young unmarried men, who were segregated in a special age-group, were frequently assigned to stay a few miles downriver from the main camp, serving as a sort of picket or guard to raise an alarm if Eskimo were observed.

With these alleged precautions, it might be supposed that surprise was minimized, but in fact both Kutchin and Eskimo did surprise the enemy not infrequently. A location on the Peel, in Yukon Territory some 80 miles above the mouth of the river, is known in Kutchin as "Eskimo Drawing Their Bows," the scene of a successful Eskimo raid, while Arctic Red River Kutchin claim to have raided scuccessfully on Aarctic Coast east of Toktoyaktuk. Both sides seem to have conformed to Turney-High's generalization that in warfare American aborigines relied heavily upon surprise but very little upon security (1949: 117).

Conflict with Indian Groups

It has been remarked that the Eskimo were not the only enemy. However, in eastern Kutchin tradition, it was only in relation to Eskimo that a self-perpetuating pattern of vengence raiding developed. Other encounters were said to have been very occasional, with the eastern Kutchin playing a purely defensive role as in the story of the outlaw raid cited earlier.

Traditional enemies of western Kutchin were the "Dwellers Among Beavers," probably not the historic Beaver Indians but the Tutchone of the Stewart River, who were said to have raided in order to obtain captives. They were great medicine men, able to steal Kutchin ammunition from a distance by means of sorcery. This could sometimes be prevented by covering the

arrows with a clean tanned caribou skin, endued with power by a shaman.

In the 1880's a large party of these people came upon an encampment of Peel and Arctic Red River Kutchin on the upper reaches of the Snake, a tributary of the Peel. The "Beaver" people numbered more arms-bearing men than did the Kutchin encampment, but the Kutchin were alerted and ready for them, so the invaders camped opposite them and began a dance. This might be preliminary to trading and games, or to a fight; even now, dancing in the presence of strangers may carry an agressive connotation. The Kutchin sent runners off to find two hunting parties of their young men, who arrived on the following day, whereat the encounter turned into a tradind-party.

Just at the turn of the century, the threat of a raid by the same people broke up a large camp of Kutchin, also on the Snake River. "When we heard they were coming," stated a Crow River man who was present, "we threw some rafts together and beat it downriver in a hurry."

Captives, Loot, and Honors

As remarked earlier, among the products of inter-community strife were captives, loot, and war honors. There is no Kutchin tradition, as there is among many peoples, of raiding specifically for the purpose of acquiring these valued objects. Undoubtedly, capture in war was one of the means by which material goods were diffused. The Kutchin, however, were involved in intertribal commerce prior to the advent of the Euro-american fur trade. Indeed the Kutchin maintained trading-partner relationships with Eskimo as with members of other Indian communities throughout the period of Eskimo-Kutchin conflict. The rapidity with which commodities of value were transmitted from one community to another across great distances in the Arctic has been described by Whymper (1869: 162) and Stefansson (1914) and has been noted by Sapir (1916: 35-36). There is no reason to suppose that warfare played a major part in such diffusion.

As with other peoples of simple economies, captives of the Kutchin and their neighbors were usually adopted into the band and became full members.

There are tales, however, of women, either Kutchin or Eskimo, regarded as particularly clever and tricky, who instead of becoming acculturated to the captors, deceived and betrayed them to their own people when occasion arose.

Among the eastern Kutchin, a horizontal line of dots or dashes was tattooed in blue-black pigment on the upper arm of a man for each enemy killed. The possession of such marks was honorific, and they would be displayed at suitable occasions. However, there do not appear to have been any war leaders or warriors of outstanding repute in the eastern bands. Whenever reference is made to great war leaders, eastern Kutchin refer to men of the Yukon area. The references are made with evident mixed feelings: regret that the band cannot boast of such great men, mingled with pride that their forebears were not subject to such bullies; for the war leaders are depicted as arrogant and insufferable toward their own people.

The Neutral Ground

Demographically, the principal effect of conflict between eastern Kutchin and Eskimo was that a considerable stretch of the lower Peel and the upper Mackenzie delta was rendered uninhabitable, certainly during the early and middle nineteenth century, and probably for some time before that. The danger of sojourning in this no man's land or neutral ground is illustrated in two stories. In one, an Eskimo with a painfully infected eye deliberately moved into the Middle Peel Channel, off the lower Peel, courting death at the hands of the enemy. The alleged site of his camp is still known as Rotten Eye Portage. In the other story, an aged Kutchin couple had lost their only son in an Eskimo raid. They were inconsolable, but too poor to organize a retaliatory raid. It was not uncommon for the bereaved family in such a case to move away from their people voluntarily and camp alone, becoming in the course

of time "bush people:" allegedly desocialized and partially dehumanized personages with a touch of the supernatural (Slobodin, 1960: 127). In this case the elderly parents moved right down into mid-delta. Through craft and hard work they succeeded in killing a large party of Eskimo, whereupon they left the neutral ground and rejoined their own people.

The hazards of life on the lower Peel reinforced the upriver orientation of the Peel River Kutchin. One effect of this was that beaver-hunting played a small part in the economy of this people, although the lower Peel was excellent beaver country. The members of this band were hunters and trappers of the high valleys and mountains. Neither before nor after the advent of European traders were family hunting territories established by Peel River people, as occurred among the Algonkian-speakng beaver hunters (cf. *inter alia,* Cooper 1939; Speck and Eiseley 1942; Leacock 1954). The writer has less information on Arctic Red River land tenure. Some families of this band apparently summered on the Mackenzie at the mouth of the Arctic Red River and did hunt beaver. There is a suggestion in the available data that family hunting territories for beaver were held, although not in perpetuity. Whether these were pre- or post- contact is not known.

Kutchin as Middlemen

During the early years of the nineteenth century, Mackenzie Eskimo and eastern Kutchin became aware of the Hudson's Bay Company trading post at Fort Good Hope; indeed, the explorer Franklin states that this post was moved down the Mackenzie to the Lower Ramparts in 1823 "for the convenience of... the Loucheux [Kutchin]" (1828: 23). The eastern Kutchin, especially the Arctic Red River people, thus found themselves in circumstances which have been experienced by a number of non-urban peoples in relation to the expanding economy of an urban civilization. They were in the highly profitable position of middlemen between the civilized traders and more distant aborigines, and were able to maintain this advantage for a time due to the possession of superior weapons obtained in trade.

The Kutchin evidently appreciated the advantages of their situation. Before the arrival of the Hudson's Bay Company at the Lower Ramparts, that canyon had been the scene of Eskimo raids. However, after the establishment of Fort Good Hope at the Ramparts, Eskimo were unable to get there due to intensified resistance by the Kutchin in their efforts to prevent the Eskimo from trading directly with the Europeans. This resistance has been recorded by Richardson (in Franklin 1828: 204 and Richardson 1851: 204, 223), Simpson (1843: 104), and Petitot (1889. 91).

With the design of bypassing the eastern Kutchin and reaching the Eskimo as well as other Kutchin bands, the traders established themselves on the Peel in 1840. The Peel River band chief earnestly requested that the new post be located at the principal summer rendez-vous of his people, seventy-five miles up the Peel from its confluence with the Mackenzie. This, however, did not suit the traders' purposes, and the post was built on the lower Peel. As it was on the dangerous neutral ground, for several years it was visited by few Kutchin or Eskimo. With the development of trade at this center, clashes between Eskimo and Peel River people were frequent. It cannot be stated with certainty that they were more frequent than previously, as there is no basis of comparison. However, between 1840 and 1856 there were seven major fights between the two peoples on the lower Peel or near the mouth of that river, as reported by European observers (Slobodin 1959: 41-42). All of these were departures from the usual vengeance-raid in that they were stand-up battles or melees, several occurring in the middle of the day.

Cessation of Hostilities

The Eskimo raid in 1856, in which four Peel River Kutchin were killed, is the last recorded instance of inter-group violence. However, throughout the remainder of the nineteenth century, relations between the two peoples remained tense. Fort McPherson, the trading post on the Peel, was a fortress in fact, with arms at the ready when parties of both peoples were trading at the same time. In the summer of 1904 armed conflict

between Kutchin and Eskimo was barely averted. The death of many Eskimo later that year in a measles epidemic is attributed by the Indians to the sorcery of an Arctic Red River shaman. As late as 1938-39, many elderly and middle-generation Eskimo and Kutchin remained hostile and mistrustful of the other group. Younger people, however, had already mingled at residential schools and elsewhere; with the passage of another decade the ethnic hostility was a fast-fading tradition.

Increasingly during the later nineteenth century, and more rapidly after the great rise of muskrat prices during World War I, the eastern Kutchin tended to occupy the downriver areas of their river basins, near the Mackenzie. In modern times they are predominantly lowland peoples, although there remains a strong emotional attachment to the mountainous section of their home territories.

Why did the two peoples stop fighting in 1856 when pacification was not imposed by *force majeure?* Missionaries, both Roman Catholic and Anglican, who had reached the region a few years earlier, claimed credit for the peacemaking, and undoubtedly their influence, as well as that of the fur traders, carried a great deal of weight. It is likely, however, that the natives, by this time adapted to the Euro-Canadian fur trade, saw trading and trapping as more profitable than fighting. For generations thereafter, there was a delicate balance between peace, or rather armed neutrality, and war, but it is significant that at the time of the threatened clash in 1904 peace was maintained through the persuasion of trading-partners on both sides.

Hypothesis on the Development of Raiding

It is quite possible, although there is no direct historical evidence on the matter, that there is truth in the Kutchin tradition of friendly relations with the Eskimo prior to the period of hostilities.

It is almost certain that the efforts of the eastern Kutchin to prevent the meeting of Eskimo and European fur traders were motivated by a desire to maintain a profitable position as

middlemen in trade. As has been pointed out, similar situations have been important throughout world history, and have undoubtedly played a major part in the history of American Indians. In the Canadian North, for example, Fort Churchill was established in 1717 as a deliberate move to circumvent Cree opposition to direct trade by the Hudson's Bay Company with Athapaskan peoples. The Chipewyans, earliest-contacted of the Athapaskans east of the Mackenzie, followed a course similar to that of the Cree, driving westward to the Athabaska and Peace Rivers and establishing a highly profitable position as middlemen between fur trade posts on Hudson Bay and the Beaver and Slave Indians.

The Kutchin middleman position appears to have antedated the advent of fur trade posts in their country. The stimulus given to trade on the north Pacific coast of America during the eighteenth century by the visits of European vessels had repercussions in intertribal trade.[6] Olson (1936 has described such trading between Tlingit and Athapaskans of their hinterland, and references are made to it by Goldman (1940) and McIlwraith (1948). The Yukon Flats Kutchin have traditions of large-scale annual trading expeditions, well-organized under high-ranking leaders, which crossed the "Icy Mountains" to peoples at or near the Pacific coast. The principal Kutchin exports were furs and skins; the commodities brought back included dentalium shells, copper work, and, later, hardware of European provenance.

Although Russian traders on the Yukon did not reach Kutchin territory, their trade-goods moved eastward in intertribal trade. "As the goods which the Mountain Indians [Crow River Kutchin] exchange with the Esquimaux at Herschel Island are very unlike those issued by the Hudson's Bay Company post, I conclude that they obtain them from the Russians," Richardson remarked in 1823 (Franklin 1828: 180). Indeed, nearly twenty years after the establishment of the Hudson's Bay Company post on the Peel River, one of that company's Chief Factors

[6] McCLELLAN (1950) discusses the effects of early European trade upon tribes inland from the Northwest Coast.

reported gloomily that the Russians controlled trade with the Eskimo "between Cape Barrow and the Coppermine" (Anderson 1858).

That the Kutchin understood the value of their commercial position in relation to the Eskimo is indicated by the violent reaction of the "Mountain Indians" against the distribution by Franklin's expedition of goods among the Mackenzie Eskimo, which they saw as a threat to their trading monopoly. This very nearly resulted, or so the explorers believed, in the ambushing of their party by the Kutchin (Franklin 1828. 177ff).

It is suggested that the commerce between eastern Kutchin and Eskimo was gradually intensified during the eighteenth century by the stimulus provided, at a distance, by European trade with tribes to the west of the Kutchin. This resulted in increased contact as well as increased friction between these peoples. "Trade", as Herskovits points out, "is often consummated with an underlying sense of hostility" (1952: 180). Yukon trading expeditions frequently turned into war-parties. The contact and the friction between Kutchin and Eskimo was further heightened by the approach of the European fur trade, until, with its establishment in the lower Mackenzie valley under direct European control the Kutchin middleman position eventually broke down.

The foregoing is admittedly speculative, but it conforms with the meagre historical evidence which is available. If valid, it provides a soci-economic explanation for the regularized vendetta raids between eastern Kutchin and Mackenzie Eskimo, a pattern of behavior not otherwise characteristic of either people.

Summary

At the time of European contact and for several decades thereafter, eastern Kutchin bands and the neighboring Eskimo engaged in mutual raiding. The raids were occasioned by need to restore personal and social balance through vengeance. The desire for loot, captives, and war honors appears to have played a secondary role. It is suggested that raiding was stimulated and perhaps initiated by intertribal trading in which goods from

more complex cultures, including the European, played an important part, and in which the Kutchin were middlemen to the Eskimo.

The eastern Kutchin did not occupy this position in trade with any people other than the Eskimo, nor do they have a tradition of aggressive action toward any other people. Their neighbors to the west were in turn middlemen to the eastern Kutchin and aggressors against them.

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One Factor Magic: A Discussion of Murdock's Theory of Social Evolution

JAN DE LINT AND RONALD COHEN

In the last decade Murdock's *Social Structure* had a varying impact on anthropological studies. Some of its terminology has become part and parcel of the technical anthropological vocabulary, while other aspects of his work such as the use of the Human Relations Area Files, or the feasibility of subjecting such data to statistical procedures have not been generally accepted[1].

In the present paper it is not our aim to enter these areas of discussion. Our intention is rather to question those sections of Murdock's work which are pertinent to the theory of human social organization. These sections contain Murdock's theory of social evolution and its application to historical reconstruction. This theory shows how parts of the social structure are functionally interrelated and how these relationships can be used to demonstrate a certain sequence of change among the component parts of the structure.

Goode (1959:182) characterizes the theoretical approach which Murdock uses as one in which the researcher sets up a hypothetically closed system and attempts to find interrelationships among its important variables. Underlying this approach is the assumption that no one variable is *primary,* but rather that any change in one of them brings about a predictable degree of change in the others. With this brief characterization of Murdock's theoretical approach in mind, let us now turn to his conception of social evolution.

Murdock first notes that "...the various aspects of social organization admit of only a very few, relatively obvious, alternative variations" (p. 200).

[1] See, KÖBBEN, A.J. (1952).

The reason for this has been explained earlier:

> Where there are no practical limits to the variety of responses which people can make in particular situations, cultural forms can vary endlessly... The situation is quite different where there are practical limitations to the variety of responses which people can make. Under such conditions cultural similarities will appear in many different places, irrespective of historical contacts... (p. 115).

And also:

> While kinship terms themselves show unlimited variability, the methods of classifying them do not. With regard to each of the nine criteria of classification, for example, there are only two alternatives; a people can choose only to recognize or to ignore generation, sex, affinity, etc., in assigning a kinship term to a particular relative. There are also limited numbers of possible or practicable marriage forms... preferential marriages... rules of descent... family forms..., and so on (pp. 115, 116).

Given the fact that in this sense limited variation in social organization exists, Murdock is then faced with the problem of explaining these similarities. His solution lies in the realm of what may be called the 'closed system' approach since it excludes limited variation among external factors from consideration in his analysis. Offering his reason for choosing this method he claims that he

> ...is unable to conceive of any single external factor capable of producing similar effects in remote and diverse geographical areas among peoples of contrasting levels of culture while at the same time allowing for wide differentiations among tribes with demonstrably close historical connections (p. 200).

However by limiting the sources for change in social organization to factors within this system itself and by expecting these factors to account for similar forms of social organization in remote areas and for dissimilar forms of social organization in closely related areas, Murdock is well on the road towards what we have called one-factor magic. It is interesting to watch this intellectual perambulation take place, and luckily Murdock, in his own words, has allowed us an intimate glimpse into just how the attraction of single factor causation pulls anthropologists into its snare. He says that

> We must look for some aspect of social organization which acts as a filter, which is capable of responding in only a limited number of ways but by each of them to a number of quite diverse external stimuli. Such a structural feature must, in addition, be peculiarly sensitive to outside influences and at the same time be itself especially competent to effect compensatory readjustments elsewhere in the system... The *one* (italics ours) aspect of social organization that is peculiarly vulnerable to external influences is the rule of residence (pp. 200-201).

Once the magical factor has been singled out, the evolution of kinship organization can be visualized as a consistent result of changes in residence patterns. Thus Murdock can now claim that

> It is in respect to residence that changes in economy, technology, property, government, or religion, *first* (italics ours) alter the structural relationships of related individuals to one another, giving an impetus to subsequent modifications in forms of the family, in consanguinal kin groups, and in kinship terminology (p. 202).

The skeptic may well wonder at this point whether some other less simple explanation could be advanced to describe the same evolution. Indeed Murdock has anticipated criticism and argues that the burden of proof must be taken by his critics since his evidence (including his deductive reasoning) lead him to conclude that social evolution must be explained by reference to components within kinship organization itself and primarily the residence rule among these components. His justification for taking this position is his "specific disproof in Chapter 8 that historical connections significantly affect the forms of social organization" (p. X). This disproof consists of the following amply documented argument:

> The scattering and almost random distribution of the traits of social organization, which is equally characteristic of remote or unrelated and of contiguous or related peoples, renders practically *useless* (italics ours) all historical interpretations based upon expectations of diffusion (p. 196).

Yet Murdock himself describes how such phenomena as Christianity, Islam, or the introduction of cattle, are all conditions which may lead to certain changes in residence rule, and subsequently, in social organization. Thus although various

aspects of social organization have probably not diffused as such, conditions intimately related to this aspects certainly have.

The puzzling almost random spread of kinship systems around the world does not necessarily disprove diffusionist explanations. It may also be due to the fact that kinship has been isolated from its context, that in other words its relation ship to the total social organization is not taken into account.

So far it has been emphasized that there are two closely related techniques in Murdock's work which predetermined the form and content of his theory of social evolution. Firstly, his attempt to use the "closed system" approach necessitates that any possible determinant of change which is outside the boundaries of his isolate (i.e. kinship organization) is kept separate from his discussion of changes taking place within the isolated area of study. Secondly, the complexity of social evolution is simplified by making all external factors operate through only one internal component of the isolate, namely the residence rule, which in turn sets off a series of changes within the kinship organization.

In order to test whether there is any validity in the temporal pre-eminence given in theory to the residence rule, it must be shown that no changes in kinship organization can take place without the occurrance of a *prior* change in the residence pattern. Luckily Murdock provides us with ample material to substantiate this point. He claims (p. 203) that factors promoting monogamy such as Christianity, or widespread poverty, or a change in the sexual division of labour, *also* favour neolocal residence. In other words a change in marriage rule certainly *does not follow* a change in residence rule! On the same page he tells us that conditions favourable to the development of the nuclear family and to the disintegration of clans and other large aggregates of kinsmen, are *also* responsible for a change in favour of neolocal residence. Again a change within the kinship organization admittedly *does not follow* changes in the residence rule.

Neolocal residence is held to be favoured by a modification of inheritance rules such as the replacement of primogeniture by the division of an estate (p. 204). Again this illustration

contradicts the primacy Murdock attributes to the residence rule in the process of internal adjustment to changed conditions.

Turning now to Murdock's discussion of factors favoring bilocal residence we find similar contradictions. Where the theory should call for a listing of "external" factors which cause changes in residence rule and subsequent changes in kinship organization, we notice instead statements such as the following:

> Differentiation in the status of children according to order of birth and primogeniture without regard to sex seems especially conducive to bilocal residence (p. 204).

And again,

> Anything which lessens the strength of unilinear bonds favours bilocal residence, provided that kinship ties in general are not weakened (p. 204).

Similar inconsistencies turn up in the author's discussion of factors promoting matrilocal and patrilocal residence rules. Thus for example:

> A relatively high status of women, which favours bilocal residence, is also conducive to marilocal residence... But whereas it is a women's comparative equality with man in property and other rights that promotes bilocal residence, it is her superiority to him, especially in production and in ownership of the principal instrument thereof — land — that favours matrilocal residence (p. 205).

And also,

> It (polygyny) is, however, particularly congenial to patrilocal residence, where women are isolated from their kinsmen and tend to be economically and socially inferior to men. Hence anything which favours polygyny likewise favours the development of patrilocal residence, e.g. Mohammedanism may have such an effect (p. 206).

From these quotations it appears that changes in residence rules do *not* necessarily precede other changes in social organization. Certain "external" factors may logically and empirically result in shifts and variations in descent rules, in the economic system, or even in marriage preferences. In other words these aspects of social organization, as shown by Murdock himself, seem to have as much "filtering" power in initiating chain reactions as the magical factor of residence rule. Despite this difficulty the

author goes on to discuss the effects brought about by changes in the residence rule. Everything is now turned upside down, for what had previously been discussed as causes which result in residence rule change, are now treated as effects of this change. The following excerpts illustrate this paradox:

> The shift to neolocal residence *results* (italics ours) in the emergence of the isolated nuclear family (p. 208).

Compare the above quotation with the following:

> The development of neolocal residence, in societies following other rules, appears to be favoured by any influence which tends to isolate or to emphasize the individual or the nuclear family (p. 203).

In other words, whatever favours the nuclear family, favors neolocal residence, favours the nuclear family, etc., etc! Circular reasoning is also evident in the following:

> A change to neolocal residence from any form of unilocal residence has a disruptive *effect* (italics ours) upon existing unilinear groupings (p. 208).

Compare this with:

> Since... any influence which tends to undermine or inhibit large local aggregation of kinsmen will create conditions favourable to neolocal residence (p. 203).

The reader is referred to Chapter 8 of *Social Structure* for more of these circular statements. The above quotations however clearly illustrate how Murdock substantiates his theory of social evolution. Much of what is attributed to the cause of a certain residence rule is also shown to be the result of that same rule. All this is quite natural when we consider that *Social Structure* is a correlational study which attempts to obtain sets of concomitant co-variants among the elements of kinship organization. However it is one thing to establish a relationship between variables such that a change in one brings about a predictable change in the other, and quite another thing to establish priority or causal connection.

It should be noted in passing that Murdock (1959) has recently retreated somewhat from a total reliance on one-factor causation. In an article on the evolution of social organization he says that he now believes that the "choice between the two

bilateral systems, Types 1 and 2, may be initially determined not by factors immediately affecting residence in marriage, but by shifts in property systems" (1959:140). He still however feels that residence rules are the primary determinants of unilineal kinship systems and are secondary or derivative in the evolution of bilateral organizations. This retreat is therefore far from being a route. Murdock still adheres to a "closed system" approach and continues to connect many aspects of kinship organization to non-kinship factors through the causal aegis of the residence rule.

Although Murdock's approach suggests interesting and empirically verifiable interrelations among the elements of kinship organization, it has not, in our opinion given us much insight into the actual mechanism of social evolution. Consider in brief his reconstruction of the evolution of kinship organization. Eleven major types are distinguished on the basis of certain criteria. Each is again subdivided on the basis of residence rules, giving a total of forty-seven types of kinship systems. It is then argued on the basis of correlations previously established, that certain types can only give rise to a limited number of other types. For example bi-Eskimo can only change into normal Hawaiian, matri-Eskimo to matri-Yuman, matri-Fox or normal-Nankanse. However if these rules are applied literally for all types, it does not take too many changes before all the forty-seven types have changed into only seven[2]! It must be assumed

[2] The following table indicates what happens if Murdock's rules of change from chapter 8 of *Social Structure* are applied to his forty-seven types in a regular progression.

Table of Murdock's Kinship Types
and the Changes Which They Go Through in Time

Type	I	II	III	IV	V	VI	VII
Normal Eskimo	1	?					
Bi-Eskimo	1B	2	?				
Matri-Eskimo	1M	3M,9,4M.	10,11.	11	?		
Patri-Eskimo	1P	3,4,5.	4P,6,7,8.	6,7,8.	7,8.	8	?
Normal Hawaiian	2	?					
Neo-Hawaiian	2N	1	?				

Matri-Hawaiian	2M	3M,4M,9.	10,11.	11	?		
Patri-Hawaiian	2P	3,4,5.	4P,6,7,8.	6,7,8.	7,8.	8	?
Normal Yuman	3	6	7,8.	8	?		
Bi-Yuman	3B	2	?				
Matri-Yuman	3M	10	11	?			
Neo-Yuman	3N	1	?				
Normal Fox	4	4P,7,8.	6,7,8.	7,8.	8	?	
Bi-Fox	4B	2	?				
Matri-Fox	4M	11	?				
Neo-Fox	4N	1	?				
Patri-Fox	4P	6,7,8.	7,8.	8	?		
Normal Guinea	5	6,7,8.	7,8.	8	?		
Bi-Guinea	5B	2	?				
Neo-Guinea	5N	1	?				
Normal Dakota	6	7,8.	8	?			
Bi-Dakota	6B	3B	2	?			
Neo-Dakota	6N	3N	1	?			
Normal Sudanese	7	8	?				
Bi-Sudanese	7B	4B	2	?			
Neo-Sudanese	7N	4N	1	?			
Normal Omaha	8	?					
Bi-Omaha	8B	4B	2	?			
Neo-Omaha	8N	4N	1	?			
Normal Nankanse	9	?					
Avuncu-Nankanse	9A	?					
Bi-Nankanse	9B	2	?				
Duo Nankanse	9D	5	6,7,8.	7,8.	8	8	?
Neo-Nankanse	9N	1	?				
Patri-Nankanse	9P	1P,2P,9D.	3,4,5.	6,4P,7,8.	6,7,8.	7,8.	8
Normal Iroquois	10	11	?				
Avuncu-Iroquois	10A	11A	?				
Bi-Iroquois	10B	3B	2				
Duo-Iroquois	10D	6	7,8.	8	?		
Neo-Iroquois	10N	3N	1	?			
Patri-Iroquois	10P	10D,3.	6	7,8.	8	?	
Normal Crow	11	?					
Avuncu-Crow	11A	?					
Bi-Crow	11B	4B	2	?			
Duo-Crow	11D	4P,7,8.	6,7,8.	7,8.	8	?	
Neo-Crow	11N	4N	1	?			
Patri-Crow	11P	4	4P,7,8.	6,7,8.	7,8.	8	?

The possibilities of change are as follows:

From 1B to 2; from 1M to 3M, 4M, or 9; from 1P to 3, 4, or 5 (p. 228).
From 2M to 3M, 4M, or 9; from 2N to 1; from 2P to 3, 4, or 5 (p. 230).

that these seven types have somehow the power to regenerate the original forty-seven but Murdock does not show us how this is done. Yet he claims that this classification of kinship organization may be regarded as a maze in which a society can start at any given point and arrive at any other point (p. 221).

It is our opinion that the study of social évolution is a promising field of inquiry. Indeed among contemporary anthropologists Murdock has made a major contribution in reorienting research interest back into this field. However in terms of theory construction in the field of social evolution our discussion has directed attention to several important points. The study of social evolution, and for that matter social change in general, should not be oriented towards the treatment of certain easily selected aspects of social organisation which are analysed as a 'closed system.' The limited variability of social structure may well reflect our present state of descriptive methodology and in no way justifies any attempt to construct a theory of social evolution based on the proven interrelationships of component parts in the 'closed system.'[3]

The discussion has documented our position concerning the correlational method used in conjunction with a 'closed system' approach. Correlations which are concomitant cannot be utilized for the establishment of temporal priorities which have occurred

From 3 to 6; from 3B to 2; from 3M to 10; from 3N to 1 (p. 232).
From 4 to 4P, 7, or 8; from 4B to 2; from 4M to 11; from 4N to 1; from 4P to 6, 7, or 8 (p. 234).
From 5, to 6, 7, or 8; from 5B to 2; from 5N to 1 (p. 235).
From 6 to 7 or 8; from 6B to 3B; from 6N to 3N (p. 237).
From 7 to 8; from 7B to 4B; from 7N to 4N (p. 239).
From 8B to 4B; from 8N to 4N (p. 240).
From 9B to 2; from 9D to 5; from 9N to 1; from 9P to 1P, 2P or 9D (p. 242).
From 10 to 11; from 10A to 11A; from 10B to 3B; from 10N to 3N; from 10D to 6; from 10P to 10D or 3 (p. 244).
From 11B to 4B; from 11N to 4N; from 11P to 4; from 11D to 4P, 7 or 8 (p. 247).

Column I represents the forty-seven types which Murdock distinguishes. Column II represents those types which can be arrived at by applying the rules of change. Column III represents those types which emerge if the rules of change are applied to the types in column II. This operation has been repeated for columns IV, V and VI. It should be noted that no possibilities of change are given for types 1, 2, 8, 9, 9A, 11 and 11A.

in man's social evolution. This is due to the fact that such correlations are reversible in their effects on one another and with such data evolution becomes a 'chicken and egg' question which cannot be resolved. Furthermore, if such a method is employed and social evolution is the research goal, then the researcher is easily tempted to arbitrarily choose one primary component in the 'closed system' which responds to outside influences first, then causes chain reactions of changes to occur within the system. In the study of social evolution temporal priority of causal factors cannot be assumer, they must be demonstrated, and if evolution is multilinear they must be irreversible.

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[3] In a recent article, Hsu (1959) has commented on the pitfalls facing anthropologists who attempt to do comparative analysis on certain segments of social organization, such as kinship, which are easily available.

On Certain Dental Characters of the Eskimo of the Eastern Canadian Arctic

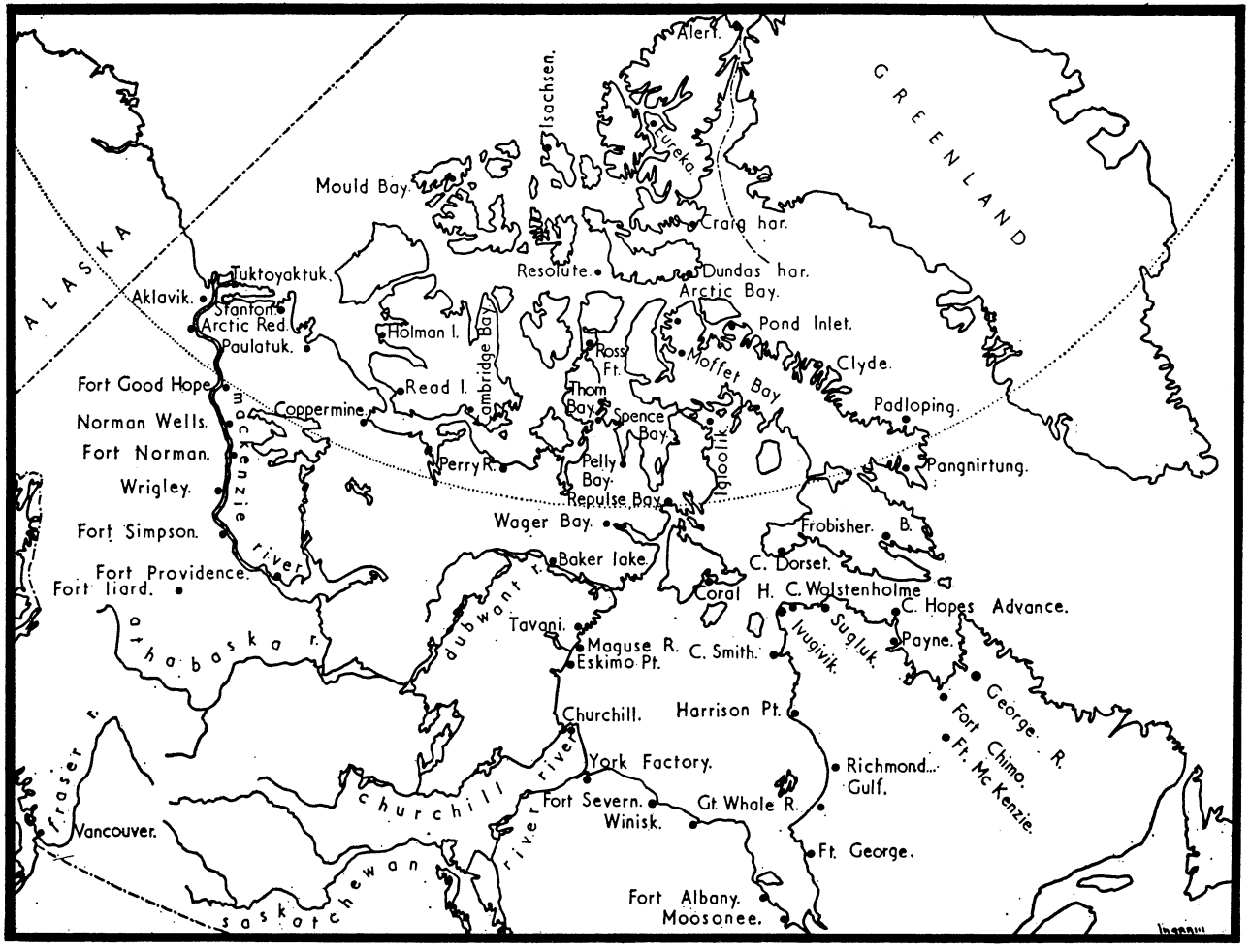
LAWRENCE OSCHINSKY AND ROY SMITHURST

The data below have been gathered during the months July, August, and September 1959 on the Canadian Research Ship C.D. Howe on a cruise in the Eastern Canadian Arctic. The Eskimos of the following settlements were visited: 1) Wakeham Bay, 2) Sugluk, 3) Ivugivik, 4) Coral Harbour (Southampton Island), 5) Cape Dorset, 6) Lake Harbour, 7) Frobisher Bay, 8) Brevoort Island, 9) Pangnirtung, 10) Broughton Island, 11) Padloping Island, 12) Kivitoo, 13) Clyde River, 14) Pond Inlet, 15) Arctic Bay, 16) Resolute, 17) Grise Fjord.

The most detailed studies of the dentition of Eskimos and related people have been done in Greenland and the Aleutian Islands while the Eastern Arctic remains terra incognita from the physical anthropological and odontological points of view. The authors were of the opinion that it would be interesting and important to study the incidence of shovel-shaped incisors, and certain aspects of maxillary lateral incisor crowding, to compare these data with the results of the above mentioned studies. Since these characters are polygenetically determined they are less subject to rapid change by such processes as genetic drift which always plays havoc with the distribution of monogenetic traits in small populations.

The dentition of mongloid peoples as a group and Eskimos in particular is characterized by a high incidence of shovel-shaped incisors, a low incidence of Carabelli's cusp, a high frequency of mandibular torus, a high frequency of an edge occlusion of the incisors and a frequent absence of third molars.

Hrdlicka in 1911 although he did not invent the term "shovel-shape" was the first to point out that this character was


Alert.
Isachsen.
Eureka.
GREENLAND
Mould Bay.
Craig har.
Resolute.
Dundas har.
Arctic Bay.
Tuktoyaktuk.
Aklavik.
Stanton.
Arctic Red.
Paulatuk.
ALASKA
Holman I.
Ombridge Bay.
Pond Inlet.
Ross Ft.
Moffet Bay
Clyde.
Fort Good Hope.
Read I.
Thom Bay.
Spence Bay.
Padloping.
Norman Wells.
Coppermine.
mackenzie river
Fort Norman.
Perry R.
Pelly Bay.
Igloolik.
Pangnirtung.
Wrigley.
Repulse Bay.
Fort Simpson.
Wager Bay.
Frobisher. B.
Fort Providence.
Baker lake.
C. Dorset.
Fort liard.
Coral H.
C. Wolstenholme
C. Hopes Advance.
athabaska r.
dubwant r.
Tavani.
Sugluk.
Payne.
Maguse R.
Eskimo Pt.
C. Smith.
Ivugivik.
George R.
Churchill.
Harrison Pt.
Fort Chimo.
Ft. Mc Kenzie.
York Factory.
Richmond Gulf.
fraser r.
churchill river
Fort Severn.
Gt. Whale R.
Winisk.
Vancouver.
Ft. George.
saskatchewan r.
Fort Albany.
Moosonee.

frequent among American Indians as well as Chinese, Japanese, and other Mongloids. He also showed that Whites and Negros had a very low frequency for this trait. Other investigators have demonstrated that shovel-shaped incisors are present in early hominids, e.g., sinanthropus, among anthropoids, certain old-world monkeys, Icelandic Nordics, and Australoids. However the major evidence clearly indicates that shovel-shaped incisors are for the most part a mongloid feature.

Hrdlicka set up a subjective scale to denote the extent of shovel-shape which is as follows:

Shovel	— the enamel rim with the enclosed fossa well developed.
Semi-shovel	— the enamel rim was distinct but the enclosed fossa is shallow.
Trace shovel	— distinct traces of the enamel rim but which could not be classed as yet as semi-shovel.
No shovel	— no perceptible trace of rim and fossa or in which traces of these were so faint or imperfect as not to deserve special characterization.

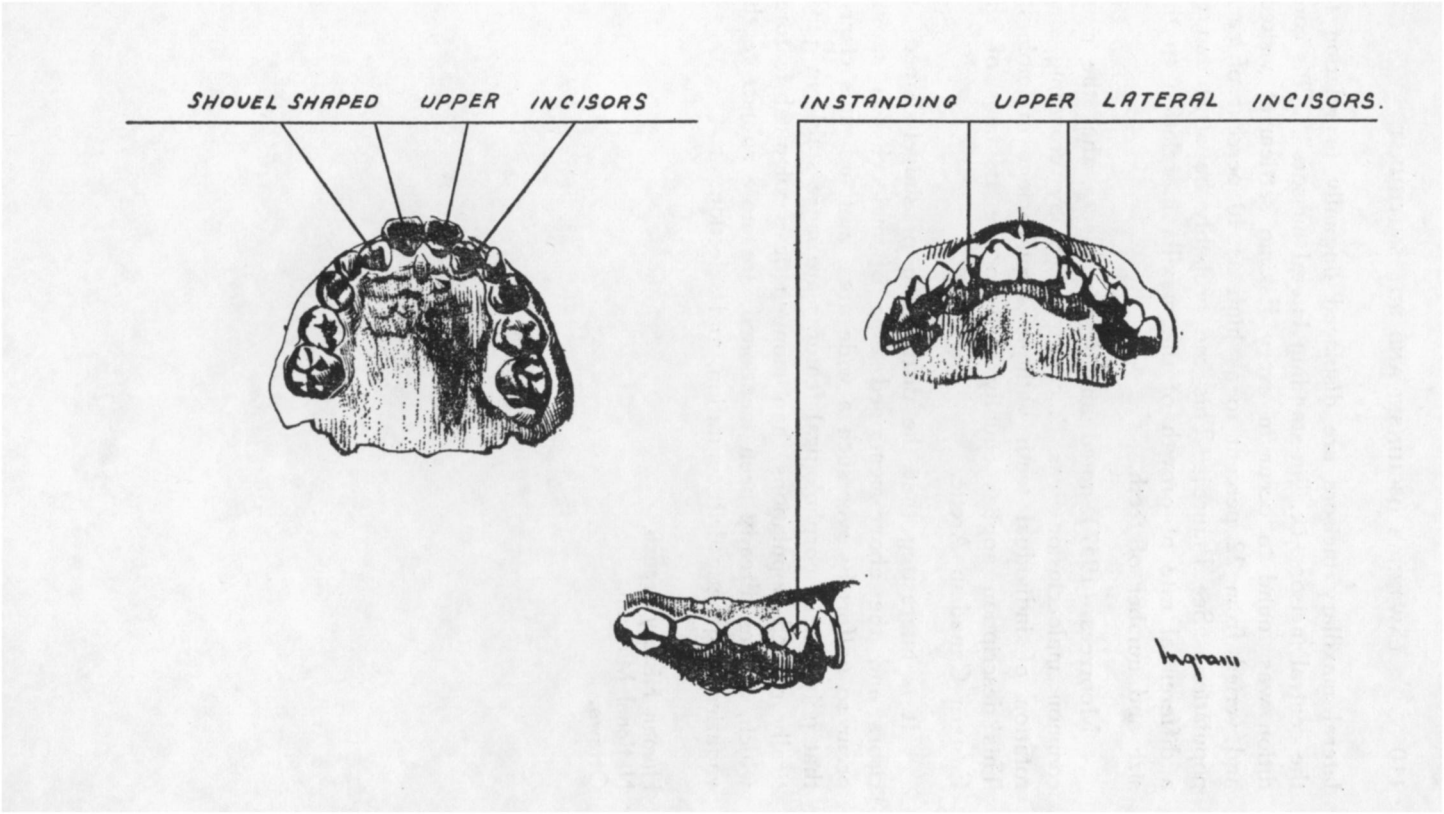
The data of the present study were graded according to the above scale but will not be presented in this detail for the time being. The authors have lumped the first three categories of Hrdlicka in order to compare their data more easily with those of other authors using different scales. As can be seen in the table below the Canadian Eskimos agree with the Greenland and Aleut data but are rather different from Hrdlicka's Eskimo series. This is probably more a discrepancy caused by different subjective methods of appraisal rather than anything else. Perhaps the metrical method of Dahlberg and Mikkelsen is to be preferred to avoid the pitfalls of various individual points of view based upon superficial description.

As has already been mentioned above, the most frequent occlusal type concerning the relationship of the incisors is an edge-to-edge or labidodont condition. Overbite or scissor bite occurred in only .4 percent of the sample. Protrusive mandible or Class 3 malocclusion, according to Angle's classification, also occurred in .2 percent. In approximately 33 percent of a sample of 2,000 Canadian Eskimos a situation obtains wherein the

Percentage Frequency of Shovel-Shaped Incisors *
Median Incisors

Group	*Author*		*Number of Individuals*	*Total of marked and semi-shoveled*	*Trace*	*None*
Canadian Eskimo	Oschinsky and Smithurst		499	99.2	0	0.8
Aleuts	Moorrees	'57	75	97.3	2.7	0
Eskimo	Hrdlicka	'20	40	84.0	15.0	
East Greenland Eskimo	Pederson	'49	116	95.3		
Mixed Indians	Wissler	'31	male 1388	85.0		
Pima Indians	Dahlberg		male 101	96.0	4.0	
Pueblo Indians	Dahlberg		21	100.0	0.0	
Sioux	Hrdlicka	'31	116	98.3	1.7	
Pecos Pueblos	Nelson	'37	324	89.5	8.3	2.2
Pecos Pueblos	Hooten	'30	124	86.3	13.7	
Early Texas Indian	Goldstein	'48	124	95.1	4.9	
Indian Knoll	Dahlberg and Snow	'	30	100.0		
Chinese	Hrdlicka	'20	male 1094	89.6	1.8	7.8
Mongolian	Hrdlicka	'20	24	91.5	8.5	
American Negro	Hrdlicka	'20	male 618	12.5	33.0	54.5
American White	Hrdlicka	'20	male 1000	9.0	24.5	66.5

* Modified from Dahlberg, 1951, "The Dentition of the American Indian" in The Physical Anthropology of the American Indian (Ed. W.S. Laughlin).


SHOVEL SHAPED UPPER INCISORS
INSTANDING UPPER LATERAL INCISORS.
Ingram

lateral maxillary incisors are displaced lingually in relation to the central incisors. i.e. "in-standing lateral incisors". This condition was found to occur in every Eskimo settlement visited, and veried from 22 percent to as high as 40 percent of each population. (See Figure). This can probably be attributed to a differential rate of growth of the maxilla in relation to the size and number of teeth.

Moorrees (1957) noted among the Aleuts, that the most common malocclusion was "...characterized by crowding and rotation of individual teeth without displacement of molars." This description applies equally well to the Eskimos of the Eastern Canadian Arctic.

It is interesting that the distribution of shovel-shaped incisors and the above-mentioned type of malocclusion should occur so uniformly over such a wide area, and indicates clearly that polygenetic morphological features are more stable in genetically related populations than mongenetic serological features which, as has already been mentioned, are more subject to the vagaries of genetic drift, mutation, and selection.

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The Middleport Horizon

BY J. V. WRIGHT

Throughout a large portion of Southern Ontario a number of Iroquois sites have been excavated which possess certain close qualitative and quantitative correspondences. It has been the spatial extent of these sites which has stimulated the term "Horizon", and, although regional variations naturally occur they appear to be of a minor nature relative to the cultural variability present in the preceding and following stages of Iroquois cultural development in Ontario. The concept of a Middleport Horizon was initially raised by MacNeish (1952: pp. 84-85) who states:

"Carrying on into the next horizon of the Pound and Middleport sites are Ontario Oblique, Ontario Horizontal, and Middleport Crossed. This period also marks the first prominence of Pound Necked, Lawson Incised, and Middleport Oblique. It is evident that about this time the Huron and the Neutral separated."

The aim of the present study is to elaborate upon this concept and to offer further substantiating data.

Artifactual an non-artifactual evidence has been used but the basic methodology follows MacNeish (1952) and Emerson (1956) in its reliance upon the analysis of rim sherds. The problem of an adequate ceramic sample presents itself at a number of the sites examined. It has been suggested by multiple ceramic analysis of single Middleport Horizon components that a sample as low as fifty rim sherds may afford type frequencies adequate for valid spatial and temporal inferences. The proposed validity of such small samples appears to stem from the few ceramic types that are present on these early sites as opposed to the more complex ceramic elaboration and cosmopolitanism of later Iroquois sites where, as MacNeish (1952: p. 92) men-

tions, a rim sherd count of from three hundred to five hundred represents an adequate sample.

The map illustrates the approximate distribution of the Middleport Horizon in Southern Ontario and New York State and the location of the six sites considered. Table I lists the basic data concerning the sites examined and Table II gives the rim sherd analysis for each site.

TABLE I

Site	*Excavator*	*Institution*	*Analyser*	*Rim Frequency*
Middleport	Wintemberg	N.M.C.	MacNeish	308
Pound	Nash	U. of T.	MacNeish	676
Robb	O.A.S.	O.A.S.	Wright	184
Millroy	O.A.S.	O.A.S.	Wright	129
Nodwell	Knechtel/Wright	—	Wright	60
Inverhuron	Knechtel/Wright/ Kenyon	R.O.M.A.	Kenyon/Wright	235

N.M.C. — National Museum of Canada
U. of T. — University of Toronto
O.A.S. — Ontario Archaeological Society
R.O.M.A. — Royal Ontario Museum of Archaeology

TABLE II

Type in Percentage	*Nodwell*	*Robb*	*Millroy*	*Middleport*	*Inverhuron*	*Pound*
Middleport Oblique	43	51	33	15	12	16
Lawson Incised	18	7	9	12	22	7
Ontario Horizontal	5	16	8	38	9	24
Iroquois Linear	13	14	—	—	3	—
Sidey Crossed	5	—	—	—	4	—
Middleport Criss-Cross	3	2	1	17	—	3
Lawson Opposed	3	1	2	—	6	P
Huron Incised	2	3	5	—	5	—
Syracuse Incised	2	—	1	—	—	—
Ontario Oblique	2	—	1	8	2	—
Black Necked	2	P	6	—	3	—
Sidey Notched	—	1	—	—	—	—
Ripley Collared	—	1	—	—	—	—
Ripley Plain	—	2	—	1	4	P
Pound Necked	—	—	19	5	10	46
Richmond Mills	—	—	2	—	—	—
Niagara Collared	—	—	—	2	—	1
Lalonde High Collar*	—	—	—	—	13	—
Seed Incised	—	—	—	—	5	—
Pound Blank	—	—	—	—	—	3
Miscellaneous	—	2	10	—	2	—

*Ridley: 1952

As can be seen from Table II, Middleport Oblique, Lawson Incised, and Ontario Horizontal have a substantial representation at all six sites and it is this trio of pottery types which is regarded as the major marker of the Middleport Horizon. Other forms are frequently in association such as Iroquois Linear, Middleport Criss-Cross, Lawson Opposed, and Pound Necked.

In addition to the distinctive pottery types the Middleport Horizon sites possess similar pipe types, projectile points, bone implements, and settlement pattern characteristics. The pipe types are predominantly of the broad, conical form and are usually decorated with very finely incised rings encompassing the bowl or a series of opposed obliques. Projectile points tend to be of the narrow, triangular, side-notched variety rather than the unnotched, triangular form which is characteristic of later Iroquois sites. This last correspondence, however, is quantitative rather than qualitative. An artifact which appears to be present on all Middleport Horizon sites is a modified deer toe bone. The proximal end of the bone is cut away leaving the exposed marrow cavity. A perforation is then made through the distal articulation. This particular artifac is frequently regarded as a unit of the cup-and-pin game but might have served equally well as a bangle. Matting or netting needles also are frequently present but their value as a quantitative marker is yet to be tested. All the sites are small, being usually less than three acres in area. The sites also appear to be situated without any regard to defence and only very tenuous evidence of palisading has been noted at one site (Wintemberg: 1948). Sites may occur on flat plains at the edge of small creeks (Nodwell and Robb), on low knowls at the edge of major rivers (Middleport) or minor tributaries (Millroy), and along lakeshores (Inverhuron). Numerous small campsites consisting of one or two middens are also characteristic of the Middleport Horizon. Some of the sites are seasonal such as the Inverhuron Site (Kenyon: 1959) which was occupied in the spring or early summer during the exploitation of fishing resources. These settlement pattern characteristics differ markedly from the western Huron of the Humber Valley area where endemic warfare led to large, palisaded villages being located on high and easily defended hills (Emerson: personal communication).

Figure I represents a partial modification of one portion of MacNeish's Iroquois developmental sequence (MacNeish: 1952, Figure 23, p. 87). Starting at the base and progressing upward a continuous sequence from the Goessens Site to the Middleport Horizon is indicated. Actually a pre-Middleport horizon appears to be represented in the Glen Meyer Focus (Lee: 1952) as reflected by the distribution of the Goessens, Stafford (Lee: 1952), Miller (Kenyon: 1960), Boys Barrie (Ridley: 1958), Frank Bay (Ridley: 1954), and Sugar Island (Ritchie: 1949) sites. An effective ceramic typology, however, must be established before the interrelationships of these widely dispersed sites to one another can be evaluated. Sometime between the Uren period and the Middleport Horizon, influences from Southern Ontario were reaching into western New York State resulting in the occurrence of Iroquois Linear and Ontario Horizontal on the Dansville and Long Point sites. To what extent the local Seneca development was affected by these influences is difficult to say but that a relationship was established between the two areas is strongly suggested by ethnohistoric data as well as the distribution of pottery types (Guthe: 1958). For example,

"The westernmost Seneca settlements were especially friendly with the Erie. On both sides of the Niagara river were the villages of the Attiwandaronk or Neutral, considered an old and parent body of all the Huron-Iroquois. Within one of their villages near the Niagara lived Ji-gon-sa-seh, "The mother of Nations," a woman who was a lineal descendant of "the first woman of earth." (Parker: 1922, p. 158).

And,

"Up to this time the early Seneca had mingled more with the Erie and the Neutral nations, than with the Onondaga and Mohawk." (Parker: 1922, p. 207).

In the eastern and the northern portions of Southern Ontario, sites of the Middleport Horizon were gradually favouring pottery types characteristic of the Huron. Present evidence suggests that the Petun are archaeologically identical to the Huron and the Inverhuron and Nodwell sites are inserted in a hypothetical branch

leading up to the Petun solely on the basis of their geographical occurrence in the historic Petun area and not on the grounds of ceramic differences. Historic Petun ceramics (unpublished data) are virtually indistinguishable from the neighbouring historic Huron ceramics thus suggesting that the tribal units represent an archaeologically artificial separation. Ethnohistoric evidence tends to substantiate the archaeological evidence. In the southern area of the Middleport Horizon a separation apparently took place around Southwald times leading to the differentiation of the Erie end the Neutral tribal units.

In conclusion, the evidence derived from the Middleport Horizon sites creates the impression of scattered, small groups of farmers living in relative peace and receiving only slight external influences. From this Middleport Horizon base it is postulated that internal change was largely responsible for the eventual differentiation of the Neutral, Erie, and Huron-Petun tribal units.

Human History Branch,
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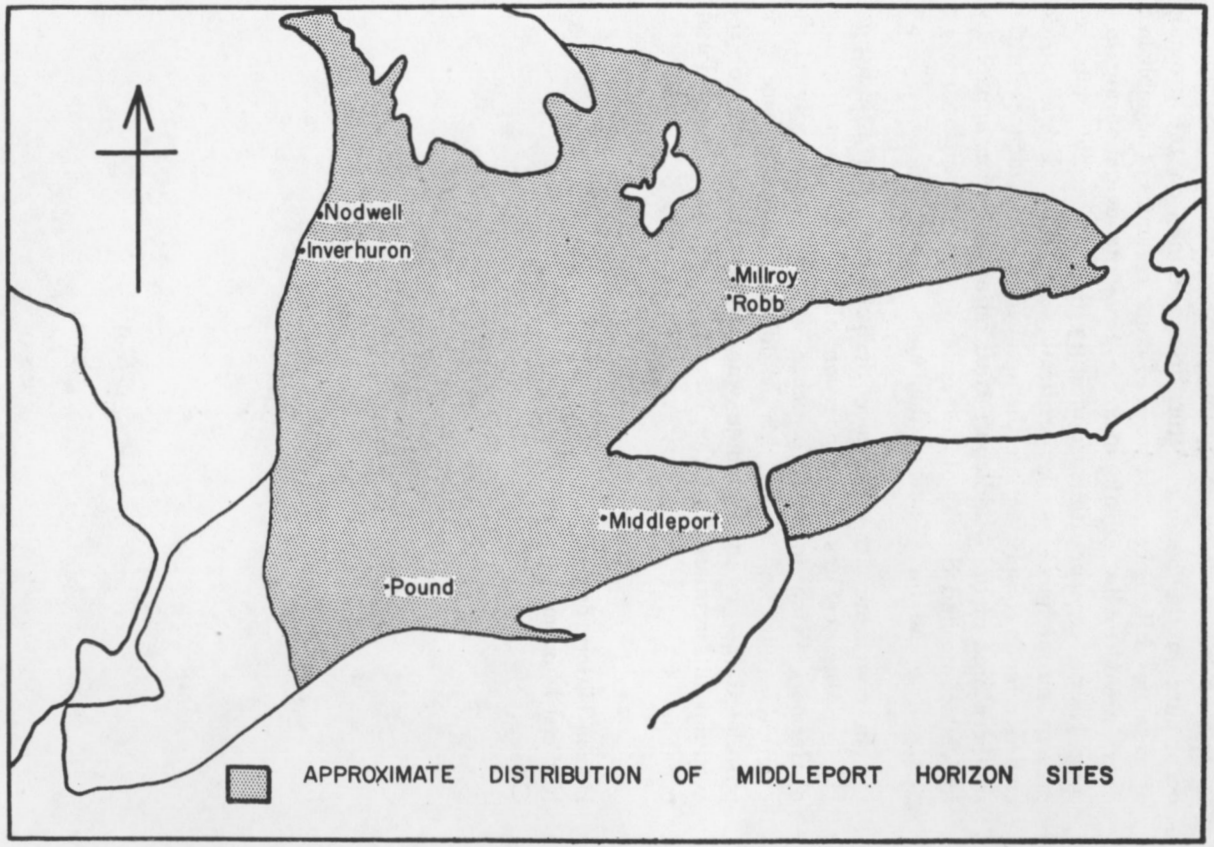
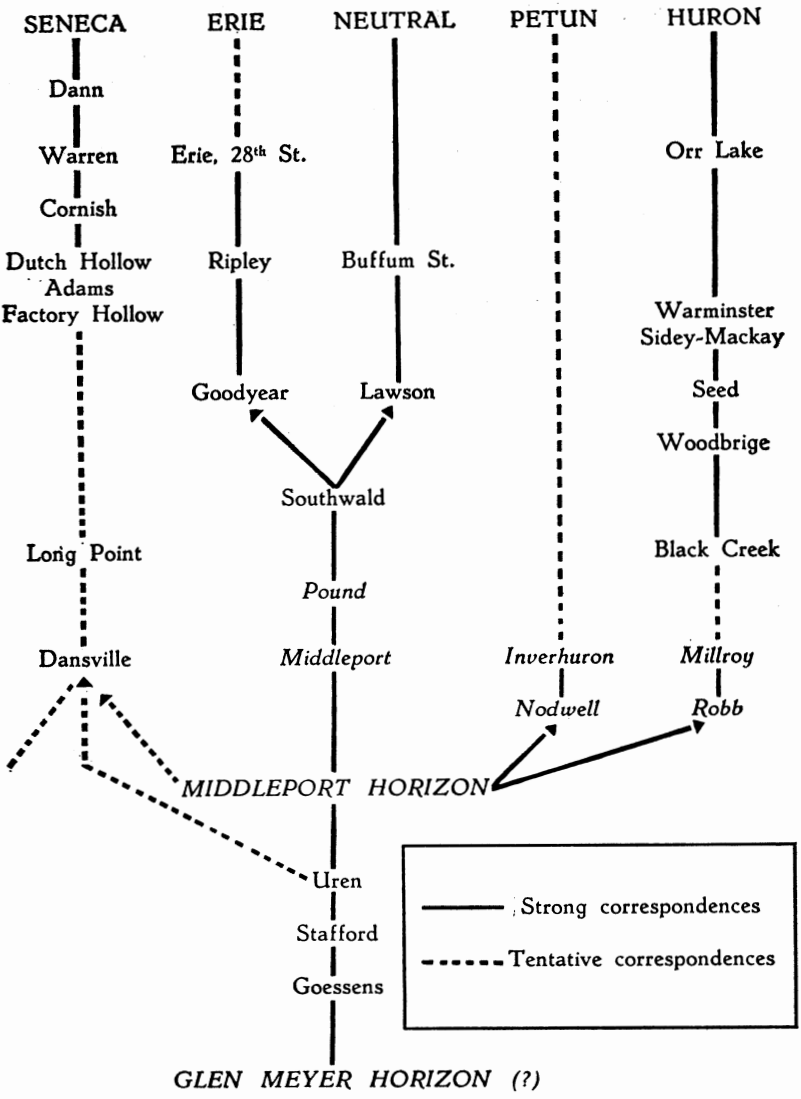

Nodwell
Inverhuron
Millroy
Robb
Middleport
Pound
APPROXIMATE DISTRIBUTION OF MIDDLEPORT HORIZON SITES

FIGURE I



DEVELOPMENTAL SEQUENCE OF THE ONTARIO IROQUOIS

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Recensions -- Book Reviews

Social and Economic change among the Northern Ojibwa. R.M. DUNNING, University of Toronto Press, 1959, X, 217 pp. 30 plates, 16 line drawings. $5.50.

Dr. R.W. Dunning in "Social and Economic Change among the Northern Ojibwa" offers a major work and interpretation of the Ojibwa of northern Ontario. This study, the first of its kind to deal so exhaustively with the contemporary socio-economic conditions of an Indian community in the eastern subarctic of North America, is a most welcome and valuable contribution. The following remarks are not meant to detract from the merit and scholarly presentation of the monograph. Nevertheless, a few comments are in order.

Although Dunning claims (footnote page 187) to have had a working knowledge of the Ojibwa language, it is most unfortunate that he did not attempt to employ a consistent system of transcription based on the extensive linguistic work of Bloomfield and others among the Ojibwa and Cree. In addition, the native names for certain plants and animals appear to have been incorrectly translated. Dunning gives *shengop* (page 21) for hemlock and balsam. Cognates of this term are employed to the north and south of Pekangekum for black spruce and a special term is generally used for balsam. Hemlock, on the other hand, does not grow within several hundred miles of Pekangekum. On page 23 the term *numay* is said to mean sucker whereas elsewhere within the eastern subarctic the term is used for sturgeon. It would have considerably improved this section if the Latin names of the species had been inserted. An English name sometimes refers to several different species. The word "pickerel", (page 23), for instance, in northern Ontario refers to *Stizoxtedion vitreum,* but in many published sources devoted to the study of fish to various species of *Esox.*

There are some technical errors in his description of subsistence activities. "When the ice has formed to a depth of 24 to 36 inches the traps (for beaver) are laid" (page 25). The implication is confusing since the depth of the ice makes no difference in the capture of beaver. Furthermore, although nets are employed for the capture of beaver throughout the eastern subarctic they are of much heavier construction than gill nets (page 26) which would never hold a beaver. Finally as regards the capture of beaver, the Indians of northern Ontario did in the past break into a beaver house by a method called *e·ske·.* While the practice has decreased it is to be questioned that it has vanished (page 26). In my experience informants suppress mention of this technique since it is forbidden by law and strong pressures were brought to bear in the past to eliminate various trapping violations.

The problem of land ownership is not clearly presented. Dunning says that the community or band communally owns the land (page 48) but modifies this later (page 105) by excepting the right to trap fur bearing animals. This right was vested in the co-residential group. But on page 55 he states that the co-residential group has had communally owned hunting territories since the first records in 1876. And he further states (page 107) that it is the "...trapping group which ultimately controls rights in use as well as rights of disposal of trapping territory." There seems to be an unnecessary amount of confusion over the owners (band or co-residential group) and what is owned (right of usufruct or the land itself).

Another problem that is not clarified in this work is the use of the term "band." In a footnote on page 153, Dunning suggests that there was no band organization in the past and yet on page 186 he speaks of the "traditional leader of the band." Was there or wasn't there a band? If there was a band in the past it is not fair to use the same word for the present grouping which has altered its character tremendously, as pointed out by Dunning (pages 20, 47, 183-185). If there was no band in the past then the term can be used for the present grouping in the sense employed by the government to mean an administrative unit. But this is not the same as what is meant by the term "band" in the anthropological literature.

A point of some signifiance in Dunning's study concerns his description of the kinship system. Since the system can be classed as bifurcate collateral as Dunning states (page 47), his quoting of Hallowell to the effect that it is bifurcate merging (page 74) seems out of place. Hallowell employs the term not in its commonly accepted meaning but in a manner that would include many if not all kinship systems found the world over. On page 76 Dunning suggests that the differences in kin terms between Pekangekum and all other Ojibwa groups fit in with the changing structure of the community. But the differences in terms can be more logically explained as dialectic variants. If this is not so it is still difficult to follow his argument that they reflect structural changes. Dunning in his kinship chart (page 110) gives "son-in-law" and "daughter-in-law" terms for the spouse of cross niece and cross nephew respectively. While I do not doubt the validity of this at Pekangekum, it does pose certain problems not examined by Dunning. If bilateral cross-cousin marriage is practiced, as Dunning states, how is it that the spouses of cross nephews and cross nieces are called by affinal terms and consanguinial terms as would be expected? This suggests a changing kinship pattern but no notice is taken of this, although the study deals in part with changing kinship behavior. Another point in kinship is the lack of clarity regarding its extension. Dunning implies that the kinship system from Ego's point of view encompasses the entire group regardless of the distance of genealogical connection (pages 72-73, 199). But in several recorded cases of marriages the third collateral line seems to be the limit (page 116), as among the members of the Trout Lake Band to the north of Pekangekum. Finally, in Table XXV (page 154) four marriages are

listed as between non-relatives, suggesting that Ego does not include all members of the community as kin, either affinal or consanguinial.

Finally, I question Dunning's interpretation of the use of the term "totem" when employed in the phrase quoted (page 82) of a Dear Lake Band informant. Certainly to the east at Round Lake such a statement would be translated as "everyone is my fried or relative," and would not imply that everyone was "brother" as Dunning has done in this case. This would be a minor point except for the fact that it is used to strengthen his argument in favor of the solidarity of brothers.

This leads to a final major consideration of Dunning's argument. Dunning's major thesis is that formerly group size and dispersal were restricted and controled by the "ecology." With the advent of subsidies and a money economy the ecological controls were no longer operative. There was an increase in population, size of co-residential group, and solidarity between brothers. The increased solidarity between brothers is thought to be shown by the change in residence pattern (page 108) from partial uxorilocal residence to a predominance of virilocal residence. In addition band endogamy came into effect along with band unity and exclusiveness.

Criticism can be made of one point in this argument: Dunning's statement that the solidarity of brothers has become strengthened through the lessening of ecological controls. First, he gives no indication as to what the bond between brothers was in the past. Secondly, he bases the change on the fact that the number of cases of virilocal residence has increased in the past forty years. His use of percentiles (pages 62-63) to show this trend when his number of cases is well below 100 does not seem legitimate. (This comment also applies to the use of percentiles to show changing incidence of cross-cousin marriage, page 151.) Furthermore, he reduces the number of permanent uxorilocal residences by stating that the immigrant husbands had no other choice within the band (page 61). Although this may be true could they not have returned to their natal band? In addition, his figures for the past are as he himself says, "perhaps not quite so accurate." If the data as given by Dunning are treated statistically in a four-fold table the results barely support his contention of significant residence change. If the three immigrant husbands are included or a reduced time span employed there would be no significant change in residence pattern.

	Case of Residence		
	past	present	
Uxorilocal Residence	5	3	8
All Forms of Residence	18	62	80
	23	65	88

Chi-square equals 6.0. P equals approx. 0.05 with two degrees of freedom.

Since the numerical changes in residence pattern do not appear to warrant the weight accorded them by Dunning without much more corroborative

data, then the statement (page 208) that "Both residence patterns and camp conflicts reflect the development of... the solidarity of same-sexed siblings" is not substantiated.

The weight of criticism frequently obscures the positive contributions of an ambitious work such as Dr. Dunning's. In spite of the technical errors and a few questionable interpretations, I would again like to stress the value of Dr. Dunning's work. He has brought much clarity, originality, and sound observation to the social and economic organization of the Cree-Ojibwa of the subarctic. His point regarding past residence patterns (page 49) in which the need for continuity of the co-residential group is uppermost in determining residence solves a much misunderstood situation. Another excellent contribution, perhaps the most important, in his discussion of the random nature of cross-cousin marriage (page 154). Other points of value, such as the wealth of detailed information this work contains, could be mentioned. There is no question that Dr. Dunning's work makes a fine contribution to the literature on the Cree-Ojibwa peoples.

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A Journey from Prince of Wale's Fort in Hudson's Bay to the Northern Ocean 1770, 1770, 1771, 1772. Camuel HEARNE. Edited with an Introduction by Richard GLOVER. Toronto: The Macmillan Company of Canada Limited, Publishers 1958. xliii, 301 pp., 6 illustrations, 1 map, $6.50.

Samuel Hearne was employed between 1769 and 1772 by the Hudson's Bay Company to locate a copper mine that had been reported as existing to the north and west of the Churchill River near the Arctic coast. After two unsuccessful attempts, the explorer, accompananied by Chipewyan Indians, reached the Coppermine River traveling overland across the Barren Grounds from Prince of Wale's Fort at the mouth of the Churchil River. The original edition of Hearne's account of this remarkable feat of exploration was published in 1795 and has long been considered a very rare book. In 1911 the Champlain Society published a limited edition of the journals edited by J.B. Tyrrell and it too is difficult and expensive to obtain. A need thus existed for a readily accessible, up-to-date edition of Hearne's travels, and this scholarly edition by Professor Richard Glover of the University of Manitoba fulfills the requirement admirably.

In his introduction, the editor provides the most detailed biographical information available on Hearne and his discussion of the explorer's early

career in the British Navy helps to understand why he was seemingly oblivious to the difficulties and physical hardships encountered on his overland journey. Professor Glover's access to the archives of the Hudson's Bay Company has made it possible for him to indicate clearly the role of Moses Norton, Governor of Prince of Wale's Fort, in planning Hearne's three expeditions. The Governor desired to convince his superiors in London that he was carrying out important explorations and although the presence of copper and its inacessibility for commercial purposes had long been known, he persuaded them that a white man should be sent to chart a route to the copper mine. It is no credit to Governor Norton that Hearne eventually achieved success on a hopeless task

The editor emphasizes the importance of Hearne's contributions to the methodology of exploration and praises his abilities as a pioneer field naturalist. Errors in surveying and mapping are defended on the grounds that at no time did Hearne conceive of his task as one of geographical exploration. He was simply to find the copper deposits and report their existence to his superiors. Of particular interest to anthropologists is Hearne's graphic and detailed description of the Chipewyan Indians and his remarkable biography of the guide Matonabbee.

Professor Glover's work naturally invites comparaison with J.B. Tyrell's pioneer study, and without minimizing the work of the latter editor, it is possible to say that Professor Glover has utilized all the ressources of modern historical research to stress the significance of Hearne's magnificent accomplishment. Of particular importance in this regard has been the opportunity to compare the published version of the third expedition with a near-contemporary copy of the original journal in the British Muesum.

Professor Glover is certainly to be congratulated on a thorough and scholarly job. The only improvement that suggests itself would be to have included a modern map on which Hearne's route could be traced.

James W. VANSTONE,
University of Toronto.

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Man's Way: A Preface to the understanding of Human Society. Walter GOLDSCHMIDT. New York, Henry Holt and Co. Inc., 1959, 253 pp. $2.90.

Le principal mérite de ce livre de synthèse anthropologique c'est qu'il est justement une synthèse de théorie anthropologique. Sans employer le mot "interdisciplinaire", le professeur Goldschmidt non seulement tient compte des points de vue historique, psychologique et sociologique mais il démontre

que chaque point de vue, pour être intelligible, requiert les autres. L'aspect le plus original de l'exposé de l'auteur, c'est d'être centré sur deux théories qu'on ne voit pas souvent discutées ensemble: le fonctionnalisme et l'évolutionnisme.

Après avoir rappelé les principales caractéristiques de la nature biologique de l'homme, il fait appel à un besoin universel de l'homme qu'il pose en postulat: "the need for positive affect". Ce besoin consisterait dans le désir que l'homme a de contacts humains, de reconnaissance, d'acceptation, d'approbation, d'estime et de domination. Ce besoin ne serait pas un produit de la vie en société mais découlerait de la nature sociale même de *l'homme; il pré-existerait à l'apparition de la culture!....*

La thèse fonctionnaliste implique que pour survivre toute société doit posséder certains éléments que l'auteur appelle "impéritifs sociaux"; ces impératifs sont au nombre de six: groupes, valeurs, statuts, rôles, contrôle et idéologie. La façon dont chaque société répond à ces impératifs universels varie de société à société. Selon l'auteur, les différences les plus importantes peuvent s'expliquer par le système technico-économique de chaque société. Trois chapitres sont consacrés aux mécanismes de l'évolution sociale, à l'évolution et aux impératifs sociaux et enfin à l'évolution de la société. Pour l'auteur, la théorie évolutionniste est considérée comme une théorie fonctionnaliste parce que dans la première comme dans la deuxième les institutions y sont considérées comme des mécanismes d'interaction sociale qui servent à perpétuer la vie d'une société et que tout changement technologique à l'intérieur d'un système social affecte les autres éléments de ce système. Le Professeur Goldschmidt traite la maîtrise du milieu comme variable indépendante tandis que le reste du système qui varie avec la technologie est considérée comme variable dépendante. L'un des seuls reproches qu'on pourrait faire à l'auteur, c'est de choisir ses exemples chez les peuples primitifs surtout et de négliger ainsi les peuples historiques et contemporains.

Le livre est écrit dans une langue qui ne s'embarasse pas du jargon qui fleurit dans trop d'ouvrages de ce genre.

Marcel Rioux.

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Africa: Its Peoples and Their Culture History. G.P. Murdock. Toronto: McGraw-Hill (Canada), 1959. xiii, 456 pp., plus tribal map. $11.75.

The excellent quality of anthropological field studies coming out of Africa in the last several decades have left little to be desired when judged as social anthropological accounts of particular groups of people. Indeed

much of the orientation of social anthropology has found its theoretical force in the African researches of workers like Fortes, Evans-Pritchard, Gluckman, Forde, and their students. However in terms of the more various interest in man manifested by American anthropology there are large gaps in the African literature. The ethno-history, folklore, art, personality and culture, and socio-cultural evolution of African peoples have received comparatively little attention from the anthropologists in recent times.

Professor Murdock has attempted to fill some of these gaps by establishing a base line in our knowledge of Africa as a whole. To do this he selects only a few of the sectors of ethnography, such as food-producing activities, the division of labour by sex, housing and settlement patterns, kinship and marriage, the forms of social and political organization, and a "few miscellanea such as cannibalism and genital mutilation" (p. viii). The time span encompassed begins about 7000 years ago with the Neolithic and ends with the conclusion of European colonial penetration around the beginning of the present century. This is where Murdock breaks new ground. His major purpose in synthesizing the ethnographic material is to reconstruct the culture history of the African continent.

After considering certain general problems on the nature of society, history, language, race, geography, and government as these relate to the entire continent, the author treats each area of Africa separately in what he hopes is some kind of chronological order. That is to say, major historical events in one chapter often include antecedent events of major concern to the peoples described in subsequent chapters. The basis of classification is partly historical, and partly cultural. Large areas showing regional unity in their historical development, such as the "Sudanic agricultural civilization", are subsequently divided up into culture "provinces" which approximate to the more traditional concept of culture area in North American ethnology.

Since history is the major aim of the book, the reader should be well aware of the methods used by Murdock in performing his reconstruction. As is usual with this author he conscientiously lays out his methodology for all to see and we are told (p. 42) that the materials utilized are as follows: (1) written records; (2) archeology; (3) linguistic relations; (4) distribution of domesticated plants and animals; (5) the methods and rules of reconstruction developed by Murdock in his book *Social Structure* (1949); (6) the age-area theory as applied to ethnographic data in areas of continuous distribution of cultural forms. The author claims that he "has not encountered a single instance in Africa when suggestive leads from two or more of these sources (the six methods) have indicated inconsistent conclusions" (p. 43).

Certainly the study has turned up at least surprising if not absolutely validated conclusions on the history of Africa. We are told that agriculture was independently developed on the upper Niger in the fourth millenium B.C., and that the sudanic states arose there between 2000 and 1000 B.C.

The trans-Saharan trade is said to have begun in second millenium B.C., much earlier than has previously been supposed, and Madagascar is said to have been unpopulated before the Indonesian (from Borneo) speaking peoples arrived by way of East Africa to inhabit the island. In a surprisingly high number of areas Murdock attempts to demonstrate that matriliny has preceded the contemporary patrilineal institutions in the social organization. These are only a few of the many new pathways that the author establishes. What may seem to the uninitiated as a mere synthesis of contemporary knowledge is to the specialist a literal mine of new ideas and hypotheses about African prehistory.

Naturally in a work of such comprehensive scope, the documentation is hardly constant througout. The culture history of Madagascar is very amply documented and would require very weighty evidence to disprove, while that on the dating of trans-Saharan trade rests on the "fact" that "the bedrock along caravan trails has been polished smooth by the bare feet of countless thousands of human porters before animal transport came into general use" (p. 127). On the other hand some of his insights are fairly healthy inferences based on closely related data. Thus the well documented early inception of agriculture in the western Sudan allows him to infer a second milienia B.C. date for the rise of nation states in this area, even though our earliest previous date is only 100-500 A.D. (i.e. the beginning of Ghana).

The problem of matrilineal origins is a thorny one. Here Murdock relies heavily on his own methods of sociological reconstruction devoloped in *Social Structure,* and on distributional (age-area) information. Elsewhere in this journal reasons are given for doubting the complete trustworthiness of the so-called evolutionary theory of Murdock. It is this review's judgment that when inferences rest solely or more strongly on the 1949 theory without much additional distributional evidence, the validity of a previous matrilineal state is less substantiated. It should be remembered in this connection however, that as long ago as 1937 Murdock established correlations between matrilineal institutions and less complex forms of socio-cultural development, while patrilineal institutions were correlated with the more complex forms. This leads to the hypothesis that where simpler cultures have been replaced by more complex ones, there is a least the possibility that matrilineal institutions may have preceded patrilineal ones. Whether or not Murdock has proven his case completely on the basis of his 1949 work, he has done convincing work even earlier which suggests that we should not throw out his African reconstruction without some serious consideration of their worth.

A final criticism that this reviewer feels should be made concerns the author's attitude towards several of his fellow anthropologists. He claims that "Work in the African field whether historical, or scientific, which the author considers definitely bad, he simply ignores in this book, whatever the reputation of the person responsible." (p. 41). Except for one bibliographic reference (p. 258) Herskovits is never mentioned. Radcliffe-Brown's

view of historical anthropology is interpreted as a "typical Freudian reaction formation" (p. 41), and his contribution to anthropology is largely placed in the wastebasket of erroneous ideas which have been disproven every time they have come into question (pp. 41, 378). To point out that Junod may have been correct in his interpretation of the mother's brother relationship among the Thonga, while Radcliffe-Brown was wrong in one thing; to publicly accuse the latter of some neurotic adjustment to another school of anthropology than his own requires documentation as does any statement about human behaviour. Could it be that Professor Murdock is himself reacting a bit emotionally?

Africanists can and will criticize this book on particular points in every area. However it must be realized that except for some rather poorly constructed attempts by Kulturkreise workers, this is the first time a "generalizing" anthropologist has given us a complete coverage of the ethnohistory of Africa. As Professor Murdock admits, it is a base line. If we disagree with this or that point, then it is our job to clear up the errors and make each area and its history known more precisely. In other words, this book is in the fanal analysis, a pioneer attempt. We are indebted to its author for having given us enough insights and hypotheses to keep historically minded anthropologists interested in Africa, busy for the next decade.

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The World of Men. John J. HONIGMAN. New York: Harper & Brothers, Publishers, 1959. XII, 971 pp.

Depuis quelques années les manuels d'anthropologie paraissent aux U.S.A. à un rythme accéléré. On souhaiterait un meilleur équilibre: quelques-uns de plus en français et quelques-uns de moins en anglais. Ce qui ne veut pas dire que celui du Professeur Honigman ne soit pas bienvenu; il ne fait aucunement double emploi avec ses devanciers car il fait état des résultats et des tendances les plus récents de l'anthropologie. Bien que "The World of Man" soit avant tout un ouvrage d'anthropologie culturelle, il traite suffisamment d'anthropologie physique, d'ethnologie et d'archéologie pour que l'étudiant ou tout autre usager ait une vue d'ensemble de l'anthropologie.

Ce manuel présente un certain nombre d'innovations qui en font le manuel le plus complet qui ait été publié. Par exemple, une des sept parties de l'ouvrage (p. 23 - 121) traite des méthodes et des techniques de recherche des principales disciplines de l'anthropologie; non seulement le volume présente-t-il les résultats des études spécialisées mais il discute de la

façon que ces travaux ont été conduits. Rares sont les autres manuels qui sont aussi explicites là-dessus. En plus de très nombreuses bibliographies, plusieurs chapitres contiennent des listes de films ethnographiques qui illustrent les questions à l'étude. Encore une heureuse innovation! Pour n'en mentionner qu'une autre, disons qu'une partie du volume est consacrée au développement de la culture occidentale; souvent les manuels d'anthropologie isolent artificiellement l'Occident du reste du monde.

Pour les Canadiens, "The World of Man" présente un autre avantage; l'auteur ayant longtemps travaillé au Canada, illustre son ouvrage de beaucoup d'exemples tirés de nos sociétés indiennes et esquimaudes; les travaux d'auteurs canadiens y sont cités.

Du point de vue théorique, on peut dire que l'ouvrage est éclectique. Est-ce un défaut ou un avantage pour un manuel? Chacun en décidera selon ses préférences. Disons en tout cas que l'auteur ne se fait pas faute de citer ses devanciers et qu'il donne, somme toute, un bon état de la question anthropologique.

Marcel Rioux

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The Death of Adam. John C. GREENE. Ames, Iowa: Iowa State University Press, Thomas Allen Ltd., Publishers, 1959, 388pp., illustrated. $5.40.

This book is concerned with the development of the physical and biological sciences in the period from the late sixteenth century to the publication of Darwin's "Origin of Species". The author gives particular attention to the process by which the scientific method came to replace theocratic determinism as a basic speculative philosophy.

The particular problem that is posed by the author is the growing awareness on the part of scholars that the history of the natural world went much further back into the past than a literal interpretation of the account of creation as given in the biblical book of Gensis suggests. The apologists among the biologists and other scientists maintained, among other things, that biological species are immutable since the concept of extinction or mutation of species, to their way of thinking, had been dismissed, ex cathedra, by Aristotle. In an age when Archbishop Ussher's pronouncement relating to universal creation went unquestioned, there could be no possibililty of granting an antiquity to man earlier than 4,004 B.C.

The discovery of fossils of marine animals on mountain tops, of giant extinct reptiles and mammals and of exotic species in remote areas and stratigraphic geological data introducted an element of doubt in the minds of the various newly-formed learned societies. The doctrines of uniformitarianism

advanced by the pre-Darwinian evolutionists, namely Buffon, Lamarck, St. Hilare, Lyell and Erasmus Darwin gradually replaced the theory of catastrophism of Cuvier and his school who would not, under any circumstances, admit the possibility of the mutability and gradual evolution of species.

It is apparent that the author has delved deeply into the primary sources which form the basis for his synthesis and all those who are interested in the development of the theory of organic evolution will find this book most stimulating. Perhaps the author might have dealt more fully with the debt which the post-Renaissance scientist owes to such Greek and Roman thinkers as Xenophanes, Democritus, Epicurus and Lucretius. The illustrations are poorly reproduced and do not accord with the otherwise high standard of the publication.

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Human Evolution. Noel HORN, and Harry S. SMITH. New York 17: Henry Holt and Company, 1959, 447 pp, illustrated. $5.50.

Those of us who teach or have taught physical anthropology will almost certainly welcome the appearance of this book of readings extracted from various publications dealing with human evolution and racial differentiation. The work is divided into seven parts or sections including discussions on the scope of anthropology, genetics, fossil man, and theories of racial origin. There is an equal emphasis on the processes as well as the products of evolution which reflect the contemporary trend in this field.

Many readings quoted are taken exclusively from American, English, and French sources but none are given from Italian, Spanish, Belgian, Dutch, German or Scandinavian works. Often the readings are extracted from accessible books such as Boule and Vallois "Fossil Men", and Hooton "Up From the Ape" rather than from the original monographs and papers.

It would have been interesting if Weidenreich's and Hooton's criticisms of the limitations of the serological-genetical approach had been included since otherwise the uninformed reader might erroneously assume that morphology has no place in modern racial taxonomy. Since morphology is still regarded as a valid criterion in palaeontology and zoological taxonomy it is strange that some anthropologists and haemotologists are of the opinion that it is inapplicable to study studies of human racial taxonomy.

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The Eskimos. Kaj BIRKET-SMITH, Forward by C. Daryll FORDE. Translated from the Danish by W.E. CALVERT, revised by C. Daryll FORDE, Methuen and Co. Ltd., London, 1959. XV, 262 pp., 67 illustrations and 3 appendices.

This is the revised and enlarged edition of a 1936 text by the doyen of arctic ethnographers. It is based on wide personal field experience and a broad knowledge of the pertinent literature. Most aspects of Eskimo anthropology are covered in a remarkably rich summary. While it is not, unfortunately, the "best of Birket-Smith," it does several things. Among them, it corrects many misconceptions, including the common classroom error that Eskimo culture is homogeneous. Distinguishing varieties of Eskimo culture is one of the book's basic themes.

The ten major chapters deal with the discovery and conditions of the country, "Outer Character and Inner Qualities," the language, the food quest, "Fighting the Cold," The Society," "View of Life," prehistory, and the Eskimo-white contact. These are preceded by an excellent "Introductory," and followed by appendices summarizing Eskimo tribal groups, noting some rules of pronunciation, and providing a general bibliography.

The chapter, "Outer Character and Inner Qualities" is perhaps the least astute. The author's concept of "race," although unstated, seems to require refurbishing and poorly supported arguments are occasionally evident. After Collins' comments on the matter one is surprised to see the distinctive Eskimo mandibular morphology attributed to diligent chewing. It is surprising also to read (page 37) the unsupported statement that the Australian aborigines, "physically seem to occupy a lower stage than the rest of mankind." The few pages on "Inner Qualities" are well-motivated and offered with clearly-stated reservations. Nevertheless they will form a trying passage for any personality-culture specialist. The reference to McDougall's instinct theory will seem anachronistic to many, especially "the instinct of subjection" (page 52). On page 51, one reads, "A primitive trait which must not be forgotten is an often rather pronounced lack of self-control and a consequent absence of the feeling of responsibility." Page 53 notes that, "The Eskimos are easily influenced..." Page 54 adds, "The lack of any feeling of responsibility can make them cold-blooded witnesses of murder..." Conversely on page 55, the writer observes, "They are individualists insofar as nothing is considered more repulsive than aggressiveness and violence, while on the other hand, far-reaching helpfulness among campfellows is an inevitable duty."

The chapter "Origin and Development of Eskimo Culture" has been greatly revised but the result is rather a Cretan labyrinth with the inland origin hypothesis at its centre in the rôle of minotaur. It is some 35 years since Birket-Smith presented the most developed statement of that hypothesis. With arctic archaeology then barely born, the inland origin view of Eskimo culture was based on ethnological data and inference. The body of archaeo-

logical knowledge has grown since then. The present chapter indicates the great difficulties experienced by those trying to fit that coarse, cold body to a pre-tailored ethnological suit. The book is not marked by a theoretical or methodological sophistication and nowhere is this more evident than in the chapter on prehistory. One reads, for example, "An element that does not harmonize with the cultural pattern must be foreign;" (page 178). It would serve as an interesting hypothesis, perhaps, but for the moment it is not a demonstrated law. Exceptions readily come to mind. On page 179, the single explanation offered for the occasional spread of traits alone, rather than "in associated groups," is that, "...man's conservative tendencies will lead to selection from among any new elements which may appear." Even as *one* of the reasons, it is no explanation but only the illusion of explanation. In addition, the chapter is scarred by errors: polyhedral cores and microblades are found much farther south than Fort Liard (page 187) and their occurrences in Oregon, Washington, and British Columbia are germane to arctic prehistory; that "...the present Central Eskimo very seldom use trace buckles..." (page 188) is at variance with my limited observations and the much broader experience of others I have questioned; it is generally held, and supported by C-14 dates, that Near Ipiutak culture is older, not younger, (page 189) than Ipiutak; the "whole" pre-Dorset "complex from the eastern Arctic has" not "been named Sarqaq" (page 191); published data indicate that true burins are not absent in Dorset culture (page 191); it is incorrect to say of Dorset that "Art does not appear till the late periods," (page 191) (both of these incorrect statements on Dorset use generally unpublished data to refute published data); it is by no means certain that, "The use of polished slate in the early Dorset phases was a transient phenomenon taken over from the Woodland cultures" (page 195).

A final brief chapter, "Eskimo and Whites," summarizes the contact and present situation in Alaska, Greenland, and Canada. Commenting only on Canada, I find the revision inadequate. While noting the federal government's positive attitude, efforts of the past decade in health, education, administration, and economic development have been largely ignored — no mention is made of the dental and tuberculosis program, nursing stations, rehabilitation centres, crafts development, job-training, school expansion, Northern Service Officers, etc. Conversely, Birket-Smith generously remains silent of our errors, failures and shortcomings in these areas. While the Hudson's Bay Co. is not an angelic host in the north, neither is it so mean and massive a monster as suggested by the author.

This generally negative review is not a just reflection of its subject which contains an abundance of excellent data and is surely as readable a summary of Eskimology as is available. I have focused on the three chapters found least impressive in order to offer a caveat to the many readers who might accept Birket-Smith's account as fiat.

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Brefs comptes rendus - Book Notes

Anthropology in India, 1957-1958. Calcutta: Department of Anthropology, Government of India, Indian Museum, 1959. 39 pp. (not for sale).

A booklet containing details of the activities of the department of Anthropology of the Government of India for 1957 and 1958. In this issue are given lists of the Centers for Anthropological Research in India, the research projects sponsored during 1957-58, and publications on Indian Anthropology during the same time.

The Archaeologist at Work. A Source Book in Archaeological Method and Interpretation. Edited by Robert F. Heizer. New York: Harper & Brothers, 1959. XII, 520 pp. $8.00

This is a book of readings in Archaeology attempting to solve the problem of "too few books for too many students." The carefully chosen selections are for the greater part from books long out of print and cover a wide range of topics. The purpose of the book is to illustrate the work of the Archaeologist: locating, reconstructing, restoring events or material items, age-dating, analysing lithic and metallic sources, etc. All through the book can be seen the two main warp threads: archaeological technique and interpretation of data.

Aspects of Caste in South India, Ceylon and North-West Pakistan. Edited by E.R. Leach. Cambridge Papers in Social Anthropology N° 2. New York and London: Cambridge University Press, 1960. VIII, 148 pp. $3.75. (Published by the MacMillan Company of Canada).

This is a symposium of five essays on caste, each author writing from a personal point of view. However, all the essays are centered on the meaning of Caste. A typical example of a case is first given, then variants of this pattern as they occur in Ceylon and North-West Pakistan. The introduction presents certain generalizations that emerge from this discussion.

Aspects of Primitive Art. Robert REFIELD, Melville J. HERSKOVITS and Gordon F. EKHOLM. New York: Museum of Primitive Art, 1959. 100 pp., 12 plates. $2.75.

This book is the result of three lectures given at the Museum of Primitive Art in New York, by three distinguished and well known Anthropologists. It is a study of Primitive Art: how to look at its works and how best to achieve a sympathetic understanding of them.

Ayvu Rapyta. textos miticos de los Mbyà-Guarani del Guairà. Leon CADOGAN. Sao Paulo: Universidade de Sao Paulo, Faculdade de filosofia, ciencias e letras. Boletin N° 227 — Anthropologia N° 5, 1959. 217 pp.

The author here makes available a collection of mythical and legendary texts, culled from years of contact with the Mbya-Guarani indians of the

Guaira district. The work gives a definite insight into the mysticism of theses aborigines, underlining no few analogies with the great religions of humanity, and indicating the influence of early Christian missionaries.

"Ayvu Rapyta" contains the esoteric religious teachings of the Mbya, which had been jealously guarded from all foreigners for centuries by the tribal priests.

The present compilation of texts is the result of the personal revelations made to Leon Cadogan by members of the Mbya, of which two are still living.

Being a Palauan. H.G. BARNETT. New York: Henry Holt and Company, 1960. VIII, 88 pp. $1.25.

This is an account of Palauan society and culture, and of what it means to live and die in a small village in Palau.

Bunyoro. An African Kingdom. John BEATTIE. New York: Henry Holt and Company, 1960. X, 86 pp. $1.25.

This book may well serve as a general introduction to the social life of the Nyoro, a Bantu-speaking people. It is, in fact, a case study of behavior in a changing society.

The Cheyennes. Indians of the Great Plains. Adamson HOEBEL. New York: Henry Holt and Company, 1960. X, 103 pp. $1.25.

This is a case study in cultural anthropology. It deals with the Cheyennes of the period 1840-60. The author endeavors to show how past and present social history interact to give culture its characteristic cast.

Children of their Fathers. Growing up Among the Ngoni of Nyassaland. Margaret READ. New Haven: Yale University Press, 1960. 176 pp., 29 plates. $4.75.

This book is an anthropological approach to the study of education. It portrays the life in a Ngoni village from birth through adolescence and to maturity. Children are studied in their day-by-day training, this giving us an insight into their family and social life. This study emphasizes how adults bring up their children to fit into their society and to perpetuate their cultural values, and how the Ngoni children learn what the adults teach them.

A Kootenai Grammar. Philip CANESTRELLI, s.j. Santa Clara Cal.: N.H. Downing Press, 1894. 144 pp. Bond and offered for sale in 1959 by the Oregon Province Archives, Crosby Library, Gonzaga University, Spokane, Washington.

The text of Canstrelli's grammar is in latin, as it was designed primarily for Jesuit missionaries who were of many different native tongues. The general arrangement of the grammar is classical, so to say, and follows the parts of speech.

Mélanges géographiques canadiens offerts à Raoul Blanchard. Cahiers de géographie de Québec: Québec (numéro spécial) 3e année, n. 6, avril-septembre 1959. Les presses universitaires Laval, 1959, 494 pp.

Les Mélanges géographiques canadiens sont un recueil d'études de géographiques canadiennes offert à Raoul Blanchard, "qui consacra l'évolution de la géographie québecquoise en intervenant au niveau de la recherche et de l'enseignement supérieur".

Nature and Man's Fate. Garrett HARDIN. New York and Toronto: Rinehart & Company, Inc., 1959. XI, 375 pp. $6.95.

In the very words of the Author: "The primary aim of this book is to bring the reader abreast of the latest thought in evolutionary theory, to show him its implications for the future." Indeed, after sketching the history of evolutionary opinions, the author goes on to demonstrate that a core of biological fact has been brought forth that will give man a full understanding of the rules of life and some rational knowledge of his own fate.

Readings in Anthropology. Vols I and II. Morton H. FRIED. New York: Thomas Y. Crowelle Company, 1959. Vol. I, XII, 482 pp. Vol. II, X, 598 pp.

This work contains 73 articles dealing with the main component fields of modern Anthropology. The first volume presents readings in Physical Anthropology, Linguistics, and Archaeology, while the second volume deals with cultural Anthropology.

Tepoztlàn Village in Mexico. Oscar LEWIS. New York: Henry Holt and Company, 1960. VIII, 104 pp. $1.25.

This book is about life in the Mexican Village of Tepoztlàn as it is lived today. The author also gives an account of the sweeping changes that has come to this typical village.

The Tiwi of North Australia. C.W.M. HART and Arnold R. PILLING. New York: Henry Holt and Company, 1960. XII, 118 pp. $1.25.

This book is a case study of cultural Anthropology. It deals with the system of influence and power based on the Tiwi women. It presents a vivid picture of Tiwi cultural life.

Worker in the Cane. A Puerto Rican Life History. Sydney W. MINTZ. New Haven: Yale University Press, 1960. XIV, 288 pp. $5.00.

This story of a Puerto Rican sugar cane worker and his family constitutes an important social document portraying the rapidly changing patterns of life in rural Puerto Rico.

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