

UHLENBECK, C. C., *Het Passieve Karakter van het Verbum Transivum of van het Verbum Actionis in Talen van Noord-Amerika* ("The Passive Character of the Transitive Verb or of the Active Verb in Languages of North America"). Reprinted from "Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen, Afdeling *Letterkunde*, 5^e Reeks, Deel II," 187-216. Amsterdam, 1916.

In this highly suggestive and important paper the distinguished Dutch philologist Uhlenbeck undertakes to show that in many American languages (as, for example, also in Basque) the transitive verb or verb of action is not fundamentally active in voice, but rather passive; that the logical subject (from our own point of view) is really a sort of instrumental, or, better, agentive; and that the logical object is grammatically the subject of a passive verb. Thus, in a sentence like I KILLED HIM, the primary idea expressed by the verb-stem is BEING KILLED rather than KILLING: whence it follows that the I is really an agentive (BY ME, THROUGH MY MEDIATION), and that the HIM is best rendered as a subjective HE: HE WAS KILLED BY ME. Uhlenbeck does not assume this interpretation to hold generally for America, but is careful to point out that in a number of American languages (e.g., Klamath and Maidu) we have true active forms. Nevertheless, he looks upon the passive conception of the logically transitive or active verb as belonging to a particularly primitive stage of linguistic evolution. Even where a newer conception has supplanted the old, he sometimes finds reason to believe that the latter may still be traced in survival phenomena. In other words, he believes that the passive verb as fundamental concept belongs to the same group of antique linguistic phenomena as, say, grammatical gender.

I think it would be doing Uhlenbeck no injustice to say that his main interest in

writing the paper was not a strictly philological one, but rather to contribute to ethno-psychologic speculation on the basis of linguistic data. The gist of the paper, together with Uhlenbeck's psychological interpretation of the linguistic facts and the inferences made by him, is given towards the end (pp. 213-215), and it seems advisable to quote from this passage in some detail: "The pronominal elements in conjugation present, as we have already noted more than once, a certain case-value. In the languages with passive conception of the so-called active, or of only the transitive, verb, two case-values are to be clearly distinguished in the pronominal affixes; namely, that of a *casus energeticus* and that of a *casus inertiae*. Each of these two is found in two varieties, according to whether the whole active verb, or only the transitive verb, is passively conceived. The energetic, in other words, may be a transitive case (as, for example, in Basque), in which case it has an intransitive case opposed to it; or, as case of the logical subject in all verbs of action, it may be an active case (as, for example, in Dakota), in which instance it may be contrasted with an inactive case. It is easy to discover the nature of the *casus inertiae*, whether intransitive or inactive. It is the case of him who or that which is, or gets to be, in a certain state, aside from his (or its) own will and without his (or its) own participation, whether under the influence of a stronger person or thing or as if it were of himself (or itself). But what is the essential nature of the energetic case? It is a case of instrumental-like character, but nevertheless to be clearly distinguished from an ordinary instrumental. One might call the energetic the case of the primary instrument; the ordinary instrumental, that of the secondary instrument. For the primitive linguistic feeling, the real agent is a hidden power. It acts *via* the apparent agent, the primary instrument, which again can itself make use of a secondary tool. Take, for example, a

sentence like HE KILLS THE BIRD WITH A STONE. A Blackfoot would express this in the following manner: THE BIRD BY-MEANS-OF-IS-KILLED-BY-HIM A STONE. He who kills is what is generally called the 'agent,' but in truth is only the apparent agent, the primary instrument, which is itself controlled by a hidden power. The apparent agent, although itself dependent, works on the logical object (i.e., the grammatical subject) by its own emanating *orenda*; and even when it is the logical subject of an intransitive action,—which is often the case in the mentality of peoples that recognize the contrast, not of transitive and intransitive, but of active and inactive,—it works similarly by virtue of the same outstreaming mystic power. Therefore the energetic case, the exclusively transitive as well as the general active, can be called *casus emanativus* or 'case of outstreaming power.' When it is an active case, it can be more closely defined as the 'case of operative power;' when it is a transitive, as the case of power that operates on something else."

For us the main point of value in the paper is the fact that Uhlenbeck has striven to explain three distinct linguistic phenomena, each of which had been abundantly recognized as such, as symptomatic of one fundamental feature,—the passivity of the so-called transitive and active verbs. These phenomena are the close morphological resemblance in certain languages between normal passive forms and at least certain transitive forms; the classification of verb-stems on the basis of singularity or plurality, according to the number of the intransitive subject and transitive object; and the frequent classification of pronominal elements into two groups that do not correspond to our normal subjective and objective (i.e., either into intransitive subject and transitive object *versus* transitive subject, or into inactive subject and transitive object *versus* active subject). A few remarks on each of these points.

Uhlenbeck's data for the first class of evidence are taken from Algonkin alone (Ojibwa and Blackfoot; Michelson's corroborative evidence for Fox is also referred to). For certain Algonkin verb-forms there can, indeed, be no doubt that Uhlenbeck's findings are correct; but frankly I do not see that he has succeeded in showing that the Algonkin transitive as a whole needs to be interpreted as a passive. I would tend rather to feel that certain true passives had been dragged for purely paradigmatic reasons into transitive company; e.g., Jones's Fox form for HE—ME is evidently identical with his I as passive subject, and has morphologically nothing to do with such true pronominally compound transitive forms as THOU—ME. That the passive is unrelated to the true transitive in Fox, seems to me to be strongly suggested by the occurrence of two morphologically very distinct forms for the combination of two third persons,—a true transitive (e.g., HE SEES HIM), and a passive of the same structure as the HE—ME and similar forms already instanced (this passive occurs in two distinct forms,—an agentive, HE IS SEEN BY HIM; and a non-agentive, HE IS SEEN indefinitely). However, there no doubt are languages whose whole transitive is morphologically a true passive. This is notably the case with Yana, in which such a form as HE SEES ME is quite evidently to be interpreted as meaning properly I AM SEEN BY HIM; THOU SEEST ME, as I AM SEEN (BY THEE is merely implied); I SEE THEE, as THOU ART SEEN (BY ME is merely implied); and so on. Yet even where there is a close morphological resemblance between transitives and passives, it does not always follow that the transitives are of passive origin. Thus, in Takelma such a form as HE SEES ME is closely related to I AM SEEN, but is not derived from it. On the contrary, the passive is formed from the transitive by means of a suffix which differs for various tense-modes. Hence it seems plausible to interpret it as a sort of impersonal, though there is a true impersonal (with or without object) in

Takelma, besides. At any rate, the pronominal object of the transitive cannot in Takelma well be interpreted as the subject of a passive, for the simple reason that it shows no resemblance to the intransitive subject, which differs in turn from the transitive subject. This and other examples that might be adduced show conclusively that evidence of the relation between passive and transitive forms cannot without further ado be used to demonstrate the passive origin of the transitive. Morphological evidence for such an origin undoubtedly exists in some cases, but hardly so abundantly as to establish the general validity of Uhlenbeck's main thesis.

That in those American languages that distinguish singular and plural verb-stems the determining factor is not altogether the number of the subject, but, where the verb is transitive, the number of the object, is well known to Americanists. Uhlenbeck quotes examples from Athapascan, Haida, Tsimshian, Chinook, Coos, and Pomo. Naturally there are many other languages that present the same feature. Uhlenbeck considers it as a reflex of the primarily passive nature of the transitive verb; the logical object of an action being psychologically, and in many cases grammatically, the subject of the passive form of the action, and hence directly comparable to the subject of an intransitive verb. A rapid survey of American languages classifying verb-stems in the manner described soon discloses the fact, however, that there is no clear correlation between this feature and the classification of pronominal affixes into transitive *versus* intransitive, or into active *versus* inactive, as contrasted with subjective *versus* objective. Thus, while Haida classifies its pronominal elements into active and inactive (to use Uhlenbeck's terminology), and Tsimshian and Chinook into transitive and intransitive, there are not a few languages of subjective *versus* objective pronominal classification that recognize precisely the same feature of number-classification of

verbs as these languages. Shoshonean, for example, is a group of languages (I speak chiefly for Southern Paiute) that rigidly classifies its pronouns into subjective and objective; yet it makes an unusually liberal use of verb-stems that are distinct for singular and plural, singularity or plurality of the transitive verb being, as usual, determined by the object. One way out of the difficulty is to assume, as Uhlenbeck is evidently inclined to do, that in such languages as Shoshonean and Klamath the present classification of pronominal elements is a secondary feature, and that the numerical classification of verb-stems reflects an older status of pronominal classification. As I see no warrant for such an inference, I prefer to doubt seriously whether the two features are causally related. On general psychological principles, it seems likely enough that transitive activities are necessarily more closely connected in experience with the object than with the subject. A passive interpretation of the transitive is hardly necessary. I would suggest, however, that the link between the subjectively determined intransitive and the objectively determined transitive verbs lies in the causative origin of many transitives. If TO KILL is really in origin TO CAUSE TO DIE, then the difference between ONE MAN DYING and SEVERAL DYING would necessarily have to be reflected in a difference between CAUSING ONE MAN TO DIE, KILLING ONE MAN, and CAUSING SEVERAL TO DIE, KILLING SEVERAL. And, indeed, a survey of transitive verb-stems that recognize a distinction of number shows that they consist chiefly, if not entirely, of such as can be, in part even morphologically, explained as causative derivatives of intransitives. If such causatives be taken as a starting-point for number-discrimination in the object, other types of transitive with number-discrimination, if such exist, might be explained as due to analogy.

The greater part of Uhlenbeck's paper is taken up with his third class of evidence, the

classification of pronominal affixes. The Basque forms (intransitive subject and transitive object *versus* transitive subject) are taken as his starting-point, and attention is called to parallels in Eskimo and, hypothetically, an inferred stage in Indogermanic. The Indian forms are quoted from Tlingit, Haida, Tsimshian, Chinook, Muskhogean, and Siouan. Riggs's Dakota evidence, in particular, is presented in great detail; the conclusion arrived at being that all active verbs are passives in nature, the logical subject being really an agentive. Comparison with other Siouan dialects (Hidatsa, Ponca, Winnebago, Tutelo) shows the pronominal peculiarities of Dakota to be general to Siouan; the Catawba evidence throws no light on the subject (I cannot refrain, in passing, from remarking that there is no bit of American Indian linguistic research that more urgently needs doing than the preparation in the field of a Catawba grammar; Gatschet's sketch is worthless). It follows clearly enough from Uhlenbeck's evidence, which could no doubt be greatly augmented, that the ordinary contrast between subject and object does not hold in these languages; but I do not see that the interpretation of the transitive or active verb as a passive is a necessary one. At least two other possibilities seem open. Uhlenbeck's *casus inertiae* may be an intrinsically caseless form which takes on all functions not specifically covered by the transitive or active case (subject of transitive or active verb); in other words, the I of I SLEEP, and the ME of HE KILLS ME may be identical in form, not because of any identity of verb-morphology, but merely by way of contrast to the distinctively transitive form of the I of I KILL HIM. This explanation would probably imply a previous stage of complete lack of pronominal differentiation. Secondly, instead of interpreting the object of the transitive verb as a sort of subjective (in other words, deriving it from the intransitive or inactive

case), one may, on the contrary, look upon the latter as an objective, the inactive or intransitive verb being interpreted as a static verb without expressed subject, but with direct or indirect object. Thus, forms like I SLEEP or I THINK could be understood as meaning properly IT SLEEPS ME, IT SEEMS TO ME (cf. such German forms as *mich hungert*). Personally, I consider the latter explanation as very likely for those languages that, like Tlingit, Haida, Muskhogean, and Siouan, distinguish between active and inactive verbs. On the other hand, it seems considerably more far-fetched in the case of languages that distinguish between transitive and intransitive verbs (I RUN, for example, as IT RUNS TO ME). This brings me to what I consider the greatest weakness of Uhlenbeck's paper,—the inclusion under one rubric of transitive *versus* intransitive, and active *versus* inactive. I believe he would have made a more convincing case if he had confined himself to the former category, and adopted our second suggestion for the latter. In brief, the transitive verb *may* be plausibly interpreted as a passive, though this hardly seems necessary to me where there is not direct morphologic evidence of the kind that Uhlenbeck has produced for certain Algonkin forms; the active verb is far more plausibly otherwise interpreted.

To Uhlenbeck's speculations as to the primitiveness of the passive verb I am not inclined to attach much importance. Such questions must be attacked morphologically and historically, not ethno-psychologically. As long as we are not better informed as to the exact distribution of types of pronominal classification and as to the historical drifts inferred from comparative linguistic research, it is premature to talk of certain features as primitive, of others as secondary. For the present, I should like to point out that we know of at least five, fundamentally probably only three, types of pronominal classification

in America, as indicated in the following table:—

	Obj. tr.	Subj. intr.		Subj. tr.	Example.
		Inactive.	Active.		
1. . .	A		A	B	Chinook
2. . .	A	A	B	B	Dakota
3. . .	A		B	C	Takelma
4. . .	A		B	B	Paiute
5. . .	A (sometimes subj. of passive)		A	A	Yana

Identity of letter symbolizes identity of pronominal form. Type 4 is probably either simplified from type 3 or else represents an earlier stage of it; both developments may well have taken place. Type 5 is no doubt a specialized simplification of type 4. What the historical relations between types 1 and 2 and between each of these and types 3–5 are, it is impossible to tell at present, though there is at least some evidence to show that type 4 tends to develop from type 2. The interpretation of the nature of the verb in each of these types is not always easy. The passive interpretation of the transitive may apply in certain cases of types 1 and 5.

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UHLENBECK, C. C., *Het Identificeerend Karakter der Possessieve Flexie in Talen van Noord-Amerika* ("The Identifying Character of the Possessive Inflection in Languages of North America"). Reprinted from "Verslagen en Mededeelingen der Koninklijke Akademie van Wetenschappen, Afdeling *Letterkunde*, 5^e Reeks, Deel II," 345–371. Amsterdam, 1916.

Uhlenbeck calls renewed attention in this paper to the well-known fact that in many American languages the possessive pronouns, generally affixed to the noun, occur in two more or less morphologically distinct series,—one for nouns possession of which is of an inseparable nature, the other for nouns

denoting separable possession. The former category includes chiefly terms of relationship and nouns denoting parts of the body. A careful survey of the evidence presented by Uhlenbeck shows, that, though body-part nouns and terms of relationship are not infrequently classed together in contrast to separable nouns, there are sometimes special morphological features that distinguish the two types of inseparable nouns; further, that in certain languages only the terms of relationship constitute a special class as regards possessive affixes. Languages distinguishing separable and inseparable possession as such are Tlingit, Haida, Tsimshian, Chimariko, Muskhogean, and Siouan. As a rule, however, the two pronominal series are not fundamentally distinct, but are morphologically related; in Tlingit, Tsimshian, and Siouan, the separability of the noun is indicated by an affixed element, while only in Chimariko are the possessive elements of the two series radically distinct. Moreover, in both Haida and Siouan the terms of relationship are not treated in quite the same manner as the body-part nouns. In Algonkin, of which he treats Blackfoot in particularly great detail, Uhlenbeck finds that, while there is no rigid classification of possessed nouns into separable and inseparable, a suffixed *-m-* is used with great frequency to indicate the separability of the noun.

The relative independence of terms of relationship as a class, suggested by Haida and Siouan, is still further emphasized by Takelma, in which such nouns have a peculiar set of possessive affixes as distinct from all other nouns, including such as refer to parts of the body; further by Yuki and Pomo, in which only terms of relationship have possessive pronominal affixes. In Mutsun (Costanoan), moreover, where there is, properly speaking, no possessive inflection, terms of relationship have different endings, according to the person of the possessor. Such examples strongly suggest that alongside of, or inter-