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SOME MOOT PROBLEMS IN SOCIAL ORGANIZATION

By ROBERT H. LOWIE

AXIOMS

TWO recent papers on social organization¹ explain the distribution of social phenomena by diffusion from a single world centre. Conclusions of such import challenge scrutiny as to the methods by which they have been reached. In my judgment they are corollaries of questionable axioms.

According to Professor Olson, unilateral descent is an almost inconceivable anomaly, hence multiple origin is unthinkable: not only have all American clan systems sprung from one source but that source must lie in the Old World to save us "from the awkward plight of positing their special creation within the New World" (pp. 411-14). The axiom is not new, since Lewis H. Morgan (*Ancient Society*, pt. II, ch. XV) considered the clan (gens) an "essentially abstruse" institution and alleged "the improbability of its repeated reproduction in disconnected areas." Those who do not accept the dogma naturally have offered hypotheses to account for clans. Professor Olson chides them for explaining "the esoteric in terms of the prosaic." What does he conceive an explanation to be? Should it reduce the unknown to the unknowable?

There is one attempt to prop the axiom. Unilateral reckoning, it seems, is esoteric because "it contradicts the duality of parenthood and results in an unnatural stressing of one side of the family. . . . Children naturally feel that the parent bestowing clanship is of more importance than the one who is nothing more than a biological accessory." Yet in our own society the father alone bestows "familyship" without reducing the mother to the status of a mere biological accessory. Correspondingly, in primitive matrilineal societies the father is *not* eliminated; he and his kin remain important, as the Crow, Trobriand Island, Banks Island, Hopi data irrefutably demonstrate. Indeed, the avunculate is so frequently balanced by the position of the father's sister that I have suggested the term "amitate" to express her status.

¹ E. M. Loeb, *Die soziale Organisation Indonesiens und Ozeaniens*, A 28: 649-62, 1933.
R. L. Olson, *Clan and Moiety in Native America*, UC-PAAE 33: 351-422, 1933.

Implicitly Dr Loeb holds the same axiom; explicitly he contrasts the approach of the "evolutionist" and the "culture historian." The latter does not ask where or how a trait originated, but merely determines distributions and empirical trait-complexes. To chart these, we learn, automatically yields the direction (*Verbreitungsrichtung*) of the spread of elements (p. 650)—and that apparently is all we ought to seek. Why, we ask, is it taboo to convert an empirical association into an organic one (p. 661)? Does Dr Loeb imagine moieties, clans, avoidance rules, totemism, exogamy, and cross-cousin marriage to have all sprung up simultaneously in a single spot by a divine fiat? If not, some of them are earlier and may have paved the way for others. If so, traits *a* and *b* would be functionally related, and the occurrence of *a* would favor a repetition of *b*.

Culture history favors neither diffusion nor independent evolution. Scientific methodology prefers the diffusionist approach wherever the alternative is to fall back on that vague concept, psychic unity; for diffusion explains a differential similarity while psychic unity would call for universal occurrence. The tables are turned when the observed resemblance is traced to a shared concomitant; for then the purely external factor of a migration is supplanted by an insight into essential relations.

Be this as it may, Dr Loeb errs in asserting that a chart ever reveals anything but spatial range. It certainly does not automatically prove the direction of trait migrations. That must be established by extraneous evidence. Dr Loeb and Professor Olson offer some such evidence; let us examine it.

HISTORICAL CONNECTION AND HISTORICAL CONNECTION

Intercourse between India and Indonesia is a matter of documentary record. Dr Loeb meritoriously adds to the evidence by registering Tamil kinship terms in Sumatra and Mentawai and even in Fiji. This recalls the Tamil bell found in New Zealand in the early days of European settlement.² What, however, does all this prove? Connection, assuredly, if philologists accept the linguistic proof. But *not* what Dr Loeb imagines, viz., that cross-cousin marriage, etc., must also have come from the same source as the terms. Abyssinia shares with Western civilization coffee, firearms, and Christianity. But though the coffee-tree is indigenous in Abyssinia, the Roman Catholic and the Greek Orthodox Church did not evolve from the Monophysite sect, and gunpowder has a history of its own.

The fallacy is a favorite one of Olson's. California and the Pueblos share aspersion, plume offerings, ceremonial pole climbing, etc.; therefore "the

² Elsdon Best, *The Maori as He Was*, 60, 1924.

unilateral complex in California is derived from that of the Pueblo area" (p. 362). What such coincidences can prove is contact, hence the *possibility* of dissemination for other traits in *either* direction or from a common source. The "unilateral complex" is not one of these in my opinion, because it is not a trait at all (see below).

Both authors become unintelligible about cross-cousin marriage. The Western Cree are reported to prefer it; therefore, argues Olson, they probably once had a clan or moiety organization (p. 359). Yet no one has determined the correlation of the two phenomena, and a very high coefficient would be required for such an interpretation. Actually, to take a few random cases, cross-cousins are not permitted to marry by the Maricopa, Crow, and Hidatsa, and the alleged occurrence among the Hopi remains unconfirmed. Considering the fairly wide distribution of Omaha and Crow types of terminology, the matrilateral form of this marriage would involve union with a "mother" or "daughter," respectively, which would be repugnant to many natives. Olson, however, persists in stressing cross-cousin marriage as a common correlate of unilateral groups in every area of the New World. The arbitrariness of its adhesion is offered as evidence of a common origin (p. 412). If the association were as regular as, say, that between shamanism and the tambourine, it would doubtless be significant; since it is sporadic, if not actually rare, it is not.

To turn to Loeb, we learn that the symmetrical form of cross-cousin marriage as found among Australians, Dravidians, and Fijians is presumably the normal and original one (p. 652). This statement, once plausible, has long ceased to be so. The Miwok case is flatly contradictory, some Australians practise asymmetrical forms only, and so do certain African tribes. What is more, Gifford, Rivers, and Kirchhoff—dealing with three distinct areas—all explain the origin of cross-cousin marriage on an asymmetrical basis. This is equally supported by Malinowski's data on the Trobriands and Durlach's on the Tlingit. If the really preferred marriage here is not that with the cousin, but with the man's paternal aunt, for which the father's sister's daughter is merely a substitute, the implication is obvious. I do not consider the matter closed; I am demanding a recognition of well-authenticated facts.

CONCEPTS AND REALITIES

I distinguish between concepts with classificatory utility and historic realities. The term "missile" is not meaningless; it includes the dart of a blowgun, an arrow, a slingshot, etc. But it would be absurd to derive the arrow from the stone of a sling. In my judgment a "unilateral organization"

exists only in our abstracting minds, and its two types—maternal and paternal clans—are not species of one genus, but wholly diverse entities. That is why the suggestion that a clanless tribe could have observed a paternal clan and, borrowing the abstract idea of unilateral descent, created a maternal system, or vice versa, appears not only improbable but preposterous. What can a clanless visitor observe in a society organized into clans? It is as though he were to note the use of a blowgun and forthwith to introduce a bow among his own people. There are no Platonic ideas of clans floating about in savage communities. A paternal clan is an alignment of kin; the observable phenomenon is that children follow the father's group, that ego, his father, father's brother, etc., are grouped together. The visitor can take over this pattern or leave it alone; what sort of counter-suggestion would make him get the idea of never under any circumstances permitting the inclusion of children with their father, of rigorously segregating them from the children of their paternal uncles, and so forth? That would indeed be spontaneous generation with a vengeance; and I can imagine no more "awkward plight" than that of having to assume a repeated transmutation of this kind.

I see no mystical value in the observation made by several writers that maternal and paternal tribes exist everywhere in close contiguity. The logical possibilities are: loose organization, maternal and paternal descent; hence a matrilineal block naturally adjoins a patrilineal one or both a patrilineal and a clanless area.

When, of course, definite clan systems are established, either type may easily absorb features like badges, designations, etc., from the complementary type and such traits may also be adopted from and by groups not based on descent at all. Olson makes a really interesting point in showing that certain polar ideas such as Sky-Earth, Birds-Beasts, Summer-Winter, are linked with moieties. Unfortunately he fails to inquire in how far these pairs are properly equivalents. Still more unfortunately he fails to see that these data, constituting a worth-while problem in themselves, are not closely related to his central theme, the history of "the unilateral exogamous group." The mongrel assemblage of "moieties" assembled under that head (pp. 401-407) neither correspond to that definition nor to any other one subject of discourse. Since the term etymologically means "one half," authors cannot be legally restrained from using it in that generic sense. But of the sixty-odd tribes in Olson's list at best seventeen are exogamous. In other words, the overwhelming majority of the instances are entirely irrelevant to the development of unilateral organizations. Even the Yuchi "moieties" with their endogamous tendency, even the Kickapoo,

Sauk and Fox "moieties" without definite fixity as to descent are grist for Olson's mill!

If we were willing to equate such pairs as Summer-Winter with Sky-Earth; if we granted that the general opposition of "moieties" in games is not a natural accompaniment of a pre-existing cleavage into two social groups; if in short we made the utmost allowances in favor of historical connections, the data would still tell us nothing as to the origin of the wholly diverse "moieties." They would at best prove that certain secondary features had spread widely, attaching themselves to this, that, and the other type of social unit. To infer more would be like arguing that the Republican and Democratic symbols prove the political parties to have originated as noble families with armorial bearings.

Olson's predilection for the "moiety" concept betrays him into scurvy treatment of other units. This applies especially to the non-dual phratry. In the roster of tribes of sibs without moieties, the Yuma, Delaware, and Crow are omitted "because their sibs are grouped into phratries." The gratuitous implication is that the phratries, too, are only masked moieties. Yet the Menomini had three (or five) phratries; the Wyandot, three; and the Iroquois League councils formed a tripartite instead of the otherwise customary dual grouping. It is not a foregone conclusion that the four Tsimshian clans developed by simple splitting; and since Australian sections are constituted on a principle radically different from that of the moiety they can hardly be said to have arisen "by simple division of the moieties" (p. 366).

MOIETY AND SIB

According to Olson, moiety and sib are units of the same category; and the moiety is the older of the two (pp. 403-407). If one defines the sib as a unilateral exogamous group, and the moiety as one of two sibs or major sibs (p. 353), the essential logical unity of the two follows by definition. Also the overwhelming majority of Olson's "moieties" are barred. Whether, however, a particular sib system and a particular moiety system are genetically related cannot be argued in the abstract.

As to the priority of moieties, I am open-minded. Here, too, each case must be examined on its merits, while wholesale settlement will obscure historical problems. Olson's argument is curious. He begins by considering the relative areas of distribution. The comparison "yields interesting but . . . inconclusive evidence." He then counts "moieties" and sibs, separately and jointly, a fruitless count because "moieties" no longer means anything in particular. A comparison of the occurrence of monitor pipes and sibs

would be quite as valuable. But the author himself modestly concludes that the relative frequency "tells little regarding the probable priority of sib or moiety." "More significant," he thinks is the considerable coincidence of sibs and moieties which "argues strongly for multiple and dual groups being part of the same generic complex and diffusing as such." This, if true, would be irrelevant and even contradictory to the thesis: if moieties and clans belong together and diffuse together, the argument in favor of either being prior to the other is nil.

The simplicity of the dual as opposed to a multiple organization is next cited on behalf of its priority as though the logically simpler were necessarily the historically older. However, Olson himself attaches little importance to this, for he passes on to kinship terminologies as being "of greater significance." The point here is that the widespread classification of cross-cousins as a group opposed to parallel cousins fits in with the moiety rather than the multiple clan division. Why is this subject dragged in at all, let alone, credited with significance? It applies at best only to the relatively few exogamous moiety systems of America. Moreover, this significant item suddenly vanishes in thin air when Olson concludes his irrelevancies with the admission that kinship nomenclatures, like the statistical count, and the consideration of distribution "again lead us into conflict and doubt on the question of relative priority."

Having thus admittedly produced no shred of evidence, Olson quaintly ushers in his final paragraph with the statement that "Other arguments favoring the priority of the dual organization are not lacking." What are they, then? Clans are said to subdivide more frequently than they fuse or become extinct. Actually, two cases—the Delaware and the Osage—are cited; they could easily be balanced by contrary instances from the Pueblo area alone. However, this type of argumentation is vicious. Whether segmentation or reduction is more significant cannot be settled by a count; here once more each case demands individual attention. The tortuous history of Assamese units suggests caution. But even if subdivision were more common, how would it demonstrate the primary number of units to be two rather than three or four? Three clans can be subdivided as well as two; the very Delaware cited by Olson have three phratries. Olson contends that the persistence of moieties after the rise of multiple clans is readily explicable. The *persistence* doubtless is; we should like to have some evidence of their prior existence.

Other points might be taken up. Olson (pp. 382–89) has misunderstood Kirchhoff, whose investigation indicates that only a few of the South American tribes examined had unilateral organizations. Olson's moiety

obsession prevents him from properly stressing the virtual absence of the dual organization in Africa (p. 412). And is the evidence for Chinese moieties really so strong (p. 414)? However, I shall confine myself to a single case.

METHODS OF PROOF

Granted Morgan's and Olson's dogma that the unilateral organization is an inconceivably abstruse creation, a single centre follows as a corollary and further debate is unnecessary. Those who reject the axiom naturally demand proof of the alleged historical unity. My treatment in *Primitive Society* of the Northern Plains clan systems as distinct from the Eastern systems involves certain standards of scientific evidence. My discussion is defective in not sufficiently stressing the diversity of matrilineal and patrilineal organization. Irrespective of this, historical connection of a kind—not derivation, say, of the Menomini from the Iroquois system, but transfer of such features as types of clan names—is strongly indicated throughout the vast Eastern area, even embracing the Southern Siouans. The Omaha no less than the Iroquois have totemic associations of clans, sets of personal names owned by clans, a grouping of clans into moieties.

The particular names of clans are certainly arbitrary parts of a system, i.e., they might just as well be one thing as another, hence they are suitable for an investigation of historical connections. Starting with the two Iroquois tribes that lack moieties, the Mohawk and Oneida share a Wolf, Bear, and Turtle trio; and this occurs, along with other names, among the Seneca, Cayuga, Onondaga, and Tuscarora, also among the Wyandot. Passing from the Iroquoian to the Algonkian stock, the trio turns up among the Shawnee, Mohegan, Ojibwa, Menomini; the Potawatomi have Wolf and Bear, as also the Sauk and Fox; the Delaware share Wolf and Turtle. Siouan tribes fall into line: Wolf and Bear occur among the Winnebago, Oto-Missouri, Iowa; Bear and Turtle among the Omaha; all three are found among the Kansas.

If next we take the residual clans of the Seneca, the westernmost member of the League Iroquois, we find Beaver, Deer, Snipe, Heron, Hawk, all of which except Deer occur also among the Ojibwa. The latter have a variety of new clan names, but their distribution is not random: Snake is shared with the Shawnee, Abenaki, Winnebago, Oto-Missouri, Iowa, Ponca; Crane, Bald Eagle, Marten, and Sturgeon with the Menomini. Taking the Winnebago as a starting-point, at least seven of their twelve names—Thunder, Hawk, Eagle, Bear, Wolf, Deer, Elk—are duplicated in the Menomini list, and their Fish may correspond to Menomini Sturgeon or Sunfish. The Menomini, again, share fully seven of the fourteen Kansas

names. In other words, irrespective of linguistic grouping, the tribes from the Atlantic coast to the lower Missouri area not only share the same type of designation, the names themselves largely overlap. There is a continuous chain linking New England tribes with those of Kansas; even Abenaki and Iowa still share Wolf, Bear, Snake, and there is an indefinite series of links in various directions.³ This is proof of historic connection *in the sense defined above*.

But among the Northern Plains tribes the scene shifts with a vengeance. Taking Morgan's rather than my own Mandan list, three Eastern names persist—Wolf, Bear, Eagle; but the only name shared by the Hidatsa is a new one, Prairie-chicken, and that is the only Hidatsa name definitely known to be derived from an animal, one other being untranslatable and given by Morgan as that of an unknown species. The rest are: Hill People (Wide Butte), Water (Red Water), Knife, Lodge (untranslated: maxo'xati), Bonnet (Lower Cap). Among the Crow anything smacking of animal names disappears. My standard list contains not a single sample. One alternate designation for a clan, Pretty-prairiedogs, is obviously also a sobriquet; another translated "Bad Horses" really means "His-horses-are-bad;" Morgan's "Antelope" means (from his own native term) "Antelope-eaters;" his "Skunk" is perhaps to be similarly qualified: and neither for it nor for his "Ravens" have I ever secured an equivalent. In any event, these names are not traceable to the Mandan or Hidatsa, let alone, Eastern tribes. Gros Ventre and Blackfoot names follow the same pattern as the Crow.

Making the most of Morgan's Mandan list, the line of cleavage would put the Mandan with the East, curiously enough separating them from the Hidatsa, with whom their relations have been so close for two hundred years. As for the moieties into which Mandan and Hidatsa clans are grouped, the idea of a dual division of a tribe may quite conceivably have been diffused from the East *without the idea of unilateral descent and without exogamy*, which is lacking here as so frequently in North America. Observing a spatial halving of the tribe, say, in a camp circle could easily suggest similar arrangements to an outsider. The transfer of *that* idea implies nothing as to the diffusion of the clans in the absence of specific evidence to that effect.

Such evidence I consider lacking because (a) the type of clan names is radically different west of the Hidatsa, while no Eastern clan names are duplicated; (b) the clans in the Northern Plains do not own sets of personal names.

³ The above statement is deliberately incomplete, being designed to illustrate method.

In reply Dr Olson points out that nicknames occur among Eastern tribes also, though as secondary designations. He fails to note that most of those quoted preserve totemic references in contrast to such Crow terms as "Those-who-bring-game-home-without-killing," "Tied-in-a-knot," and "Greasy-inside-their-Mouths." He quotes *one* case from the Eastern Dakota on the Mississippi—hardly a typical Plains people—as definite proof that "among some peoples of the Plains nicknames have usurped animal names" in recent times. He conjectures that the secondary nicknames of the East were merely stressed by the Northern Plains Indians. His explanation of why they were stressed is remarkable: these people, it seems, had developed a nickname type of pattern for *all* names. "Tribes, societies, and persons are called after some real or fancied characteristic or event. So are the sibs. . . . It would be surprising if sib designations did not conform" (p. 356). Yet the list of societies from this area bristles with such names as Tobacco, Kit-fox, Raven, Deer, Dog, Buffalo, Mosquito, Skunk, Wild Geese. Indeed, such appellations suggested to Schurtz a possible connection with totemism. As for personal names, a respectable number is derived from the animal kingdom. Buffalo-bird-woman and Gray-bull, Medicine-crow and Horse are certainly not nicknames of the Sore-lip or Greasy-inside-the-mouth variety.

In order to account for the lack of clan ownership of personal names, which he admits to be "conceivably an important difference," Dr Olson suggests that nicknames by their very nature are so variable that sets of them are scarcely expectable. This rests on the misapprehension that a set would necessarily revolve about the same idea, as in the case of, say, Omaha names referring, directly or obliquely, to the Elk. According to Goldenweiser, this does not hold for the Iroquois, where the sets "have nothing whatsoever to do with the eponymous animal."⁴ But even if the statement held true, it would merely explain why supporting evidence of this kind is lacking without adding one shred of positive evidence for the historical unity of Eastern and Northern Plains clan systems.

The one fact which Dr Olson resolutely refuses to face and explain is why, say, the Iroquois and Omaha clan systems, belonging to two wholly distinct stocks and tribes separated by over fifteen hundred miles, are definitely alike in pattern apart from descent, sharing even the specific clan names Deer, Bear, Turtle; while the fellow-Siouan Hidatsa and Omaha, separated by only a few hundred miles, share nothing—neither

⁴ A. A. Goldenweiser, *in* Summary Report of the Geological Survey of Canada, for the calendar year 1912, 470, 1913.

rule of descent nor clan sets of personal names nor type of clan names' nor a single concrete clan name. If the divergences from the Eastern pattern are expectable "peripheral vagaries," why the amazing abruptness with which they set in? For nearly two thousand miles one can go step by step from the Atlantic coast to the mouth of the Missouri and find a gradual progression even in the very names; then comes a complete loss of all resemblance.

But Dr Olson finally does vouchsafe some evidence of similarities that in his opinion outweigh all differences. It is his favorite notion of polar ideas linked with moieties which he finds cropping up on the Upper Missouri. Actually, he has a single instance, the Mandan, where Prairie-Chicken, Eagle, Crow and an untranslatable name are found on one side, opposed to Wolf and two untranslatable names on the other. As shown, such alignment might be diffused apart from the basic unilateral scheme, and besides the evidence for historical connection of the *Mandan* with the East is not denied. Concerning the possible connection of the Hidatsa, Crow, and Blackfoot organization with the East Olson does not present any evidence. Contrary to his vehement statement (p. 360), these differ from the Eastern systems on every point that does not flow from the definition of a clan as a unilateral exogamous unit.

The insistence on moiety polarity fairly clamors for reëxamination of the data. I must here content myself with a partial survey. The Iroquois are credited with the Bird-Animal antithesis (p. 402). Morgan's findings fail to corroborate this assertion. Mohawk and Oneida lack moieties altogether. The Seneca group Snipe, Heron, and Hawk together, but *with the Deer at the head*; and according to tradition the bird clans are subdivisions of an original Deer clan. The Cayuga transfer Snipe to the Bear moiety, Hawk remaining with Deer. The Onondaga have a single bird clan, Snipe, as part of Morgan's Wolf moiety. Among the Tuscarora, the one bird clan, Snipe, is on the side of the two Wolf clans. Interestingly, Great Turtle and Little Turtle appear in *complementary* moieties. Let those who will accept an esoteric grouping of clans on a Bird-Beast basis as a fundamental fact of American social organization.

In the Introduction to his paper Dr Olson tolerantly suggests that differences in the interpretation of unilateral phenomena are legitimate differences of interest and objective rather than of method and validity (p. 351). It seems to me they rest rather on a different conception of proof or on the notion that a mere statement of possibilities may take the place of proof.

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