

**ETHNOLOGICAL AND LINGUISTICAL  
ASPECTS OF  
THE URAL-ALTAIC HYPOTHESIS**

BY S. M. SHIROKOGOROFF

(Reprinted from Tsing Hua Journal, Vol. 6)

1970

ANTHROPOLOGICAL PUBLICATIONS  
Oosterhout N.B. - The Netherlands

Photomechanic reprint after the edition of Peiping, China, 1931.

Exclusive distributor in the U.S.A., its possessions and territories, and Canada and Mexico:

HUMANITIES PRESS Inc.  
303 Park Avenue South  
New York, N.Y. 10010

## ETHNOLOGICAL AND LINGUISTICAL ASPECTS OF THE URAL-ALTAIC HYPOTHESIS

BY S. M. SHIROKOGOROFF

### TABLE OF CONTENTS

INTRODUCTION: I. Preliminary Remarks

#### PART I

CHAPTER I. ETHNOS AND CULTURAL PHENOMENA: (2) The Human Unit; (3) Definition of "Ethnos"; (4) Process of Variations in Ethnoses; (5) Centrifugal and Centripetal Movements in Ethnoses and Formation of New Ethnoses; (6) Cultural Elements and Complexes; (7) Cultural Complexes as Functional Phenomenon; (8) Variations of Cultural Phenomena; (9) Impulses of Variations, Their Intensity and Tempo of Variation

CHAPTER II. LANGUAGE: (10) Language; (11) Sounding Starters; (12) The Language in Ethnoses; (13) Formation and Transmission of Linguistic Complexes; (14) Classification of Languages; (15) Centripetal and Centrifugal Movements in Languages; (16) Language as Ethnographical Phenomenon in the Process of Ethnical Variations; (17) The Problem of Common Words; (18) Particular Cases of Common Words; (19) Correlation Between Ethnoses and Languages

CHAPTER III. THEORY OF EVOLUTION AND LANGUAGE: (20) Theory of Evolution and Languages; (21) Logical Consequences of the Theory of Evolution Applied to Languages; (22) Phonetic and Morphological Consequences Implied by the Theory of Evolution; (23) Conception of "Progress"; (24) Element of Time; (25) Conception of "Primitive" Language; (26) Authors' Attitude and Theory of Evolution; (27) Linguistics as One of the Elements of the European Ethnographical Complex



## PART II

## URAL-ALTAIC HYPOTHESIS

## CHAPTER IV. URAL-ALTAIC HYPOTHESIS AND TUNGUS MATERIAL:

(28) The Theoretical Background of the Ural-Altaic Hypothesis; (29) Consequences Resulting from the Fundamental Propositions; (30) Tungus Comparative Material; (31) Aspiration and Bilabialization of the Initial Vowels a Phonetic Fashion; (32) The Goldi Language Not a Typical Northern Tungus, Neither a Southern Tungus; (33) Mongol Aspiration and Bilabialization; (34) Linguistic Importance of a Correct Solution of the Problem of Aspiration and Bilabialization

CHAPTER V. ANALYSIS OF PARALLELS: (35) The Scope of the Present Analysis; (36) Examples of Cases Rejected Without a Detailed Analysis; (37) Examples of Cases Rejected After a Minute Analysis; (38) Cases with Initial and Intervocalic Labial Consonants; (39) Cases with Initial and Intervocalic Dental Consonants; (40) Cases with Initial and Intervocalic Glottal Consonants; (41) Cases with the Initial and Intervocalic  $\eta$ ; (42) Conclusions

## CHAPTER VI. METHODS: (43) Influence of Theoretical Presumptions of Evolution; (44) Phonetic Laws and Their Application; (45) Restoration of Stems and Difficulties in Operating Them; (46) Semantic Parallelism and Its Application; (47) Operation with the Morphological Elements and the Using of Foreign Words; (48) Summary as to the Methods Used; (49) The Origin of Methodological Peculiarities Met with in the Discussion of the Ural-Altaic Hypothesis

## CONCLUSION: (50) The Problem of Common Words; (51) The Tungus Language and the Ural-Altaic Hypothesis

## INTRODUCTION

## 1. Preliminary Remarks

Some three years ago I published a paper<sup>1</sup> devoted to a complex linguistic and ethnographic problem—the terms of orientation amongst the Tungus. This paper has illustrated my idea that the problem of common words in the so-called Altaic languages cannot be settled before we have a complete analysis of the complexes compared, both from the linguistic and the ethnographical point of view. My attitude was not in agreement with the current opinions as to this question—the paper was actually directed against the practice of the uncritical using of Tungus parallels with the intention of including the Tungus languages into the larger unit of the Altaic languages supposed to have originated from an ancestor-language. My publication has been noticed by P. Pelliot,<sup>2</sup> who was particularly concerned in the treatment of the Tungus terms of orientation, for he had previously tried to bring forth some Tungus parallels with a quite definite intention of supporting the hypothesis of the Altaic pra-language;<sup>3</sup> so, referring to my paper, he says: “De bonnes remarques, et d’autres contestables; p. 184, n. 45, M. Sh. veut que M. Rudnev n’ait pas commis une inadvertance que j’ai signalée incidemment; toute l’anthropologie ne peut prévaloir contre M. Rudnev lui-même qui m’a écrit autrefois qu’il avait bien commis cette inadvertance.” This note formally contains nothing offensive (the discovery of the special system of orientation amongst the Tungus as well as the analysis of terms is a minor thing, the mentioning of which can be omitted), but in the above quotation one may see that the emphasis is put on my suggestion as to the origin of a minor half-mistake made by A. D. Rudnev. Indeed, the latter could not formally deny it. P. Pelliot has made of it a case which has at the same time screened the fundamental problem discussed in my paper. Yet, in his remark, he has mentioned *anthropologie*, which is not so simple as that. I do not need to

<sup>1</sup>“Northern Tungus Terms of Orientation,” in *Rocznik Orientalistyczny*, Vol. IV, Lwow, 1928.

<sup>2</sup>*T'oung Pao*, Vol. XXVI, 1929, pp. 411, 412.

<sup>3</sup>“Les Mots à h initiale aujourd’hui amule dans le mongol des XIII<sup>e</sup> et XIV<sup>e</sup> siècles,” in *Journal Asiatique*, avril-juin, 1925.



explain that *anthropologie* has nothing to do with the case, for I have used an *ethnographical* explanation of A. D. Rudnev's half-mistake and not an anthropological one, so that there is some other purpose in mentioning *anthropologie*. This is a veiled form of protest against using neighbouring sciences for the analysis of phenomena which are supposed to be reserved for linguistics.<sup>1</sup> As an attitude it is rather characteristic, so I shall dwell on it a little longer. Who may say that any knowledge, be it anthropology or any other science, may not be useful for linguistical work? For instance, L. de Saussure's work on Chinese astronomy ("Les Origines de l'astronomie Chinoise") was possible only on condition of this author's familiarity with astronomy. Then may not anthropology be helpful too? I think that any knowledge, even without a direct bearing on the subject, is useful. It is particularly true of anthropology, familiarity with which greatly helps in avoiding a heated discussion about the origin of the "Chinese," the "Altaics," the "Aryans," and other linguistical groups directly concerned and even sometimes discussed by linguists without their being familiar with anthropology. Indeed, there is a serious objection as to including anthropology into the group of sciences with which the linguist must be familiar; namely, the difficulty of mastering two seemingly distinct fields. But this question is technical, and shall I say personal, so it has no theoretical interest.

I have dwelt on this topic, for in the present work I will occasionally refer to anthropology, and in the general setting of the problems I shall base myself on their ethnological aspects. I cannot shut my eyes to the fact that linguistical phenomena have also their ethnographical features, and the Ural-Altaic hypothesis in its nature is an ethnological problem. I think, therefore, that anthropology, ethnography, and especially ethnology, may be very helpful, and for this reason the chief subject of the present paper—"The Ural-Altaic Hypothesis"—is preceded by a short discussion of the general problem of ethnos and language. As a matter of fact, it has been necessary, for the problem of ethnos and language has not yet been treated, so references to previous publications could not be

<sup>1</sup>The mentioning of anthropology may also be interpreted as an intention of discrediting my linguistical evidences, for the anthropological investigations constitute one of the items of my work. Indeed, it would not be a fair way of discrediting them, so for this reason I reject this supposition.

made. However, without such a preliminary introduction, the treatment of the Ural-Altaic hypothesis would even be impossible. On the other hand, I did not want to publish the general and special parts separately, for the former had been written *ad hoc* and could not be extensive enough to become an exhaustive treatment of the problem of ethnos and language—to this problem I shall return later in my other publications.

Since the problem here treated is approached from various points of view, I shall often refer to the facts and conclusions which have already become commonplace, almost truisms, for the specialists. However, they are not so for those who confine their work and interests to the restricted fields of scientific inquiry, and therefore they need to be pointed out. Yet I shall need them as a ground for approaching my chief goal. Besides the well-known facts and conclusions, I shall refer to various theories which have not yet been generally accepted; yet I shall offer some new facts and theories which have never been published. In this respect, the present work does not pretend to be uniform.

My point of view as to the methods and possibilities of discussing the problem of the common origin of the so-called Ural-Altaic languages in many a respect is different from that of other authors. The difference does not consist in the discussion of evidences proving or disproving a common origin, but the difference is due to my idea; namely, that the "origin" and "relationship" between the languages in this way cannot be discussed at all. This point of view ought to be supported by theoretical reasons, which have occupied a large portion of Part I. Another point to make clear is that of finding how such a difference in attitude exists, for without showing the process of formation of the opposite attitude my point of view might appear to be a mere "point of view," as good as any other, while the difference is much deeper than this. This has necessitated other sections in which the theory or evolution and its consequences are shortly discussed.

The difference in method and the original point of procedure for the investigation requires a revision of terms used. L. K. Ogden and I. A. Richards have shown in their work ("The Meaning of Meaning. A Study of Influence of Language upon Thought and of the Science of Symbolism," London, 1923) how important symbolism is if one wants to be understood. However, the task



of revision of terms requires more than a mere substitution of the old meanings by new ones, but a "symbolization" of old symbols, for some of the "symbols" cannot be used any more. Naturally, I cannot undertake this task in reference to a foreign language, such as English is to me. I have, however, permitted myself to introduce a few new terms: such as "ethnos," which has already been used; and "starter," which is here explained; and some other old terms to which I give a slightly different "meaning." I deem it necessary to make these remarks in order to avoid a useless and tiresome "discussion about the words."

In the treatment of the problem of ethnos and language, I shall follow the shortest way of expounding some salient points of my theory. Their complete treatment would require a much greater space than may be allowed in discussing such a particular problem as the Ural-Altaic hypothesis. So in many a case I shall confine myself only to the conclusions, omitting altogether the method of my investigation and the enumeration of facts. I must here point out that I, as well as many other authors, notice that some critics make an elementary error; namely, in their mind every work which is not supplied with "facts" is classed as a "speculative" one. It is a good word, indeed, but one must use it with a certain amount of caution and with correct reference. They refer, as a matter of fact, not to the essentials of the method, but to the technique of writing. For a genuine, speculative work, facts are not much needed, and they may be lacking altogether in the written form. However, when the current fashion requires "facts," a speculative work may be supplied with facts in any desirable quantity,—in fact, volumes of facts,—but the genuine speculative character as to method will remain as it is.<sup>1</sup> On the other hand, the bringing up of well-known facts from other publications may sometimes be absolutely unnecessary for the expounding of a new theory or hypothesis built upon these facts and based upon a purely inductive method. Such a work will not be overburdened by the repetition of facts already reproduced hundreds of times by those who want to avoid the reproach of speculation. In the present work (Part I),

<sup>1</sup>In the field of ethnography and linguistics, there are hundreds of publications in which facts occupy more space than the simple fundamental speculative proposition, but the critics do not notice the mosaic work of the authors in using facts as a shield to protect themselves from the critics.

I do not cite facts in all the cases discussed, but this does not at all mean that my conclusions are not based upon facts, and that they are merely "speculative." All of them, with the exception of specially pointed-out cases, are based upon facts, and at any moment the technique of investigation may be shown and the conclusions illustrated. However, in most cases this seems to be useless, for the facts are already known. It is different with Part II, where I am dealing with the analysis of facts and bring up new facts. Here one has a case in which the difference of method is evident. The work of A. Sauvageot discussed here is based upon and inspired by a series of hypotheses, and only seemingly based upon facts, while the facts are actually used for supporting the hypotheses. My attitude is to have reliable facts first, so, for the time being, I am not worrying about the problem whether the pra-Ural-Altaic language existed or not.



## PART I

## CHAPTER I. ETHNOS AND CULTURAL PHENOMENA

## 2. The Human Unit

Any investigator into the problem of variations of cultural and physical phenomena in man will sooner or later come to the idea that these variations take place in certain human groups which exist as units. Several attempts have been made at finding such a unit. It was supposed, for instance, that these processes took place in "races," in "nations," in small or large political units, or in other groups. Yet, since the units suggested could not be accepted as concrete ones in which these processes are proceeding, it was suggested to regard them as going on in "mankind." However, all the processes under this investigation are concrete processes, while "mankind" is an abstraction which does not help at all in understanding the mechanism of variations. The approach to this problem is greatly handicapped by the fact that the units in which the processes are going on may not be formally marked by any signs of distinction and very often they are not recognized as existing and functioning units. Yet the latter are not static phenomena; they are themselves rather processes than sharply outlined units. However, the finding of such static units is required by statically behaving minds. On the other hand, great specialization amongst the investigators, or better, great limitations of interests and competence practised by the investigators, are partly responsible for omitting the facts which distinguish one group from another. A great misfortune for the students of cultural phenomena is also reasoning by analogy with biological investigations. Since in biology the investigation into the variations (evolution) of certain physical organs is one of usual and quite successful methods for tracing the sequence of animal forms, it was postulated that the same could be done with cultural phenomena and even with isolated cultural elements. This trend of ideas existed side by side with another movement which was always trying various ways in order to find the correct solution of the problem of the human unit.

The importance of cultural phenomena, in the classification of human groups, was realized long ago. Linneus introduced into

his classification of human "races" and "species," i. e., purely zoölogical conceptions, the cultural element side by side with psychonental distinctions. This trend of ideas led to a classification which was very successful, chiefly owing to its simplicity; namely, the famous linguistico-anthropological classification of Fr. Müller. Of course, in this form it was a pure absurdity, chiefly due to badly digested biological ideas and a series of erroneous postulates. We now know that such an approach to the problem is not correct, for the "races" do not exist as units and the cultural phenomena (including language) are not confined to such units. However, the idea of including cultural phenomena into the complex of "biological" phenomena and as an essential characteristic of human groups is quite correct. In fact, the essential element of adaptation of human groups (and individuals) is the cultural phenomena transmitted by tradition, which itself is a function of continuing units. The need of quick adaptation of human groups (and individuals) stimulates a further increase of the importance of cultural phenomena: *the faster the tempo of variations in human groups, the more important are cultural phenomena as a method of adaptation.* In fact, the adaptation of man to various climatic conditions is not only going on along the line of physical change, as is usual with the animals which do not practise cultural adaptation very much, but it is going on along the line of cultural invention. This is true not only of adaptation to climatic conditions, but it also holds good for the adaptation to the primary milieu in general, and particularly when the increase of population implies an increase of food supply by means of the technical perfection of food production, etc. It is also true of the adaptation of social organization, which must be well adjusted to the needs of economic activity. It is thus evident that cultural phenomena are nothing but a new and specific form of biological phenomenon of adaptation which is affecting chiefly the function, and perhaps the matter, of the brain in the process of adaptation, at the present time leaving only secondary importance to bodily changes. By these remarks it is not to be presumed that no physical change is involved in the process of human adaptation. In my work on the process of growth I have shown that the cultural adaptation resulted in the extension of the period of growth which involved a series of other physical changes, the latter being possible only as the condition of complex biological variations, including perhaps a change of chemical equilibrium.



So cultural adaptation is not so simple as it is sometimes pictured, and it does not go without affecting the physical features, especially the chemical functioning of the organism. All cultural, morphological, chemical, and psycho-mental variations are closely correlated and bound together, which points still more strongly to the "biological" character of cultural adaptation.

I shall not repeat here the history of the modifications of the idea of physical units in man as "races," "species," and "types,"—a question of great complexity,—for already I have recently dealt with it. I shall point out my conclusion; namely, that the process of physical variations in man is proceeding in units which are differentiated owing to the distinction of cultural complexes, at present the chief method of human adaptation. With such an approach to the problem the units of mankind long ago appeared as specific ones, but the nature of these units could not be defined at once. In fact, as early as the beginning of the nineteenth century, a term for the designation of such a unit was introduced. The term was not one similar to such current terms as "race," "nation," etc., but it was one which was borrowed from the Greek word *ethnos*, introduced into the compound terms of "ethnology" and "ethnography." In the hands of specialists who had confined their interests to cultural phenomena, particularly languages, or to physical phenomena, these terms have, however, been misused and even "translated" into different languages until their original meaning has escaped the attention of specialists, and as the conception of unit was not clearly formulated, they have almost been forgotten altogether.<sup>1</sup>

<sup>1</sup>In order to avoid a discussion concerning "words," I want to point out that the sciences dealing with various aspects of man and human beings must be distinguished on the principle of practical utility. I shall use "anthropology" as a branch of zoölogy; "ethnography" as a description of ethnical units chiefly from the cultural point of view; and "ethnology" as a science dealing with the general principles of processes observed in man whose unit, where the process of variation occurs, is "ethnos." Since cultural adaptation is only a particular form of human adaptation,—a purely biological phenomenon,—the science of ethnology is indeed one of the branches of general biology which is regarded as the science dealing with the principles of variations of "living" matter in its various forms. The place of archaeology, pre-history, history, also linguistics, etc., is evident. The controversy as to the terms is not yet settled. Recently, P. Rivet ("L'Anthropologie," in *Scientia*, Vol. XLVIII, 1930) has again discussed this question advocating the preservation of *anthropologie* for

The revival of the term is, however, badly needed. I made this attempt in published form nearly ten years ago, and at about the same time, the same or nearly the same idea arose from other quarters. Since the present work deals with the problem of language in further treatment of the problem, I must here detain myself on some aspects of human units.

### 8. Definition of "Ethnos"

I have defined a human unit as a group of individuals who are united by the same languages, origin, or, better, who have the same belief as to their common origin, and who possess a certain cultural complex distinct from that of other similar groups; such a group of people preferentially marry among themselves—they are preferentially endogamous. Such a unit may be numerous, often attaining several millions, and it may be very small, containing a few hundred individuals—the number has no great importance as a distinct character of unit. The process of variation of cultural phenomena is going on in these units, as well as the process of variations of all biological phenomena. So the most remarkable feature of ethnos is that it is a biological unit of man in which the reproduction of species is going on and in which the process of physical changes takes place. The line of demarcation between two units is not strictly defined, for there are always two forces at work; namely, those consolidating the units, and those differentiating the units into smaller ones. Since this aspect of the existence of ethnos has a special importance in the problem of languages, I shall return to it later on (*vide* Section 5). As in any other biological unit, e.g., in the regional varieties of other animals, the ethnos may happen to be in a state of growth, in a state of numerical increase, in a stationary

the science here designated as "ethnology" and reserving "*anthropologie S. S.*" (somatic) for "anthropology." I prefer the term "ethnology," for this science deals chiefly with "ethnoses," while "anthropology" deals with "anthropes." Yet it seems to me that, historically, "ethnography" and "ethnology" are just as good as "anthropology," and the scientific trend points to the need of a term with a constant form, such as "anthropology." This deviation of sense, as P. Rivet (*op. cit.*, p. 89) says, has deep reasons; namely, the accumulation of new facts and the specialization of methods. As a matter of fact, one may be a good anthropologist without being an ethnographer, and one may be a good ethnographer without being an anthropologist. Yet both of them may, as is actually observed amongst specialists, remain so without becoming ethnologists. Of course, ethnology cannot be identified with ethnography, nor with anthropology.



condition, or in a state of decline. As to potential activity, the ethnos may be strong or weak. Naturally, when the ethnos is in a state of growth, it may be suspected of being "strong."

Since the ethnos is a universal phenomenon, there must be certain reasons<sup>1</sup> which underlie its existence; but I shall not now go into these details and shall confine myself to pointing out how the ethnoses may be formed. Two conditions are of importance; namely, the primary milieu and the interethnic pressure, which will be discussed later on. In fact, the similarity of ecological conditions—topography, climate, vegetation, and animals—in regions where man is living is usually confined to very limited areas. Within these areas, the best adapted unit is the one which lives the longer and becomes familiar with the local conditions. The point of importance is that the unit transmits its experience of adaptation to the local conditions through tradition, and its physical adaptation through the complex mechanism of inheritance, accumulating, in this way, the work of previous generations. Another condition—varied interethnic milieu—is not alike around the world, so every unit is surrounded by slightly different interethnic milieus, and thus the sources of influence are different.

Although the ethnos as a unit may be a concrete physical phenomenon, consisting of physical individuals, where the processes are going on it is not a static phenomenon, and thus it cannot be expressed in static terms. It must not be understood as a "tendency," for this point of view will appear defective at once when we touch the problem of physical variations, which must be located physically. The conception of this phenomenon must be as one of a process covering more or less numerous units and is thus dynamic and not static.

Referring to the preceding remarks regarding attempts at the finding of a new term for the human unit, it ought to be pointed out that, in so far as I know, two authors have already approached this question so closely that they have used the Greek word *ethnos* for the new conception of the human unit. However, there are some essential differences between their and my point of view. F. Regnault has proposed ("La Question des races devant l'anatomie et la

<sup>1</sup>Since these reasons are now more or less clear, and we do not need them for the treatment of the present problem, their discussion here requiring too much space, we shall not give them in this place.

linguistique," 1928) a new term—"ethnie"—as a linguistic unit opposite to that of "race." However, F. Regnault gives more than a simple linguistic definition of the "ethnie" which, according to what he says of this unit, seems to be the "ethnic unit" of my definition. In fact, he speaks of the "ethnies" as conqueror, conquered, civilized, migrating, losing their ethnographical complexes, etc. All these characters cannot be confined to the "linguistic unit" only. Moreover, the language may be changed, modified, lost, but the ethnic unit will survive. Therefore the language cannot be taken as the only basis for the differentiation of ethnoses. Moreover, F. Regnault understands his "ethnie" as a static conception, while the phenomenon of ethnos is a typically dynamic one. Here is the second essential distinction between "ethnie" and "ethnos." In so far as I can see from his paper, he proposed this term for the first time in 1920 (in *Revue de pathologie comparée*).

Another attempt has been made at finding a new term covering the unit of ethnos. F. de Saussure much earlier (in 1915, I think, for I don't know about the first edition of his book, but I have at hand the second edition of it, which, however, does not seem to differ very much from the first edition, for the editors say "cette seconde édition n'apporte aucun changement essentiel au texte de la première") made a definition of "ethnisme": "Il y a une autre unité [opposed to that of race] infiniment plus importante, la seule essentielle, celle qui est constituée par le lien social: nous l'appellerons 'ethnisme.' Entendons par là une unité reposant sur des rapports multiples de religion, de civilisation, de défense commune etc., qui peuvent s'établir même entre peuples de races différentes et en l'absence de tout lien politique. C'est entre l'ethnisme et la langue que s'établit ce rapport de réciprocité. . . . Le lien social tend à créer la communauté de langue."—"Cours de linguistique générale," Paris, 1922, p. 305. "La communauté de langue qui constitue, dans une certaine mesure, l'unité ethnique, . . . il y a eu un ethnisme roman reliant, sans lien politique, des peuples d'origine très diverses."—*Id.*, p. 306. Certainly this definition approaches that of ethnos, but the emphasis is put on ethnographical and linguistic complexes. Yet being conceived as a static one, it



meets with the phenomena, like the Roman inheritance, which is certainly a product of disintegration of a larger unit which did not complete its formation. Yet the political aspect of the ethnical unit, of course, ought not to be excluded, as any other ethnographical characteristic.

These two instances suffice to show that such a term was needed, but it ought to be given a more elastic, more definite, dynamic meaning, and it ought not to be confined to the language or ethnographic complex only. As an international term, "ethnie" is, of course, a French adaptation of a Greek word, and as such may not be clear linguistically for other linguistic groups; while "ethnos" preserves its original purity as a foreign word—a scientific term—agreeable to every one. It ought to be pointed out that it is not desirable to limit the content given by me to "ethnos"; and in this respect, as shown, "ethnie" and "ethnisme" cannot be regarded as the same conception as "ethnos." I defend this term because we need such a one, but if it is again modified and simplified, as has already happened with "race," then a few decades later the same need will occur again. It is not desirable to transfer this term into the linguistics, where it will be again adapted, but to leave it as an ethnological (biological) conception and term.<sup>1</sup>

Another attempt at the analysis of the phenomenon of the ethnical unit should be mentioned; namely, that made by A. Van Gennep, who very closely approached this problem in his analysis of the *nationalité*.<sup>2</sup> In so far as I know, this important contribution to the problem has not yet been completed. In the first volume, published in 1922, he approached the problem of *nationalité* from the ethnographical and linguistical point of view, treating the variations

<sup>1</sup>During the publishing of the present work there came to my knowledge a new publication by Sir Arthur Keith, entitled "Ethnos, or the Problem of Race Considered from a New Point of View" (London, 1931), in which I see, with great pleasure, that the problem of the formation of races is discussed as going on in ethnoses. However, no definition of "ethnos" is given and great emphasis is put on the problem of "nation." Since the nation in its relation to the ethnos is already discussed above, I shall not dwell now on the problem as it seems to appear from Sir Arthur Keith's publication.

<sup>2</sup>Cf. "Traité comparatif des nationalités" (Paris, 1922). This work appeared during the period when the problem of "nation" and "nationality" attracted the attention of many authors in connexion with the breaking down of the interethnic equilibrium in Europe. Cf. also an increase of interest in the linguistical problems during the same period, *infra*, Chap. III, Sec. 27.

of cultural (including language) complexes and elements observed in regional units. He should inevitably come to the same conception as other authors who approached the problem of human unit.<sup>1</sup> Since this term has quite a definite function it must not be used in the sense of ethnos nor ethnical unit.<sup>2</sup>

#### 4. Process of Variations in Ethnoses

The mode of the existence of units is defined by a multitude of conditions. There are, on the one hand, inherited conditions, resulting from the adaptation previously created during thousands of years, not only in one of the genetic units, but in all the genetic units which entered into the process of crossing within the given unit; and on the other hand, present conditions of milieu, in which the unit is living. The milieu is not a simple one: it consists of conditions beyond the control of man, which may be called *primary milieu*; conditions created by man, which are essentially a product of culture, or *secondary milieu*; and conditions which are formed by other ethnical units in the midst of which the unit is living, called here *tertiary*, or *interethnic*, *milieu*. Every ethnos adapts itself to the complex of milieus with its aptitudes resulting from the accumulation of physical peculiarities and experience transmitted through tradition. Yet the ethnos itself forms one of the elements of the milieu, and it is partly responsible for the existence of the interethnic milieu—it is a part of a system. The ethnos is thus a result of equilibrium which exists between the phenomena. Therefore the ethnos, regarded as an isolated phenomenon, is a mere and dangerous abstraction.

From the observation of facts one comes to the idea that the existence of ethnoses is conditioned and regulated by a system of

<sup>1</sup>As to the term *nationalité*, it is good for the phenomenon of nationality, which is a reality; but this term may have limited application, namely, in reference to this phenomenon only. This term is closely connected with another term "nation." The *nationalité* may be defined as "belonging to a nation" and as a "potential nation"; while the latter, as will be shown later, is an ethnos (or group of ethnoses) which has a political organization and is recognized by other similar units as a unit of a larger ethnical complex bound by the direct effects of interethnic pressure (equilibrium). Owing to this, the term *nationalité* ought to be reserved for this particular phenomenon, which has a great importance in the process of perception of the ethnical value of ethnical units, and which is one of the conditions of stabilization of the interethnic equilibrium.

<sup>2</sup>Some attempts have already been made at using this term and its equivalents in other languages in the sense of "ethnical unit."



equilibrium. The internal equilibrium of the ethnos is defined by its number (population), its degree of adaptation expressed in its culture, and the physical adaptation and territory occupied. When symbolized, it is  $\omega = \frac{q}{TS}$ , where  $\omega$  is the *constant of ethnical equilibrium*.  $q$  is the number of the population (with corrections as to the age and sex),  $S$  is the biological adaptation in the form of culture and functional power,  $T$  is the territory (with the corrections as to the utility, latitude, altitude, climate, etc.).<sup>1</sup>

The unit may exist only on condition of the preservation of equilibrium—the constant of ethnical equilibrium. If there is any change in the elements forming this equilibrium, the *impulse of variation* of other elements will appear. Positive and negative impulses may also naturally appear, for the elements may increase and decrease. For instance, the decrease of territory may produce either a positive increase of  $S$  or a decrease of population; the decrease of population may result in a decrease of territory, and a decrease of culture, or one of them, and so on; so that the preservation of ethnical equilibrium is a source of permanent variations of elements. I say permanent, for there are at least two conditions which are permanent sources of variations of one of the

<sup>1</sup>In spite of the great simplicity of the nature of this equilibrium in ethnoses, and in spite of the simplicity of a series of logical consequences resulting therefrom, it has never been systematically analysed. Early work on the problem of population and correlation of cultural and other phenomena characteristic of the ethnos in 1912 led me to the idea of binding this relationship into the simple formula shown above. It may here be noted that the idea of such a relationship was formulated during my first travelling in Siberia when I saw a series of ethnical groups showing the same kind of equilibrium, but existing under different conditions. The field observation of other groups during following expeditions (1913–1918) has strengthened the impression of actuality of such a relationship, which was naturally supported by well-known facts from historic records, and by observations of other travellers. In the courses of lectures delivered in the University of Vladivostok (1918–1922, with an interruption) I applied the theory of ethnos for the analysis of ethnical relations. In 1922 I formulated it in a published form ("Place of Ethnography Amongst the Sciences and Classification of Ethnoses," Vladivostok, 1922, in Russian), and in the same year expounded it in a special work devoted to the variations of ethnical and ethnographical phenomena ("Ethnos. Fundamental Principles of Variations of Ethnical and Ethnographical Phenomena," Shanghai, 1923, actually published at the end of 1922). Two chapters of this work were translated into English and published in 1924 ("Ethnical Unit and Milieu," Edward Evans & Sons, Shanghai, 1924), which received a certain circulation and was reviewed in several leading periodicals (e. g., in *Nature*, London, *La Géographie*, Paris, and others). This theory has also been developed from the biological point of view in my new work "The Process of Physical Growth," which

elements of equilibrium. These conditions are: a prompt change of climatic conditions and their periodical fluctuations, to which the unit must adapt itself; and there is an interethnical and inter-species pressure coming out of the neighbouring ethnoses and other animals. In fact, the interethnical milieu also forms a system of equilibrium. I will now give a rough instance. Let us suppose that an ethnos is in the process of numerical increase. It thus needs some new territory for the surplus of population. The spreading of the population is met with the opposition on the part of other ethnoses, so it may take the form of war, which requires a certain technique for defense which again implies a certain complication of the economical and social organization. The pressure on the part of neighbours also takes a form of cultural influence in its various aspects.

The opposition to the pressure must be equal to the pressure felt by the ethnos, if the latter wants to preserve its existence and territory; and it must be higher than the pressure, if it tends to spread over the territories occupied by neighbours or wants to

will soon be published. My hope that the criticism might help me in my further work has not yet been realized. By way of some encouraging reviews, one of my critics has, I think *bona fide*, declared my theory to be a kind of "mysticism"; and another, who happened to be still less prepared for accepting it, styled it as "une farce." On the other hand, I find that in 1924 R. Pearl ("Studies in Human Biology," Baltimore, p. 567, *et seq.*) put as the basis of his theory the relationship between the density and culture as a postulate, and A. Lotka ("Elements of Physical Biology," Baltimore) in 1925 touched upon this idea in his study of equilibria. Unfortunately, these two authors did not develop further consequences. In the same order of ideas are Schmoller's calculations of the density of population in its correlation with the economic system. F. Boas, in his recent work ("Anthropology," in "Encyclopædia of Social Sciences," New York, 1930), formulates thus: "The density of population is determined both by cultural achievements and by environment"; furthermore, a "correlation between density of population and diversity of cultural traits may be observed," and "political organization depends upon the size of social units and upon the density of population" (*id.*, pp. 99, 100). Indeed, in this deterministic form, which calls to mind the methods correlated with the old conceptions of "evolution" and "progress," economical determinism, etc., F. Boas's treatment of this equilibrium is extremely confusing and undesirable. As a matter of fact, social systems, and particularly political organizations, are functions of adaptation, and we have many examples of entirely different social systems and political organizations amongst the ethnoses who have the same density and the same political systems are found amongst the ethnoses possessing a different density of population. The question thus cannot be so simplified. From the above-quoted facts it may be seen that the idea of such an equilibrium is already "in the air."



imply the adoption of its own cultural elements or the whole complex. So an independent existence of an ethnos may be realized on the condition of maintaining a constant equilibrium formed by the opposing pressure of an ethnos to the pressure of all neighbouring ethnoses. However, since the changes which occur in one of the ethnoses result in the change of its internal equilibrium and ethnical value, the interethnic equilibrium will always vary, which implies adaptation and variations in ethnoses. In this way, the interethnic pressure forms an almost constantly acting impulse of variations of ethnical equilibrium, whence result variations of elements constituting this equilibrium. The interethnic pressure is one of the greatest sources of variations, as it is in other animal species competing and coöperating between themselves. Without interethnic pressure the ethnos is doomed to "vegetation." We know practically no ethnoses living beyond the interethnic pressure. The cases of extinct Tasmanians and living Eskimos are instances of isolated groups observed by travellers, but there is no little doubt that prior to their discovery they lived under the pressure of their former neighbours. Here it may be noted that, although the ethnos always tends to avoid interethnic pressure, it cannot successfully survive without this pressure, when meeting other ethnoses which are under interethnic pressure.

The cultural variations, as implied by internal causes (e. g., increase or decrease of population), or external causes (e. g., inter-ethnic pressure), appear either slowly or promptly. They may be maintained during a shorter or longer period with equal or varying intensity. These reactions on the change of ethnical and inter-ethnic equilibrium depend on the ability of the ethnos to react on milieu. Yet, in the process of adaptation, the ethnos may accept various ways of self-defence; e. g., it may change its physical features, its culture, or it may increase its number, or reject by force any pressure, etc. All depends on the way and degree of adaptability and self-consciousness of the unit.

The changes require a certain effort, psychic and mental, which is proportional to the impulses of variation. Yet the tension felt by the ethnos depends upon the period in which the changes occur and the ability of reacting. With the increase of interethnic pressure, and with the increase of impulses of variations, the tempo of changes increases. It is thus natural that *the shorter the period in*

*which changes occur, the higher the tension.* As a matter of observation, the tempo of variations is always increasing, for the impulses of variations and interethnic pressure are always increasing. It is sufficient to remember that the powerful factor of interethnic pressure—population pressure—is always increasing. If at a certain moment it attains such a velocity that the possible tempo of variations cannot be sufficient for assuring the maintenance of equilibrium, the interethnic clash occurs and the equilibrium breaks down. The increase of tempo of variations is one of the most characteristic features of the present phenomena, but it does not go parallel with the increase of the possibilities of reactions amongst the ethnos, so that it menaces the existence of the ethnoses more than ever before. In fact, the dissolution, absorption, re-dissolution, and re-absorption of the ethnoses is so common that the new changes sometimes may be observed during a man's life.

#### 5. Centrifugal and Centripetal Movements in Ethnoses and Formation of New Ethnoses

The nature of ethnoses as shown cannot be understood from the static point of view. Although the present work is not devoted to the theory of ethnos, yet more details are necessary in order to give the idea of the dynamic character of ethnos. The analysis of the mechanism of the formation of ethnoses is one of the good ways of doing it.

Many authors have observed that within the existing nations and powers, even within "primitive" groups, there are in action two forces; namely, those which "consolidate" the unit and those which "disintegrate" it. Since these terms—"consolidation" and "disintegration"—refer to a certain definite unit from the point of view of which the forces are classed, and they have already been used in reference to the phenomena that have no ethnological contents, I prefer to use different terms; namely, "centrifugal" and "centripetal" movements. Let us first define these terms and illustrate our definitions with some examples.

We shall call "centrifugal" movement such actions and behaviour of individuals and groups of individuals which result in the creation of greater cohesion between the individuals and groups of individuals observed who belong to a certain group. We shall call "centrifugal" movement such actions and behaviour of individuals and groups



of individuals which result in the loss of cohesion between the individuals and groups of individuals observed who belong to a certain group. These definitions may be illustrated thus: An individual may belong at the same time to a regional group and to a "nation," and yet to a club, to a clan, and to a large family. His activity as a member of these groups may have thus different effects. If such an individual is very active, he may influence the local group in the sense of preserving local useful customs, of elaboration of new and practical ways of local adaptation (e.g., a new method of agriculture), of an increase of vocabulary, etc. For the local group, this activity may be very beneficial, for the preservation of old and useful customs, inventions, and the further enrichment of the language assure the existence of the local population. On the other hand, if the local adaptation and further elaboration of new dialectal distinctions advance too far, the local group may become so distinct from other groups of the same "nation" that the individuals who belong to it will act as a body opposing themselves to the "nation." Every such action on the part of our individual will thus have a centrifugal effect upon the "nation" and a centripetal effect upon the local group. If the sum of all the local centrifugal actions is greater than that of the actions directed for maintaining the existence of the "nation," the larger unit—the "nation"—will sooner or later disintegrate into smaller local units. On the other hand, if the centripetal movement is stronger than the centrifugal movement, the local adaptation and individual adaptation (including all local and individual inventions) may lose their ability and thus the "nation" will not be able to use the adaptive power of local groups and individuals for the general benefit of the unit. On the whole, such a "nation" will become weaker as compared with its neighbours, which may sometimes put it in a difficult position. So the *most beneficial condition* (from the point of view of the survival of the unit) *is that in which the centrifugal and centripetal movements are equal and strong.*

The conditions of the primary milieu, which, on the surface of this planet, form thousands of distinct regions, as stated, imply a special local adaptation. The better the unit is adapted, the more distinct it is, as compared with other neighbouring groups living in different conditions of milieu. Let us now suppose that such a well-adapted unit has a great increase of population and needs either a new

specialized form of adaptation or new territory. If there is no more free territory which may be occupied immediately or after a short war for it, the only way is to arrange with the neighbours in such a way that the different units involved will become specialized in certain economical branches so that they may supply other units with their respective products. Such, for instance, are the relations among agriculturists, cattle breeders, industrial groups, hunters, etc. Certainly the degree of interdependence is variable. Specialization opens a new possibility for a further increase of population; for, along with the establishment of close relations, intercourse becomes frequent, the individuals who belong to the distinct ethnoses have no more negative reactions when meeting each other, and cultural phenomena are freely borrowed. The same is true of the language, which becomes familiar to the neighbours, who borrow "words" for new phenomena, fashions of speech, and particularly phonetic fashions. At last, when the great distinction of customs and language does not hinder consolidation of relations between distinct groups, they will begin to intermarry, and formerly distinct units will be fused into one culturally homogenous unit, a new ethnos.

In the instance given above the chief cause of fusion is an economical one. It may be of a different nature too. So, for instance, an ethnos which multiplies at a great ratio may attain such a power that it will spread over the territories occupied by the neighbouring ethnoses without destroying the population. The conquered population first opposes the alien control; but, owing to strong pressure, it is compelled to adapt itself to the new situation and thus gradually accepts the new cultural complex of the conquerors. The next step is the adaptation of the language, intermarriage, and finally complete fusion. Yet a purely cultural pressure may produce similar effects. The language is a powerful means of carrying on the centripetal movement.

It may be pointed out that when the process is going on, the possibility of further strengthening the new unit becomes harmful to the neighbouring units who still preserve their consciousness of existence. The opposition to such a process is one of the forms of interethnic pressure which again acts as a factor in consolidating the unity of the formerly distinct ethnoses, so that the interethnic pressure in general is a factor activating the processes of the centripetal movement. It is also activating the process of the centrifugal



movements.<sup>1</sup> The ways are different, but the end is the same. The process is naturally greatly confused by the fact that all phenomena must be *perceived* and *adapted* to the existing psycho-mental complex and at last expressed in "symbols," which must be correctly referred to the referents. Now let us take another process; namely, that of the further possible fate of the large, new ethnos. With the decrease of interethnic pressure, the decrease of a further increase of population, the arrest of a further adaptation of the whole unit, etc., the large unit shows all signs of decrease of the centripetal movement. The centrifugal movement, in the form of local readaptation, individualism, regional differentiation, etc., becomes greater than the centripetal movement, and the unit splits into new ethnoses. The process of formation of new ethnoses will be especially successful if the larger unit happens to be beyond the strong interethnic pressure, as is common in very large territorial units formed of numerous neighbouring units. When the existence of the newly formed units is known, they will receive "recognition" from their neighbours as independent units, and this will activate the process of consolidation of the newly formed units. At the same time, the formation of a new interethnic milieu and a new interethnic pressure takes place. The formation of new languages (dialects, etc.), marriage restrictions, elaboration of specific culture, etc., follow this process and complete it.

One cannot naturally say when the existence of a new ethnos begins and when the old ethnos as a unit dies. This is a continuous process. So if one takes a picture of the existing relations statically one cannot definitely say where the limits of ethnoses are, for the ethnoses may occupy the same territory as well, and the ethnoses may happen to be in the process of formation even within the unit of the socially (professionally) distinct groups.<sup>2</sup> The ethnoses in the process of their formation, life, and decline receive different recognition on the part of other ethnoses. Yet the definitions

<sup>1</sup>As a matter of fact, the nature of interethnic pressure is not always understood, and it is often thought to be *responsible* for these movements. It shows once more how little the ethnoses understand the essential processes undergone by them.

<sup>2</sup>Although the formation of socially distinct groups is also one of the processes of the centrifugal movement which may result in the formation of distinct ethnoses, it is usually strongly checked and arrested altogether, in rare cases going so far as resulting in an open clash among the groups. It may happen at the moment of the strong process of adaptation of the social units and at the period of great changes in culture

and terms, such as "tribe" (preferred by some authors; e.g., O. Jespersen, C. Wissler, and many others) in reference to "primitive" peoples, "nation" in reference to "civilized" peoples, "nationality," even "state," "power," etc., refer to the same processes. Indeed, when they are "recognized," which is connected with a complex "rite of passage" (A. Van Gennep's term) from one state (sometimes supposed to be an "amorphic" state) to another state of purely ethnographical value, they receive general recognition in spite of the fact that as distinct "ethnoses" with all their characteristic features, they might exist for a very long time, calculated in generations.

The difficulty in finding out the limits of ethnoses consists in the fact that the characteristics comprise various elements, some of which are not particular to the given groups, but they have wide distribution amongst other ethnoses. Even such a character as language, being itself a complex, may show different boundaries in various characters (elements). Since the process of formation of ethnoses is a continuous one, the geographical limits of dialects and languages as they are established are sometimes a mere abstraction,<sup>1</sup>

(called "progress," "development," etc.). It is remarkable that such distinction of social groups becomes greater beyond the interethnic pressure; e.g., amongst the potential ethnoses widely spreading over a large area. In all respects the formation of distinct classes corresponds to the formation of ethnoses (e.g., special dialects, endogamy, complexes of customs, "class consciousness," etc.), which may result in the formation of castes (indeed, the formation of castes may have another origin; namely, the adaptation of ethnical units for special social functions in larger units). However, under a strong interethnic pressure, these processes are usually arrested. In case of the disintegration of large units, the process may follow along both these lines: the formation of classes with fierce struggle between them and the formation of regional ethnoses. In fact, this is characteristic of the collapse of large units. On the other hand, if the interethnic pressure is increased, the social differentiation loses (especially for the unit) its harmful effects. Yet if the interethnic pressure is suddenly reduced, the large units which undergo intensive process of changes ("progress," etc.) are very likely to collapse, owing to the centrifugal movement—in the case of sparse population and small regional cohesion, owing to the formation of ethnoses, and in the case of highly differentiated and well-organized numerous groups, owing to the formation of social groups struggling among themselves. The process is usually quickly completed, the interethnic equilibrium being restored and the unit or units returning to the internal equilibrium of the centripetal and centrifugal movements. I shall not go further now, for we shall let these details suffice for the problem of language.

<sup>1</sup>Cf. the recent investigation into the geographical distribution of dialects, also the theoretical treatment as found, e.g., in F. de Saussure's "Cours de linguistique générale," *op. cit.*, Part IV, and special works on French and German dialectology.



The same is also true of the other cultural elements and anthropological features. Yet, in a great degree it holds good for the elements of perception—ethnic consciousness, etc.

The centrifugal and centripetal movements as shown is the mechanism of the formation, consolidation, and disintegration of ethnoses, the occurrence of which chiefly depends on the equilibrium of the system of these movements. As shown, the variations of interethnic milieu have also a great influence upon the intensity of these movements. In the case of sudden change, e.g., due to the fall or increase of pressure, the equilibrium of the internal ethnic movements may be shaken. In this condition, the unit may lose altogether either its centripetal or its centrifugal movement and thus it may lose its "vitality" as a unit.

Now we may come back to the problem of the number of population contained in the ethnos. I do not need to repeat that the number, whether large or small, is not a criterion for a definition whether a group of individuals is an ethnos or not. Theoretically, a new ethnos may originate from a single family, and we have some large groups comprising several millions of a relatively homogenous population which shows all typical characters of an ethnos. Thus the ethnos may practically cover a few hundred individuals, or several millions as well. As to the correlation between the degree of complexity of culture and number of population of an ethnos, for the time being it may be stated that a certain correlation exists, but it is conditioned by so many factors that it is not high. Naturally, since density of population and culture (adaptation) is a constant, it is evident that the ethnos with a simple cultural complex as a rule is smaller than the ethnos with a complex culture, the reason being that the ethnos possessing a simple cultural complex does not need a great division of work, specialization, etc., so that the small unit may survive without being at all numerous.

There are, however, some conditions which must be pointed out: The unit must be large enough in order to avoid the harmful effects of inheritance confined to limited groups, and in order to survive in the case of accidental loss of lives, e.g., owing to epidemics, wars, etc. The second condition is that of interethnic pressure. The interethnic milieu, consisting of numerous units (not always in possession of a complex culture), is dangerous for small units, which may easily be swallowed by larger ones. When in a certain

limited region the units are small, practically all of them are of small size; as, for example, in some regions of central Africa, the north-western regions of North America, in New Guinea, and in other similar regions.

There is one more condition to be mentioned; namely, the character of the geographical region. It has been pointed out that the variety of primary milieu is very great, so thousands of regions may be distinguished. In the conditions of the simple cultural complex the adaptation to the local conditions also implies the formation of small units. As it is often observed in mountainous regions near the sea, especially in low latitudes, the combination of topography, the variety of temperature, and the conditions of the sea form a great diversity of regions. On the other hand, the large territory, with uniform climatic conditions, identic flora and fauna, etc., is not favourable for the formation of small units. It does not, of course, mean that small units cannot survive, if the interethnic milieu does not require large units. Within the large units that are in the process of centrifugal movement, there may be very small potential units formed, which, in the case of the collapse of the large unit, may begin their existence as very small, independent units. Still, it is true in a greater degree of the ethnoses formed within social strata of larger units, which, from their very beginning, may be typical ethnoses, although numerically very small. It is different with the units that receive "recognition"; i.e., when their existence is perceived and the necessary ethnographical complex of "rites of passage" is performed. This is a case of the recognized "nations" and the unrecognized "nationalities." Here it must be pointed out that it is a common mistake that nations are taken as units corresponding to "tribes" of "savages." A nation may be a simple "colony" of ethnoses formed for their own safety, in view of or owing to interethnic pressure. It is quite true that such a unit *may* become an ethnos. The same is also true of groups of ethnoses controlled by one of them ("political control"), but in this case it is usual that the existence of a "nation" is denied altogether. In this case one must be very careful in using the statements of observers, for the problem whether a certain group of ethnoses is a "nation" or not, and whether a certain "nation" consists of different ethnoses or not, may have great practical importance for the regulation of interethnic relations and for controlling the centrifugal and



centripetal movements—the practical aim of every good government, and at the same time the weak point attacked by neighbouring ethnoses and ethnical colonies.

### 6. Cultural Elements and Complexes

In view of the further linguistical discussion, we now have to put emphasis on the process of assimilation of the cultural elements and the loss of original complexes. I shall call the cultural or ethnographical element a phenomenon which cannot be dissected into its component parts. So the term is a relative one, for all depends upon the observer's attitude—from which point of view one looks at the phenomenon. Let us take some instances. The cart is an ethnographical phenomenon which may be dissected as follows: the wheel, the axle, the frame, and the accessories for using draught animals. There may also be other elements used besides these. From the point of view of the economic organization of transportation, the cart will also be an element, besides the motor force used, the roads, etc. The same may be stated in reference to any phenomenon. These groups of elements constitute the complexes. So, for instance, the cart, the motor power, the roads, etc., form the complex of transportation. It is natural that in a complex like transportation the elements must be adjusted in such a way that the whole complex may be adapted to its function; e.g., the type of cart, which is also a complex, must be adapted to the existing motor power, and it must be adapted to the existing roads, etc. At the same time, the method of transportation enters as an element into the complex of economical organization. There is no need of a transport organization if there is nothing to be transported, and so on. In this way, the elements are grouped into complexes and the complexes are again grouped into larger complexes. The whole system forming the complex results from the adaptation of the ethnos to the existing conditions of milieu and its own inherited morphological and physiological complex. To establish where the inherited morphology with its functions conditions the elaboration of the system of adaptation of the cultural complex, and where the cultural complex implies a change of physical and functional peculiarity of ethnos, is sometimes impossible, for all of them are bound in the system of equilibrium. We have already pointed out that all cultural phenomena are a product of adaptation, as well as all physical features

of man, but the character of cultural phenomena needs some supplementary analysis.

### 7. Cultural Complexes as Functional Phenomenon

Referring to the living organisms, we may say that the anatomical structure and its chemical organization are physical, material phenomena; while, for instance, the movement of the given organism is its function. This function is one of the results or effects of the processes going on in the organism, and not the process itself. This function responds to the needs of the organism and as a process in highly organized animals it is regulated by the sequence of combined interactions between the central and peripheral nervous systems. The sequence of actions is co-ordinated, and, within certain limits of variations, tends to attain its maximum productivity dictated by the need of survival and naturally formed as a result of adaptation and selection of species and individuals; and thus, to a certain degree, it may produce an impression of being "inherited," i.e., the function as a sequence of actions responding to certain stimuli may appear to be a condition "inherited" together with the physical condition.

The limits of "inheritance" of functions are still under discussion, for the mechanism of transmission, by inheritance, of the complex functions is not yet clear. However, theoretically speaking, no objection can be brought forth against such a possibility. Indeed, the function as such is not inherited; but the possibility, probably chemical in its nature, of the sequence of processes resulting in the effect of certain functions may be inherited.

Let us now illustrate our idea with some examples. The self-reproduction of the species, generally speaking, is a very complex process, which involves, even in the simplest cases of monocellular organisms, a sequence of processes. In pluricellular and highly organized animals, it is still more complex than in monocellular organisms, for besides a purely physico-chemical phase of the process there comes into action the nervous system. This biological function in man, living in an organized society, is still more complex, for the individual actions are controlled by society, directly or through the complex mechanism of the social institutions reflected through the individual mind in the form of a moral complex, sometimes even in a religious form. A relatively



simple sequence of actions of the process of self-reproduction of the species in man has thus taken the form of an extremely long, complex process, in which an individual chemo-physical function is interacting with the functions which are a product of the long living together of numerous human groups. Still, formally we cannot regard them as distinct functions—the ways and the sequence of actions are different, but the function is the same. In the case of man the actions which precede the act of self-reproduction occupy the largest part of the elements constituting the function. In this function we may distinguish a strictly material aspect and an immaterial aspect as resulting from the complex psycho-mental interaction of human beings. The last instance of self-reproduction permits us to see that in some cases a purely biological function takes a very complex form, the effectiveness of which is conditioned by the interaction of various elements entirely veiling the original form of the function. In fact, the complex psycho-mental behaviour of the partners is conditioned, not only by the individual characters, but also by a complex of actions and attitudes implied by the ethnical milieu in which the individuals are or were living. Moreover, the control of the unit in which the process of self-reproduction is going on greatly interferes with the act. The complexity of this act is due to the complexity of individuals directly concerned and the complexity of the conditions in which the ethnical unit may survive. The conditions of the unit are different as to their nature. They may be social, i.e., comprising elements of the social organization; they may be economical, religious, and sometimes ethnical, as when the partners belong to different ethnical groups. Owing to this, we cannot regard the phenomenon as solely psychic, physiological, social, economic, and so forth, for all the above-enumerated elements are in play. The phenomenon is evidently a resultant from a long adjustment of all elements involved, and as such it is a complex form of adaptation of a certain biological function to the given conditions; the latter, however, do not change the biological nature of this function. For this reason, a one-sided analysis of the phenomenon inevitably leads to an incomplete understanding of its nature and function.

The above-described case is not intentionally selected. Any phenomenon of human activity may be reduced to its simplest form, in which it has to be dealt with by the biologist in a narrow sense of

the word. As a matter of fact, all human activity which results in the complex cultural phenomena is nothing but a complex form of biological adaptation. In different living organisms the ways are different, but their nature remains the same—it is a functional adaptation of the individual organism to the existing outer world.

We can now proceed to the question of variations of the forms of adaptation. I intentionally use the expression "variation," for the latter does not presume any particular or predetermined form of change. The variation is a mere change in form or function. If we suppose that the process of cell fusion, which is a chemo-physical process, and thus a material one, in the case of self-reproduction changes, two cases may be expected—one in which the old form of self-production does not hinder the new form of cell fusion, and thus may be preserved intact; and another one in which the old form comes into conflict with the new form of fusion. In this case the physical condition of the cell will be responsible for the further changes in the function of self-reproduction. Yet there may be another case in which the physical conditions of successful fertilization are not affected, but the conditions of milieu require a new form of adaptation; and if it is made without affecting the process of cell fusion, the species does not bear any harmful consequence. But it is not so when the change of the functioning system is of such a nature as to affect the self-reproduction process. The existence of the individuals who form a self-reproducing unit is then threatened by extinction. It is evident that in all cases we have a well-balanced and well-adapted system, and if it were not so the unit could not survive.

The maintaining of the equilibrium is achieved by the process of minor changes in which some elements are affected by changes, while some other elements may not. There is naturally a great number of elements constituting the phenomenon taken by us as an instance, but all of them are not necessarily subject to changes if one of them is so. This may be called "partial change." Let us illustrate this condition. In the system of self-reproduction the wedding ceremony plays a very important part, for it gives social sanction to the marriage, and thus opens possibility of self-reproduction. Self-reproduction is possible, however, without any wedding ceremony and marriage; but in the given ethnical unit, in so far as the integrity of the unit must be preserved, including its



cultural complex, the possibility of smooth functioning, etc., the process of self-reproduction may be carried out only on the condition of performing all the rites and customs connected with the act. Let us now suppose that in the wedding customs new elements are introduced—for instance, the smoking of tobacco, which was recently<sup>1</sup> introduced into the given group. The innovation will complicate the old complex customs of the wedding, unless it is substituted by some other practice; but it will produce no essential effect on the physiology of self-reproduction. If the introduction of tobacco smoking during the wedding ceremony substitutes some other custom, there may be a shortening of the wedding ceremony and a loss of some older customs (elements) which may have a certain bearing upon the function of self-reproduction. Here I have in view, for instance, the difference in the cultural complex which very often hinders the crossing of distinct ethnical groups, and thus may have a certain influence on the process of crossing with alien groups which present some physical peculiarities.<sup>2</sup> The chain of connected elements is sometimes very long and in some cases it is short. So the importance of elements in so far as they affect the whole complex ought to be weighed with great caution and only after a minute analysis.

#### 8. Variations of Cultural Phenomena

Let us now take some ethnographical complex, for example, the wheeled vehicle, and follow its variations. The wheeled vehicle made its appearance in prehistoric time. At the beginning of the historic period it was found to be very simply constructed, but already well adapted for the needs of transportation and carried on by oxen and horses. We may suppose that it did not appear before man had learnt the skillful use of implements for working wood and before the draught animal had been domesticated. The vehicle, during the long time of its existence, was adapted for various needs and particularly for war as a chariot. The art of manufacturing chariots at a certain period attained its highest skill; but, with the

<sup>1</sup>This is the case in most of the Asiatic groups.

<sup>2</sup>The introduction of the tobacco-smoking rite may serve as a method of approach between two alien groups. As a matter of fact, sometimes a minor change in the habits and customs, e. g., the change in the shape of the clothing, opens the possibility of approach of males and females belonging to different ethnical, and even "racial," groups

change of methods of warfare, the chariot disappeared altogether. During the mediæval period the wheeled vehicle went through a series of variations. At the end of the eighteenth century it attained its most complex and artful forms, which were possible because of the introduction of new material, such as iron, leather, painting, etc. The external forms, ornamentation and material, were used in accordance with prevailing tastes and technical possibilities. So different styles were created. The type *dormeuse* was gradually introduced in different countries, and everywhere it received some new elements in accordance with the local tastes for ornamentation and practical needs conditioned by distances, the state of the roads, the wealth of the owners, etc. Then different local types originated. No *dormeuse* was possible in the regions where there were no horses or where there were no roads, so these conditions put a limit to its spreading. In the nineteenth century, together with the extension of railways, the practical need of the *dormeuse* began to diminish, but it survived for a little while in some regions where the railway did not yet exist. At last it died out altogether, and is now preserved in the museums and in very rich families who keep their relics. When the horse began to give place to another motor power,—gasolene,—the material used for the vehicles adapted for a new motor power must be different—metals, leather, rubber, and very little wood. First, there was no spacious room for seats, but afterwards, with the reduction of the engine, the type of a spacious, comfortable vehicle again made its appearance and has received such great popularity that at the present time it may compete with the railway, at least for travelling at short distances. It has not yet received such a great variety of styles and types as with the *dormeuse*, but this may come later if the diversity of local ethnographical complexes requires it.

This is an instance of a varying ethnographical phenomenon which spread over the world during a relatively short period, gave hundreds of varieties adapted to the local ethnographical complexes, and died out before the new motor power was applied. However, the *dormeuse* could not make its appearance without the smith's skill, the skill of various other artisans, and without well-trained drivers, and also without the owner who needed a conveyance for long and comfortable travelling. So that the *dormeuse* was in a certain ethnographical complex in which not only the conditions of



industry must not hinder the construction of it, but in which the social conditions must be as they were, including the idea of "lady," who could not give herself the trouble of such uncomfortable vehicles as used by the farmers. However, the whole complex of the *dormeuse* was not migrating from one region to another, but only some of its elements, while other elements of an entirely different origin might be in simple agreement with the former ones. From a direct observation of ethnographical facts, we know that any element may be borrowed if it does not require the complete reconstruction of the whole complex, and that when there is need of some new element, it may be either invented or borrowed from the neighbours and even left with no modifications if it suits the complex. On the other hand, practical inutility, the need of a great readaptation of the element or the complex, and lastly a lack of familiarity with a new element may become a hindrance to the adoption of new elements. Yet we also know that the elements are always adapted to the complex; and since there are continuous changes in the complex, the elements may also go through the changes if their preservation in all forms comes into conflict with the complex. If it does not, the element may be preserved for thousands of years without change. The elements may also be borrowed without any evident practical impulse; as, for instance, a new form of dress, tie, etc. The only reason is that the other people who are worthy of imitation use a different form and the latter may be imitated. Yet it may lose its seeming utility and still be preserved, for it does no harm to the people and occupies a certain place in the complex, making a conventional distinction between the given element or the whole complex and other similar phenomena. We have seen that the wheeled vehicle was used as a war chariot and changed later into the *dormeuse*. So the elements may change their function as well. If we analyse any existing ethnographical complex, we may find that it consists of different elements and complexes well adjusted one to another. So, too, the social organization must respond to the needs of economical organization and function, the psycho-mental complex must satisfy the needs of technology, and functions of the social organization. If there are maladjustments of complexes, or if certain elements show overgrowth, e. g., owing to the temporary psycho-mental attitude (perhaps too great attention is paid to them), running over the limits of

practical utility, or if some elements show an insufficient adaptation and a deficient quantitative value, the whole complex may begin to suffer from the lack of equilibrium, which may result in a harmful reduction of resistance on the part of the bearers of the given cultural complex. When the complex shows such a disequilibrium, it loses its value as a function of adaptation of the unit, and the latter will not care any more for its preservation as a whole or as elements. The cultural complex is thus a product of long adaptation of the unit. It consists of elements preserved from time immemorial, of elements borrowed from the neighbours and adapted with modifications or without modifications, and of elements created by the unit in the process of adaptation. Some of these elements may be of vital importance, some of them may be easily substituted at any moment, and, last of all, some of them may have only a secondary importance as an "ornamentation," a completion of the complex. But, as a matter of fact, there is no element which is absolutely functionless in the system of equilibrium.<sup>1</sup> During the history of ethnical units, the complex is always in the process of change. At one period a group of elements and whole complexes may be numerous and important in the system of equilibrium, as it was, for instance, with sheep breeding in England during the eighteenth and nineteenth century, which was implied by the growing manufactures; while later it may give place to another manner of adaptation, and may disappear altogether, yet reappear again and again. So that although the complex shows its "continuity," it is not the same at any subsequent moment.

#### 9. Impulses of Variations, Their Intensity and Tempo of Variation

The change of cultural complexes and their elements as shown is stimulated by the need of adaptation of the ethnical units. But this broad statement can be dissected. First of all, there are permanently acting sources of impulses, i. e., the adaptation to the seasonal variation of weather, the periodical variations of climate,

<sup>1</sup>When one applies a "rationalistic" point of view, one falls into an elementary methodological error; namely, one implies one's own ethnographic complex (the complex of rationalization is more or less characteristic, and in various degrees, of all ethnical groups, and it is especially common in the European complex) to an alien complex. The effect is not an analytical penetration into the mechanism of the investigated complex, but the effect is the unveiling of one's own reactions.



the consummation of food, and the wearing out of utensils, clothing, dwellings, etc. However, the adaptation in the given conditions, including variations of weather and climate, may be so perfect and flexible that there will be no need of changing anything in the system. Yet the system of the supply of food, utensils, clothing, and dwellings may also be so perfect and regular that no impulse of change will appear. But there is an important factor of unavoidable change; namely, increase of population. The increase, even at a small rate, requires the readaptation of the unit, for a need of a new territory or that of a new way of division of work appears which would allow the maintaining of the existence of an increasing number of members forming the unit. The pressure of population is a permanently functioning stimulus if the unit is in the process of growth. Since the change of the economical organization or the simple regrouping of the elements is implied by this factor, the other elements of the ethnographical complex must also be readapted. Then a series of continuous changes follows.

Yet there is one more important source of variation; namely, the change of primary milieu, which may take place either as a result of a sudden change of climate, periodically occurring, or owing to the migration of the unit, total or partial, which is a common case with growing units. The readaptation in these cases is implied by the need of survival and it must proceed immediately. This process is very often correlated with the change of the physical adaptation of the ethnical groups.

At last a great source of change is the interethnical milieu; i. e., the surrounding ethnical groups. The necessity of change is implied by the pressure on the part of the neighbours who in the process of numerical increase tend to occupy new territory. In order to oppose this pressure, which often takes on a military form, the unit has to produce effort for a certain increase of its population or for the perfection of the military and other technique. Another way of pressure operates through the imitation of cultural complexes and elements found amongst the neighbours. The importance of this source of change may be realized if we remember that there is no one ethnical group which would be beyond the interethnical pressure.

The effect of the interethnical pressure may be better seen when the difference of the ethnical value of the units is considered. The

ethnoses are found under the pressure of all other units. So if the ethnical value of the ethnos is defined as shown,  $f = \frac{I}{\omega} q^2$ , and its *interethnical actual value* is increased, owing to the impulsive pressure of all neighbouring ethnical units, i. e.,  $\varepsilon = \frac{I}{\omega} q^2 \sum i$ , where  $\sum i$  is the sum of all impulses of variations active amongst these units, then the intensity of the interethnical pressure and its effectiveness may be realized.

The possible forms and sources of impulses of variations may be classed into three groups conditioned by the quantitative change, either of population, or culture (and in general adaptation), or territory, which imply the change of other elements forming the ethnical equilibrium of the unit. The impulses of this type I have called "impulses of variations," from the formula of the constant of ethnical equilibrium  $\omega = \frac{q}{ST}$ , whence the impulses of variations may be understood as proportional to the relative increase of some of the elements for  $\frac{\Delta q}{\Delta S \Delta T} = \omega$ . The impulses of variations considered as shown hold good so long as the ethnos is isolated and where physical changes do not affect the unit. However, as we have already seen, the ethnos cannot be regarded as an isolated unit—this is a mere abstraction—and thus the processes cannot be observed as they actually are.

The impulses of variation may originate owing to the pressure of interethnical equilibrium, so the ethnos may be compelled to change, for instance, its cultural adaptation, the positive outcome of which may be totally consumed by the needs of opposing the increase of the interethnical pressure (for maintaining the equilibrium); or if the reaction of implied change may show an excess, the latter will produce further impulsive variations of other elements of ethnical equilibrium as it goes on in the isolated ethnoses. The same effects may be observed in the case of the change of population implied by the interethnical pressure.

It is evident that we meet with the same situation when the source of the impulse of variation is the pressure of change of the primary milieu. However, the situation is different when there are involved changes of the physical (particularly physiological) adaptation. In



fact, the latter as shown is the element to which are adapted all elements of the secondary milieu (culture). If the variations of milieu, both primary and interethnic, imply a change of physical characters in the process of adaptation of the ethnos, the whole system of ethnical equilibrium must be rearranged. In the process of rearrangement the unit may lose its ethnical equilibrium and naturally collapse or slowly disintegrate under the interethnic pressure.<sup>1</sup>

It is impossible to enumerate here all the cases of impulses of variations and their combinations; yet for our purpose—as an introduction to the processes of variations in language—it is also not necessary. What is stated in the above lines may suffice to show how these processes are complex and how they may be analysed.

Now we may proceed to the problem of intensity and tempo of variations of cultural complexes and elements. We have already seen that in the ethnoses the variations may have a different intensity, tension, and tempo, which depend on a great number of various conditions. Let us suppose that there are no impulses of variations, which certainly would be an ideal case, then there will be naturally no variations. If the impulses appear, and if the ethnos wants to survive in its former integrity, it has to produce certain changes, which it does proportionally to the impulses of variations.

<sup>1</sup>It should be here pointed out that the pressure of population has its own peculiarities; namely, in ethnoses, in other animals and plants as well, the potential increase of population greatly exceeds the practical possibility of change of cultural adaptation and extension of the area occupied. This calls to life a complex mechanism checking the growth of population beyond the limits put by the given equilibrium. In man this must pass through the process of adaptation of the psychomental complex. If the ethnos loses its ability of maintaining its equilibrium, there may occur an uncontrolled increase of population which cannot be followed by the variation of the secondary milieu (culture), and thus the only way of adaptation will be the change of physical characters. Practically, it takes the form of pauperism and physical "degeneration." It is evident that in this process the interethnic value of the ethnos is reduced, and it is doomed to perish under the interethnic pressure. On the other hand, the overgrowth of the secondary milieu, which is not followed by a balanced increase of population, and which is not implied by the interethnic pressure, may also result in the loss of equilibrium and physical changes. Practically, however, the interethnic milieu does not leave, for the completion of the process of decline and ethnos as an independent unit perishes much earlier, very often through the process of anthropological substitution. Furthermore, the loss of elasticity in the regulation of the self-reproductive function, balanced with the growth of  $S$ , may also result in the inability of adaptation to the growing interethnic pressure (e. g., the case of ethnoses practising for a long time artificial methods of birth control), and thus loss of interethnic value, a further disintegration, and the death of the unit.

Naturally, *the greater the impulses of variations, the greater the intensity of implied variations.*

Since the impulses of variations are permanently acting, and since some of them are permanently increasing, the intensity of variations increases with them. True, there is a limit of increase; namely, when the change requires the effort which does not compensate the results achieved by the change, then the change not only becomes useless, but becomes harmful for the existence of the unit. However, the change as such means very little, for the essential condition is the tempo of changes, or changes occurring at the unit of time. This is very important, for every change must pass through the mechanism of a psychomental complex, which has to reflect the process and adapt itself for the act of change, *ante* or *post factum*. From the observation of facts, we know that the psycho-mental ability of ethnoses in tension, which must be proportional to the tempo of variations, is different. Yet there is also a certain limit of tension that cannot be passed over. In fact, the existence of cultural elements is possible only on the condition of their being recognized by the ethnical unit which underlies the mechanism of their transmission to the succeeding generations. If there is a negative reaction on the cultural element the latter cannot be included into the complex. On the other hand, the attachment to the existing elements and complexes may also obstruct the changes up to the degree when the unit may suffer from the lack of adaptiveness. In case there is no reaction on the elements and complexes the tension will be very weak, for it will require only the mental effort of logical reasoning to bring the unit to the idea that a certain change must be produced; but at the same time it will not be stabilized and economically used in the old complex. But if there is an absolute refusal to carry out a change, the changes implied may produce a dangerous effect; namely, a kind of psychic disequilibrium of the unit in which the minor changes may absorb the attention and creative power of the unit too much, in this way depriving it of the economical use of its faculties for further adaptation. So the tension cannot pass over the limit when it begins to threaten the psychic equilibrium of the unit.

From this analysis of the process of changes, we may see that some elements may be changed without producing harmful effects upon the equilibrium and the functional productivity of the



complexes. On the other hand, we have seen that the adoption of certain elements brings immediate variations and are promptly recognized, while some other elements can be accomplished only after a long process of assimilation and adaptation. Yet, in some cases, no tension is required for producing a change, while in some other cases a tension is required to pass over the limit of ethnical ability to the tension itself. Moreover, we have seen that the elements to be introduced are almost always found ready in the complexes of their neighbours, or they may be created within the unit itself if they are greatly needed.<sup>1</sup>

So that we may now conclude: the mechanism of the preservation and introduction, creation included, of new elements and complexes is such that it may be favourable or unfavourable for the continuity of elements regardless of complexes, and it may be favourable or unfavourable to the preservation of complexes, regardless of elements, the chief reason of which is that culture comprising elements and their complexes is *functional adaptation*. In this aspect, it is included as one of the elements of the constant of ethnical equilibrium and thus it is subject to variations, as any other elements constituting ethnical equilibrium.

The only reason for the existence of elements and complexes is that they are needed as a method of adaptation and the ethnoses do not hesitate to choose the way to receive and introduce them if they can be adapted to the complexes, and they are not rejected till they threaten the existing equilibrium.

<sup>1</sup>As a matter of fact, most of the new, promptly needed elements are usually discovered by one of thousands of existing units, whence they spread over other units which need such an element. However, investigators usually pay great attention to those units which are in the process of great creative adaptation. This function is observed chiefly in leading ethnical units. The mechanism of adaptation by the creation of new elements is very complex, so it will not be discussed here; for it would require too extensive a treatment, while a great majority of units never use it, for they can borrow needed elements from their neighbours, so that the ethnical unit merely draws elements from the interethnical milieu and creates only in cases of a great need of adaptation. Of course, I am far away from the idea of minimizing the importance of the creative ability of ethnoses, but it has practically a relatively minor importance in the history of adaptation of individual units. It begins to work only when borrowing is more difficult than creation, or when borrowing is impossible. In my further works I shall devote more attention to the mechanism of creation in leading ethnoses which may survive only on the condition of the creation of new elements and new complexes. I shall also show why investigators are so much interested in this problem. This is one of the curious ethnographical phenomena.

## CHAPTER II. LANGUAGE

### 10. Language

Language is one of the most complex ethnographical phenomena. Since the existing definitions of language show great variety, I shall point out the views which form the basis of my present work.<sup>1</sup> First of all, I presume, as the most probable conception, that the processes emotionally perceived by individuals as "thought" are chemo-physical in their nature which form a chain of absolute and conditioned reflexes,<sup>2</sup> and the origin and location of which are not confined to the central nervous system only. Language, in a broad sense of the word, may be thus defined as a complex of various methods by means of which an individual may produce in another individual, and in himself, a chain of emotionally perceived chemo-physical processes—the thought. Since the aim of the acting individual is to start in another individual a certain reflex, or a chain

<sup>1</sup>To clear up the field of our discussion, let us first point out that the organs used for speech throughout all human groups do not present any hindrance for producing any sound met with in different languages. Yet the organs that are now used for speech might have existed at the period when the ancestor of the present man could hardly be regarded as an animal of the same species as the present man. Investigations into the evolution of the human brain have already led anthropologists to the idea that even the earliest precursors of man, such as the *Pithecanthropus erectus*, possessed the necessary physical conditions for certain speech, in so far as one may infer from the structure of the brain. The anatomical characters of the facial, nasal, palatal, and jaw-bones in quaternary Neanderthal man (i.e., a hundred thousand years ago) were of the nature to permit their possessor of having soft parts adapted enough for producing various sounds needed for speech (cf. the works of M. Boule and Elliot Smith). Thus, from this point of view, the conditions of the physical factor is nearly constant for all human groups and practically unvariable during the period when the variations of language may be observed. Since this is true, the individual physical adaptation of the organs used for speech is the only aspect directly connected with physical fitness and unfitness, which thus cannot be held responsible for the peculiarities of phonetics of particular languages. In other words, the anatomical point of view cannot help us in finding the differences in existing languages. Thus there remain for consideration only the functional, also psychological and social aspects of the problem.

<sup>2</sup>J. Pavlov has not yet published his final conclusions as to the human "thought," but he leaves us to understand that such is the nature of all simple and complex phenomena of animal behaviour.



of reflexes, I shall use the term "starter" for all the methods used for the realization of the aim of the acting individual.<sup>1</sup>

In this complex we may include all the methods used without giving preference to any one of them and call this complex *language*. The narrow sense of "language" is that confined to a selected group of elements constituting a language.

The technical methods of language chiefly comprise two groups; namely, optical and auditory starters.<sup>2</sup> In the first group we have (1) images; (2) symbols (e.g., written languages, including those approaching the image, but used as "symbols"); and (3) gestures (e.g., those of the hands, the arms, and the legs, together with the feet, and especially of the whole body, and particularly mimics), which may have direct and symbolic functions. As a matter of fact, conversation in certain cases may be successfully carried out only by means of the optical starters of the third type. The written language may be effective without ever being "read" with the voice and even without being expressed in corresponding sounds (sounding starters), as, for example, in mathematical symbolism; yet it may give origin to a spoken language when the symbols are "named," i. e., when the symbols are "read" in sounding starters. In the second group we have combinations of sounds. The latter are built up by sounds produced by a combination of physical organs,<sup>3</sup> by the pitch of musical tone and its complexes, and by a variable length and degree of air pressure.

A spoken language may consist not only of sounds, but it may also include elements of the first group; e.g., gestures, particularly

<sup>1</sup>The starters, as well as the processes implied by them, have received various names; e.g., "sign," "image," "word," etc., and particularly "symbol." I avoid the use of the term "symbol" in this sense, for it has already been used by L. K. Ogden and I. A. Richards for covering only a certain group of starters. According to them, a "word may function as a symbol and as an emotive sign" (*op. cit.*, pp. 228, 257, and Chap. X). Indeed, "symbol" as it is used by these authors must be reserved for a special study into the "meaning of meaning" and "symbolism." Whether the starter acts like a "symbol" or like an "emotive sign," in either case the process implied is perceived conditionally. Degrees of participation of various organs in this perception are naturally different, which is again connected with the complex of conditioned reflexes, and which sometimes is strongly felt by the individual (emotion).

<sup>2</sup>I say "chiefly," for there are other groups of starters—olfactory, touch, etc., but they do not play a very great part in the language.

<sup>3</sup>According to all linguists, there are no special organs of speech, but various organs are adapted for this function.

mimics.<sup>1</sup> The using of gestures is subject to great variations in different languages and individuals. Some languages cannot be spoken in the dark, while some others may. The language of the latter group cannot be considered as "better developed or richer" than the language of the first group, but it may be regarded as more specialized in choosing "starters."<sup>2</sup> From this point of view, specialization is only a way of adaptation, and is as good as any other in responding to the needs. Naturally, the choice of starters and their grouping into complexes is a long process, originating first in individuals who are actual inventors (conscious or unconscious) of starters. Besides the starters originating in a given milieu (transmitted by tradition—imitated or invented by other members of the same group), there are starters introduced by individuals for their own personal use as well as for starters of processes in other persons, and the number may be restricted to one more person or spread over hundreds of millions. Individuals and groups of individuals may have a distinct complex of starters. We may distinguish (1) the individual language (used in monologues that are usually not pronounced, and, as a rule, unknown to other persons);<sup>3</sup> (2) the language of a small group (e.g., the family language, the lovers' language, various argos and slang, the club language, etc.); (3) the language of a larger group, containing the more or less stabilized complex of a great number of starters; and (4) the inter-species language (particularly for domesticated animals), containing a small series of conditioned and absolute starters. The use of methods in producing starters is naturally subject to great variations in these four types of language.

The question when and how language was begun we must answer in the sense that the language is not a particular phenomenon confined to man, for the ancestors of man, other and distinct animals,

<sup>1</sup>It is a well-known fact that the speeches of some famous speakers lose their beauty, even their sense, when they are reproduced without gestures and especially when they are reproduced in written form. In the last form they sometimes become "meaningless" and flat.

<sup>2</sup>It may be pointed out that the specialization of a language along the line of its approach to a symbolic written form is an evident impoverishing in the sense of limiting the means of producing starters.

<sup>3</sup>The reason for the existence of an individual language (the complex of starters!) is that individuals also need special starters for their own use when "thinking" or "monologuing."



possessed their own languages, just as other animals now living possess their own languages as a complex of sounding and non-sounding starters. From this point of view, there is no dividing line between "human" and "animal" languages, and historically the "origin" of language cannot be established. From the "human" point of view, it ever existed as a function.

It is different when one excludes certain elements forming a language or if one puts a special limit (e.g., only "articulated") or emphasis on certain elements of a language. So, in dependence upon the choice of elements, there are many distinct definitions of language. For illustration, I shall now quote some of the latest definitions. E. Sapir gives the definition of language as an acquired, cultural function which may be accepted if we extend "cultural" to the meaning of "adaptive"; but he puts an essential limitation when he considers the language as a "system of voluntary produced symbols" ("Language," p. 7.), for the language contains also "absolute starters" and the limitation is still extended when he introduces another definition—"language is primarily an auditory system of symbols," where all optical elements and all "emotive signs" are excluded. Such a definition of language practically reflects the idea of modern linguists on the *spoken* languages which may be easily viewed and with which they are dealing. The definition given by O. Jespersen ("The Philosophy of Grammar," p. 17) is different, namely: "The essence of language is human activity—activity on the part of one individual to make himself understood by another, and activity on the part of that other to understand what was in the mind of the first"; but the language is actually confined to the "human" species and to spoken language, for in reference to written language O. Jespersen says, "A written word is mummified until some one imparts life to it by transposing it mentally into corresponding spoken word," which, as a matter of fact, is not always necessary. Another definition is that "the language of a nation is the set of habits by which members of the nation are accustomed to communicate with one another" (cf. "Mankind, Nation, and Individual from a Linguistic Point of View," p. 23). Here we may point out the conception of the nation as a unit. J. Vendryes ("Le Langage. Introduction linguistique à l'histoire") agrees with the definition of language as "un système de signes" given by B. Leroy ("Le Langage," 1905) in which all starters are included. However,

"*signe*" he identifies with "symbol capable de servir à la communication entre les hommes." He confines himself to "le langage auditif, appelé aussi langage parlé ou langage articulé" (*id.*, pp. 8, 9). However, F. de Saussure (*op. cit.*, p. 26) comes to the conclusion that a language is "un système de signes distincts correspondant à des idées distinctes"; but he has previously formulated that "il ne se laisse pas classer dans aucune catégorie des faits humains parce qu'on ne sait comment dégager son unité" (*id.*, p. 25).

One may see that these definitions are not absolutely alike. These differences are due chiefly to the character of language itself. The existence of specialists, such as "grammarians," "philologists," "linguists," etc., who attack the language from various points and give different definitions of language, shows how great is the complexity of the phenomenon. At which moment this form of human adaptation may be called "language" totally depends upon the author's point of view.

#### 11. Sounding Starters

The methods of producing starters as stated are numerous. In man, at certain moments, human or prehuman, the sounding starter has received special application. However, so far as the operation with the sounds is concerned, it presents rather limited possibilities; namely, production of sounds by the lips, the tongue, the nose, the teeth, the soft and hard palate, the glottal complex, and rarely the cheeks, also all physically possible combinations of these organs. The combinations are not very numerous. For producing new varieties of fundamental sounds these may be increased by length, by tones, by sequence of tones, and by distinct degrees of stress; and lastly, by the combination of various sounds into new complex sounds. The subsequent addition of sounding starters permits a variation of complex reactions (in hearer and speaker) and gradually directs a series of subsequent reactions. The direction may be produced by various means; as, for instance, special starters, the order of different starters, etc.

The formation of stable, conditioned reflexes responding to the starters and their observation is the way to find a practical value (meaning) of various sounding starters and their practical use, as



"engrams."<sup>1</sup> It is naturally a long process and it is not particularly "human." So the sounding starters, by the side of other methods, have become a powerful means of social intercourse, owing to which sounding starters might be greatly increased up to the point of forming stable complexes. The differentiation of sounding starters into that used for the phenomena directly perceived, that directing the process of connecting conditioned reflexes in new combinations, and that producing starters only in the complex combinations is a long process, in which the sounding starters (also their written symbols and, generally, their optical starters) went through the variations which at different moments might correspond to what is called "sentence," "word," and "particle."<sup>2</sup> However, these variations are not one-sided, i. e., the sounding starter may first correspond to a "sentence," and afterwards to a "word"; and a simple conditioned reflex perceived as "thought" may be produced by a simple starter first, but afterwards it may require a complex starter. Yet the starters may be preserved for thousands of years without any changes.

In so far as the reactions of hearers are concerned, the increase of combinations of starters produces new effects. They may become very complex. Beginning from a simple sound and going through a gradual increase and changes, the complex starter may attain the length of a process lasting for several seconds. A simple complex

<sup>1</sup>This is the term used by Semon, defined by C. K. Ogden and I. A. Richards (*op. cit.*, p. 140),—"to call upon excitation similar to that caused by the original stimulus." The engrams may be dissected into elements.

<sup>2</sup>The distinction of "sentences," "words," and "particles" is conventional, indeed. It is sometimes impossible to distinguish these formal elements. E. Sapir asserts that words exist as an entity somewhat independent of the sentence, for proving which he brings some facts of his experience with the American natives. I can support this statement by similar instances of the Tungus, whom I taught to use a phonetic transcription for recording their own language. However, these facts must not be overestimated,—in some languages, the words are entirely fused together to form the "sentence," which becomes a complex starter beyond which words do not exist; yet in some languages, the "meaningless" particles used only for directing the connexion of conditioned reflexes are consciously used separately as "words." The starter may be as small as a single sound and it may be as long as a long sentence. Children often perceive sentences (J. Piaget's observations in "The Language and Thought of the Child") as single starters which may be easily "explained" by starters of various length. But it does not mean that the unit is a "sentence," and not a "word" or a "particle." The sentence may be a single starter in which words cannot be abstracted, and a compound sentence may form a compound starter in which single words may be separated. It all depends upon the character of the starters.

sounding starter isolated from a compound starter sometimes loses its functional value altogether. Thus the discrimination of nouns, verbs, words, sentences, etc., is, when made, sometimes absolutely impossible and artificial. Such a proceeding with the languages is a mere adaptation of one ethnographical complex for another. In the choice of ways for finding new starters and their combination, individuals are often led in the line of least resistance, as it is characteristic of all other phenomena of human adaptation. If a new starter appropriates its function as a useful starter, it is adopted and comes into use. If the method invented for rearranging starters for producing complex starters is practicable, and a directing starter is effective, they may receive general recognition. Their nature is, indeed, a functional one. It may thus be stated that *all methods are good in so far as they have a practical functional value and the methods are different.*<sup>1</sup>

## 12. The Language in Ethnoses

Indeed, it is a plain truth that language does not exist beyond society, in a broad sense of the word. However, the process of stabilization of a language, i. e., when starters become effective instruments in the hands of individuals, may take place only in the case of a certain stability and continuity of the organized unit. The new starters receive their functional value, gradually spreading over larger groups of individuals, so that their spreading is conditioned by the existence of interrelations between the units. The language as a complex of starters has thus certain limits for its variations and spreading. As shown, such units are ethnoses, so we may now proceed to the fate of language when it is used in ethnoses.

When an ethnical unit needs a new starter, it uses the known elementary sounds of the unit. However, since the number of combinations is limited, all possible sounds are soon exhausted, and the unit may choose one of certain ways for producing new sounds. These ways may be of purely phonetic order, as, e. g., a change of quantity of the elementary sounds, a change of sounds through

<sup>1</sup>The history of the Indo-European languages shows instances of loss of tones as an ancient element of the formation of starters, while the Chinese languages show a phenomenon of their probable increase. The old special starters (flexions and suffixes) of the Indo-European languages were lost, after which new methods were introduced (prefixes), while "flexion" and "suffixes" continue to survive. As these facts are well known, there is no need to repeat them here.



palatalization, aspiration, labialization, voicing, regulation of explosion, etc. If these methods do not suffice, there is one more means; namely, the increase of sounds with varied musical content, pitch of tone, etc. Lastly, the method of a change of the type of sound may be used; e.g., when a bilabial consonant is altered into a glottal.

The phonetic change may be substituted by other methods of production of new sounding starters, so that the original simple starter may be increased with other sounding elements. This method receives a prevailing application in the formation of new starters. The latter may be produced by the repetition of an old complex, its increase with another handy complex, or an increase with special starters leading the process of the hearer in a certain definite direction by means of various suffixes, prefixes, or infixes. As stated, this group of starters does not differ from any other group of starters. The most common case is the preservation of the old phonetic complex and its combinations with some other old complexes. A new combination receives the function of a distinct starter. It is evident that the ethnical unit may use any of the above-indicated methods with a preference to some of them, or without any particular choice of methods. The latter may depend on various conditions in which the habit of using certain methods may become responsible for a further application of the same method, till it becomes the only one used. Yet the accidental discovery of new possibilities also occurs, as it does in special sounds, like the lateral spirant *tenuis* of the Tibeto-Burman and the Palaeasiatic-American *L*, or the famous sounds of the Hottentot and Bushman languages, as is also true with the using of tones, the musical accentuation of complex starters, etc. The same is true of the methods of phonetic changes and specified starters such as affixes, or the rearrangement of the word order, etc.<sup>1</sup> Since in the possession of an ethnos there is a certain amount of various starters, they may be used for thousands of years without any essential change or with

<sup>1</sup>I think P. Rivet (*op. cit.*, p. 162) is incorrect when he says that for linguists "l'intérêt réside surtout dans l'étude des variations internes d'une même langue au cours des âges et des phénomènes généraux d'évolution," while an anthropologist (according to my terminology "ethnologist") "s'intéresse, au contraire, beaucoup plus aux mots qu'aux formes, au vocabulaire qu'à la grammaire. Les deux conceptions diffèrent exactement comme diffèrent en sciences naturelles celles du biologiste et du systématicien." The importance of the structure of a language and its phonetic

only slight variations. However, there are two permanently acting factors of changes; namely, the need of having new starters for new phenomena (variations of milieus) and the imitation of neighbours (varying interethnic milieu).<sup>1</sup> We have already seen that these conditions of existence of the ethnical unit are also responsible for the change of other elements of the secondary milieu. So, all that has been stated in this reference may also be applied to language. However, language forms only one of the elements of the secondary milieu, so that the first adaptation of language is that to this milieu. Since the secondary milieu usually changes, the amount of starters also changes in so far as new starters are needed for new elements about to be included into the old complex. Here it ought to be pointed out that the change of the secondary milieu is more likely to occur amongst ethnoses which are under the influence of a varying primary milieu, so that the greatest stability of the complex is observed only amongst the groups living in isolation and under very even climatic conditions.<sup>2</sup> The greatest influence on the changes has a particular adaptation in small groups of limited, or geographically and ethnically isolated, areas. This tendency has been indicated as one of the effects of the centripetal movements in the ethnoses. On the other hand, the adoption of new habits by a small group is easier than by a large group, especially when the latter has not created great cohesion, so that the effect of the centrifugal movement is also stronger. The new elements of the secondary milieu

variations is very great, indeed, for both structure and phonetics may directly affect the "lexic" contents of a language, and if the investigator confines himself to the idea of "words" (*mots*) [the latter as a phenomenon which may be regarded independently on other elements (and starters) forming a language], the essential sides of the linguistic complex may escape his attention, and bring him to a wrong conception as to the relationship between the languages; at least, that is what usually happens.

<sup>1</sup>It may be pointed out, for example, that the influence of changes due to children (emphasized by O. Jespersen) who imperfectly imitate adults is not a permanently acting factor of changes, although it may serve as a source of production of new or modified starters. Indeed, when a language is found in the process of readaptation all sources of changes are used. However, there are instances of preservation and sudden change of languages as well (cf., e.g., A. Meillet, "La Méthode comparative en linguistique historique," Chap. IV, where numerous instances are given), which facts point to the potential character of this source. The same is true of other sources of variations which must not be regarded as factors of changes.

<sup>2</sup>Perhaps this is the case in Polynesian groups.



are either created by the unit in the process of their adaptation or are borrowed from their neighbours. The same is true of the starters. As to the second permanent factor of change,—inter-ethnical pressure,—it is produced in different ways; e.g., the need of terms for interethnical relations, the perception of phenomena of interethnical milieu, which is gradually becoming more and more complex; and lastly, an adaptation to the periodically occurring lack of equilibrium between the centripetal and centrifugal movements. As to the sources of new starters, the unit may choose any source in any way suitable to it;<sup>1</sup> but as to borrowing from their neighbours, there are great limitations; namely, the element, especially the phonetic one, must suit the existing complex. What is actually observed amongst the living ethnoses and in their languages is that usually the unit elaborates its particular habits and complex sounds, adapting themselves and adapted by the existing "*artikulationbasis*" which are not alike amongst the ethnical units. So that a new, unknown sound has a certain chance of being adopted if it may be reproduced without great effort and without a change of the basis of articulation (*artikulationbasis*). If it requires a readaptation of the organs used for speech, it is more likely to be modified, owing to its adaptation to the existing basis of articulation. Thus the adoption of the new sounds is not an easy task. This does not occur when the basis of articulation of two groups are more or less alike—the borrowing is then easy; yet, through the borrowing of starters, the new sounds are also assimilated. Under this condition, the whole phonetic system may gradually be substituted and thus affect an original stock of starters. However, it does not mean that if the basis of articulation of two languages is different from each other no mutual or unilateral influence may occur. The whole system of adaptation for articulation may be changed. Owing to this, one may observe geographical areas covered by different languages, but possessing nearly the same phonetic system, side by side with cases where, in the midst of certain phonetic areas, islands

<sup>1</sup>The sources of new starters found within the units are numerous; e.g., sounds found in nature (primary milieu), in the phenomena of the secondary milieu, in individual invention, in children's imperfectness and new starters, accidental lapsus lingua, etc., the enumeration of which is not now important to us.

of special phonetic complexes<sup>1</sup> are preserved. The starters are also easily borrowed when phonetic differences are not great, and if the morphological characters of the languages are not too distinct. But in some cases the borrowing is altogether impossible. For instance, a long German "word" cannot be borrowed by the Chinese without being entirely modified and adapted. The long Tungus words cannot be adopted by the Chinese, as well as the long "sentence-like" Chukchi words cannot be borrowed by the Tungus, and so on. Yet the borrowing of Chinese starters with their characteristic musical content presents such great difficulties that the words borrowed by foreigners often remain unrecognized by the Chinese. The German borrowings of French starters are modified, as is true of most of the European languages, but in a much lesser degree than it is with the Chinese words. Yet the Mongol borrowings in some Tungus dialects,

<sup>1</sup>A. Meillet has always maintained the hypothesis that the phonetic elements are not borrowed, which idea is chiefly based upon the facts observed when loaned "words" are modified according to the local phonetic complex. He has explained it by another hypothesis; namely, that the acquired habits (*habitudes*) are transmitted through heredity ("*La Méthode*," *op. cit.*, p. 80). Both hypotheses, however, seem to be unnecessary, for in this case we have a simple phenomenon of adaptation of one complex to another, in which process some elements are better preserved than the others. Indeed, the mechanism of phonetic articulation of various organs is a physical phenomenon, for it is a function of the physical organs and the nervous system. The organs, and thus their potential functioning, are inherited, but the function as such is not transmitted through the mechanism of inheritance. Another question, whether the individual adaptation of organs (including the nervous system) is transmitted or not [A. Meillet postulates that they do not, for he understands "organs" as large anatomical units without taking into consideration a simple fact—the complex structure (even beyond the reach of the microscope) of certain organs which may change without affecting the morphology of large anatomical units, but which may essentially change various functions], is still discussed, but it cannot be treated here even superficially, for it will bring us too far. However, the "*habitudes*" are functional phenomena, and as such they cannot be inherited. This reference to biology is not incidental, for he sees something predestinated in language and particularly in phonetics. In showing that "*les habitudes linguistiques acquises se transmettent de génération en génération*," he makes observation that "*les enfants de parents connaissant bien plusieurs langues ou des enfants des parents bilingues seraient plus aptes à bien apprendre eux-mêmes des langues diverses que des enfants de parents parlant l'un et l'autre une seule langue et la même*" (*id.*, p. 110). Without speaking of the possibility of a simple case of selection, which in such a condition is common we may now refer to the remarks mentioned above regarding inheritance. It seems to me that when linguistic phenomena can be interpreted from the linguistic (and ethnological) point of view, it is much safer to remain on this ground. This hypothesis is needed by A. Meillet as a support for other hypotheses; namely, his supposition of the existing stability of phonetic elements, one of the backbones of the theory of common languages.



especially Manchu, can easily be recognized, for they are but slightly modified. A great difference in the phonetic system and structure of language may result in a complete isolation of language which will not influence nor be influenced by other languages. Indeed, the borrowing of starters will meet with great hindrance.

Special emphasis has been put on the difference between the grammatical forms of language and the vocabulary. The former ones, phonemes and morphological particles, as well as the order of "words," were supposed to be stable elements of the language which could not be easily borrowed, while the vocabulary was supposed to be a moving element of language. Such was A. Meillet's idea, especially in his earlier works (cf. "Linguistique historique et linguistique générale"), still maintained with a lesser emphasis. Other linguists, as, for example, J. Vendryes, bring forth a series of facts showing that, under the influence of what I call interethnic milieu, the "morphological" elements and order of "words" may change as well ("Le Langage," *op. cit.*, p. 341, *et seq.*). A number of analogous facts may be brought from the Asiatic languages too. Here we have a rather complex case, for the frequency of borrowing is not only proportional to the elements to be borrowed (the "morphological" elements as compared with the lexic elements are not numerous), but it is also conditioned by the fact of the existence of complexes which were postulated by A. Meillet as having almost absolute stability. Indeed, if there are two grammatical complexes entirely different, the chance of borrowing is greatly reduced; but if the difference is not great, the borrowing will go on very easily. In such a way, theoretically speaking, a certain grammatical complex may be gradually substituted by another one. As to the phonemes, their geographical distribution, for example, in Asia, is such that they seem to have their own areas, more or less independent on the languages spoken,—*"Chaque fait linguistique a ses limites propres,"* as A. Meillet says in reference to the dialectal vocabulary and which should be extended over other elements of language.

It may thus be generalized: *If the difference between the existing phonetic system, the method of producing new starters, the method of producing complex starters on the one hand, and those used by the neighbours on the other, is not great, the imitation and incorporation of alien elements into the existing complex will not meet with the opposition on the part of the unit; but if the difference is great, the complete*

*isolation of the language may be produced.* So that if the effort required for the adoption of new elements from their neighbours is higher than that required for the invention of needed equivalents, borrowing does not take place; but if they are equal, the chance of invention and borrowing is equal. As to the effort required, it is defined by numerous conditions, amongst which there may be mentioned, not only the physical condition of the adapted organs for the definite phonetic system, but also the degree of ethnical cohesion; that is, the equilibrium between centripetal and centrifugal movements within the given unit. It is thus evident that the problem of possibilities of borrowing is rather complex. There are some cases where borrowing is going on very easily, notwithstanding the physical difficulties; and there are even cases of complete self-isolation of the language. These phenomena sometimes cannot be understood from the linguistic point of view only, for their mechanism lies out of the range of the elements directly responsible for the existence of language.

Together with the quantitative growth of population, the change of the secondary milieu and the interethnic milieu, which are also in the process of increase, the language as a function is in the process of a continuous adaptation. During this process all available means are used, for this is essentially a function of adaptation along the line of least resistance. The complexes are built up gradually from various elements, regardless of their "origin"; so that in the large geographical areas, where the relations between the units are not physically restricted, where the interethnic pressure is intensive, and if, in addition to this, the area was several times covered by distinct migratory waves, the geographical distribution of elements (e. g., "words") seems to be independent on that of languages, just as it is often seen in other cultural phenomena.

If we now compare what has been formulated in reference to the cultural phenomena (the secondary milieu) in general with the characteristics of languages and their variations, we may see that the language is a direct product of the cultural complex. From this point of view, the nature of language does not differ from that of other cultural phenomena—it is a function.



## 13. Formation and Transmission of Linguistic Complexes

From a review of languages we may see that various methods of producing starters are used in different degrees. In some languages the phonetic methods receive greater application than the recombinations of existing starters; in some other languages the formation of accessory starters receives prevailing importance; and in some other languages the order of the starters appropriates the importance of the preferential method. But most of the existing languages use all methods; e. g., in the Chinese spoken language there are many starters which are used in accessory functions, like "affixes"; in the Mongol language A. D. Rudnev ("Material for the Dialects of Eastern Mongolia") has shown that the change of phonetic complex ("flexion") is practised; the method of the "word order" is used by nearly all languages, etc. Since we know from the history of some languages that in the course of time various methods may receive greater or smaller practical application, we cannot say what the "original" language was as to its preferential method. In fact, there are some languages which are changing their preferential method under the observation of linguists; yet there are some other languages which preserve the original method for centuries with no essential change. The condition which is implied by the study of facts is that it is impossible to say that there existed in former times<sup>1</sup> a single-sided adaptation in certain groups of languages unknown in other languages.

Indeed, the present character of existing languages is a result of the long adaptation lasting for thousands of years, and in most cases we have no evidences for suggesting what they originally were.

<sup>1</sup>To assert or even to suppose it is a dangerous hypothesis, for it implies other logical inferences without being itself an established fact. Yet, all the above-outlined methods are so simple that their existence in the languages of human predecessors is admissible as well. Then, the problem of their origin and sequence has to be brought from the field of facts to that of hypotheses. We do not know the needs of the ancestors of the early "unhuman" man, and we do not know what were their physical conditions for the production of sounds. However, we know that the man of the middle quaternary, who was not "man," did possess the complex idea of a soul (the practice of burial) and a well-developed brain. The lack of facts regarding his predecessors is not a reason for denying the possibility of a still earlier appearance of the need of a complex language. The attempts at the restoration of this complex in its "primitive form" is a mere speculation, satisfying a mind worried about the "unknown." Yet the reconstruction of the gradual discovery and prevailing methods in the formation of starters is also artificial and dangerous in its nature.

The preference for phonetic changes, or the using of accessory sounding starters, may originate, develop, and die out within short or long periods. Yet it may be preserved forever as a prevailing character. The same is true of the existing sounding starters. If there is no impulse of change, they may be preserved for hundreds or thousands of years; as, for instance, certain "gestures" which are known to big apes, monkeys,<sup>1</sup> and all human groups in the same "meaning" as typical starters. Since the appearance of the physical possibility of producing elementary varied sounds, certain combinations might receive general recognition and use, just the same as *coups de poing*, or fire making, and they may persist up to our day as well. It is impossible to have any hope of restoring them, for we know nothing as to the physical possibilities of sound production in the early ancestors of man. In the course of time they might change thousands of times. These reconstructions are not less artificial and dangerous than the reconstruction of the linguistic types.

There are starters preserved from "prehuman" ancestors, there are starters invented on various occasions and for various needs, there are starters borrowed from various sources: from the primary milieu—various sounds of "nature" and animals—and from the interethnic milieu. They are modified according to the easiest manner of their reproduction, or they are adopted as they are perceived. This complex is again modified under the influence of changing phonetic systems. The latter sometimes spread their elements over certain territory as any other ethnographical element which can be easily adopted when needed or desired. Sometimes the whole phonetic system is gained, little by little, by new-spreading fashions. Yet the methods of producing complex starters ("phrases," "sentences") may also change under the influence of neighbours who show a change of prevailing method. So the languages may also be composed of various phonetic and constructive methods, the origin of which may be traced back to different periods and sources. As an entity, it may persist for a very long time with no change at all, and it may also change any day. It may be borrowed as an entity and in its particular elements, and it may disappear altogether.

The fact that there are different ways of adaptation and preferential methods for the formation of starters is in agreement with

<sup>1</sup>Recent investigations on gorillas (Verkes) and chimpanzees. I have my own observations on monkeys.



the phenomenon of internal equilibrium of a once well-adapted ethnographical complex. Such a complex, owing to its utility, has a certain definite function in the whole cultural complex, so that, together with other complexes, it is transmitted to the succeeding generations again as a complex of elements, i. e., element by element, till the complex is ready for its function, as it is with other complexes of the secondary milieu. During the transmission, various conditions making the transmission of the complex more or less difficult are involved. Practical ways of learning and of teaching a language is one of the important features of the process of transmission of the complex. On the other hand, imitation by children and their ability of observation and reproduction is another important feature of the same function.

However, during the process of transmission, the complex may be slightly modified. The modifications constitute the history of language. But it is different when the complex is adapted by an ethnical unit which has previously lost the adopted complex. In such a case, the whole entity may be modified. The language is not thus the same, for it is modified; and it is not of indigenous origin, for it has been borrowed from another ethnical unit. When it is transmitted through a series of generations and a new generation comes into contact with the ethnical unit from whom the language has been borrowed, it may produce its influence, which will be an "alien" one, on the continuing language, and so on. Indeed, we may imagine an ideal case where a language has spread over a certain territory, together with the bearers; as, for instance, where a language spreads over territory with an ethnical unit which multiplies itself with great rapidity and occupies new territories, at the same time destroying other ethnical groups and gradually changing the phonetic system, structure, and basic elements of the language. But practically such cases are extremely rare. Yet the first question will be about the bearers of this language.

#### 14. Classification of Languages

The relationship between different languages in the eyes of observers is defined by their similarity. The latter is defined by the quantity of similar elements, not all of which produce an equal impression of importance. So, for instance, the similar structure of languages may be omitted from one's sight by the fact of the

phonetically different sounding starters, and the similarity of the phonetic system may be omitted from one's sight by the same dissimilarity. On the other hand, the similarity of starters, even with the condition of a different "structure" of language and a different phonetic system, may produce a greater impression than the differences. So, in this respect, the personal impression received by the observer, his personal ability of perception of sounds, etc., may be responsible for the intensity of impression of similarity. Yet the variety of impressions also depends upon the ideas of the observer. One may pay attention to the "verbs"; another, to the terms for "numerals"; and a third, to "cultural phenomena"; etc. But in all cases the chief idea is to find "similarity" and "dissimilarity."

However, an absolutely erroneous inference is often made from the facts of similarities; namely, that similarity indicates a "common origin." First of all, the similarity may be due to the convergence, as it is, for instance, with the phonetics, where the choice of possible elementary sounds is limited, and where, since the basis of articulation is the same (the variety of articulations is very limited, indeed), the similarity of elementary sounds is quite natural. The same may be stated in reference to the prevailing type of "structure" of language. In a lesser degree it may be referred to the "common" starters. Second, the similarity may be due to the spreading over of a certain territory of certain phonetic fashions, or the "structure of language," and especially starters. The number of common elements of these origins may cover from zero to the totality of the language. In the last case, two similar languages may produce the impression of being originally the same language, although both of them may arrive at a certain similarity by a gradual accumulation of elements from a third source. Such a common origin may exist, and it does exist, as the transmission of one complex from one to another ethnical unit or from one to another generation.

The classification of languages into groups is simply a method of grouping facts—complexes of language—into larger headings for better memorizing, and it must not imply conclusions as to the "origin" of these languages, for the similarity of languages as ethnographical complexes is not correlated with the origin of the complex as a whole. It is a function, and as such it can have no origin. Indeed, we can and must speak about the origin of a people as a physical body, but we cannot treat it in the same sense as the



origin of a language, which is actually a simple metaphor. But what has happened with the classification of languages according to their similarity is that an inference by analogy has been made: since they are similar, they have originated from a pra-language-ancestor.

The elements into which a language is divided may be analyzed and the "origin" of some of the elements may indeed be established. So the whole complex of language may be dissected into its elements and there will always remain some elements which cannot be connected with any of the other existing linguistic complexes. In the process of analysis one may find, in a group of languages, the elements *A*, which are connected with a known ethnographical and ethnical complex; the elements *B*, which may be connected with another complex; the elements *C*, which may be connected with a third complex; and finally there will remain elements *D*, which can be connected with no one of the existing nor the extinct languages. Will they be the original language?—Not at all. They will be merely unknown elements hypothetically referred to a certain ethnical unit, also hypothetical, and in hypothetical form. These elements *D* may be of the same various origin as are elements *A*, *B*, and *C*. Here, naturally, limits are put upon us by our knowledge of the "history" of language and the history of its bearers. At which moment shall we refer to the language as connected with a definite ethnical and ethnographical complex? It is an absolutely arbitrary choice, which, practically, is conditioned by other theoretical presumptions.

#### 15. Centripetal and Centrifugal Movements in Languages

Referring to the centripetal and centrifugal movements in ethnoses discussed in Chapter I, we may now state that these movements may have a direct influence on the language. Yet, since the language itself is one of varying elements, it may also have its bearing upon the intensity of movements. O. Jespersen, as linguist, has pointed out certain conditions which are essential to the change of languages. Some of his observations I shall now quote.

He points out that the splitting of languages amongst "primitive tribes" is greater than it is in "civilized countries," which is quite true in some cases, but the difference is not due to the "civilization" or the "primitiveness." As shown, the size of the units, whence

that of the linguistic units, is a function of the cohesion between the populations occupying a certain territory and the methods of adaptation. The instances of very "primitive" groups whose language is not "split" do exist; e.g., the Polynesian dialects are distributed over an enormous territory. Then there are, besides, a great diversity of languages; for example, as reported by travellers in reference to New Guinea, where nearly every few miles one finds a new language. For such diverse conditions various reasons may exist. One of them is the geographical position; another one may be the interrelations between the ethnoses, etc., without taking into account the historic formation of the ethnoses and other conditions which have nothing to do with the cultural state "civilized" and "savage," of common classification. A great "splitting" of languages is found in the most "civilized" western Europe, where ethnoses, although numerous, occupy very small territories.

With good reason and a near approach to the conception of *ethnos*, O. Jespersen points to "human geography, which is a decisive factor in the formation of dialects" ("Mankind," *op. cit.*, p. 42). In "human geography" he includes, for example, the differentiation of local groups due to the former administrative church divisions (in France).<sup>1</sup> In the same group of phenomena, one must include all distinct elements resulting from the centrifugal movement, a study of special importance which, being underestimated, is left without further discussion. He points out (*id.*, p. 43) the existence of "two opposing tendencies, the one in the direction of splitting, the other in the direction of larger and larger units." The discussion regarding the question of which tendency is stronger and which is weaker is rather interesting from the ethnographical point of view, for it reflects the behaviour of the authors, some of whom *do see* the centripetal movement

<sup>1</sup>A. Meillet ("La Méthode," *op. cit.*, pp. 55, 56), in reference to the ecclesiastic administrative divisions, points out that they were a continuation of the Roman administrative units, which in their turn had not been incidental to, but based upon, the consideration of existing relations amongst the people. Yet, in general, he does not give great importance to the political divisions as a factor influencing languages, but he sees deeper reasons for the existence of political divisions. No doubt, he is absolutely right in principle; however, there are cases when absolutely arbitrary divisions (like that between some states in North America) at last may produce their political effects upon the population in the sense of directing and enforcing centrifugal movement. Indeed, a political division as such is not an important factor, but still it is one of the elements having some weight in the system of centrifugal movement. Cf., also, J. Vendryes, *op. cit.*, p. 307, on influence of political division.



and some of whom do *not* see it, for some of them *approve* of it, while some of them *disapprove* of it. O. Jespersen finds that what I call here the centripetal movement as to the language is gaining a larger population (the formation of larger units) ("Mankind," *op. cit.*, p. 45).<sup>1</sup> This is, of course, a great overestimation of the geographical factor where O. Jespersen uses such an expression as "the local dialects purely conditioned by geographical factors" (*id.*, pp. 45, 46). As shown and pointed out by O. Jespersen himself, it is also characteristic of groups in which the geographical condition cannot be taken to be responsible.<sup>2</sup> As to the factors of the centripetal movement, to which

<sup>1</sup>This conclusion is one of those conclusions which are laid down as the theory of a future international and unique language for all mankind—an old and well-known theory, inspired by the desire of growing ethnoses who want to assimilate all "mankind," at least "civilized." But it is remarkable that this behaviour is also characteristic of ethnical units which are in the process of disintegration and whose leaders realize the impossibility of further keeping the old *modus vivendi*. Yet the same idea originates amongst the ethnical units which by this means may defend themselves against the assimilative movement. In fact, the adoption of an alien language does not yet mean a complete loss of ethnical independence, especially in the case where the differentiation of units operates in non-linguistical elements, e.g., "religious," "physical," "social," etc. So the same idea may be conditioned by various causes. A. Meillet, in reference to the European countries (*pays*), says: "Il y a là un état de choses nouveau et qui n'est pas susceptible de durer à la longue: la multiplication des 'langues communes' dans l'Europe d'aujourd'hui, en un temps où il y a au fond unité de civilisation matérielle et intellectuelle, est une anomalie" ("La Méthode," p. 20). Here we may note (1) the idea of unity of civilization, and (2) the conception of anomaly. The idea of unity is an organical conception, for we are allowed to speak only of the "seeming similarity." The idea of anomaly is rather interesting, for it reveals a new complex. In fact, either the state of things is in conflict with the observation of a great number of similar facts (e.g., the anomaly of embryological development), which is not the case, for the present situation in Europe is unique, having never occurred before, or it is in conflict with certain theoretical presumptions, which is just the case. A third supposition may be made; namely, that of the desire of a certain definite achievement, but it must not be, I believe, discussed in the case of A. Meillet. There is a simple rule that, if there are anomalies of such a type, the attention must be drawn to the revision of premises—in this case, the establishment of "norms," which ought to be scrutinized. As a matter of fact, what is now seen in Europe is well understood as absolutely "normal" effects of strong centripetal and centrifugal movements under intensively varying interethnical pressure. The case of O. Jespersen is different, for he wants to introduce an international language, which operation naturally must be "rationalized." Cf. also N. Marr's theory of pyramidal unification of language which is also a rationalization of a credo, but in this case, one may guess, professed *ex officio*.

<sup>2</sup>Reference to the geographical conditions is very often made when the other reasons of existence of phenomena cannot be easily understood. Indeed, it is a very soothing hypothesis: since geography is a responsible factor, the analyses and investigations may at least be postponed.

O. Jespersen pays special attention on expense of factors of the centrifugal movement, he points out (1) the war, which produces a mingling of population; (2) the annual market; (3) intermarriage;<sup>1</sup> (4) religion; (5) literature and the theatre, (6) political divisions; and (7) the formation of great towns. It is difficult to say why these factors have been selected from hundreds of other manifestations of the centripetal movement. Since there are mixed-up factors such as "war" (mobilization), which has but very small importance in particular cases of not yet completely formed ethnoses, and "political division," which alone may be responsible for both "splitting" and the formation of a "larger unit," one may see that the importance of these two movements escapes his attention. However, under O. Jespersen's penetrating analysis, the importance of the centripetal movement comes out quite clearly. Indeed, this has been done from the linguistical point of view. If one turns the problem around, i.e., what influence has language on the above-quoted seven factors, one may also see that without a previous ethnical total or partial fusion, they may have no place. In other words, language is an important factor in the process of ethnical differentiation (the formation of smaller ethnoses) and integration (the formation of larger ethnoses), but it cannot be taken alone to be responsible, and it cannot be isolated from the cultural complex and even supplied with magic power over all other manifestations of ethnical adaptation. Thousands of investigations may be written with the consideration of various influences over certain isolated phenomenon, the variations of which actually are conditioned by that of the nature of the ethnoses and particularly by the equilibrium of the centripetal and centrifugal movements. When the mechanism of these changes is not clear, the explanation of variations of certain isolated phenomena cannot be successful. Hence we have psychological, sociological, geographical, economical, mathematical, historical, and other approaches<sup>2</sup> to the minor problems, while the attention must be directed to the mechanism of changes. The description of all cases of gravitation is naturally impossible, just as the description of all

<sup>1</sup>It is interesting that this particular case has attracted the attention of many authors.

<sup>2</sup>I omit all cases of "socialistic," "communistic," "religious," "moral," and other ways of attacking the problems, which as such have naturally nothing to do with the science.



cases of cultural variations is also impossible, for we do not know what existed before, except a very short period of reliable records, and what new cases of variations will exist later.

Referring once more to the centrifugal movement, it may be pointed out that the language in this respect is an extremely sensitive phenomenon. In fact, the formation of new sounds is an individual invention, the formation of new complexes of starters is a very common phenomenon in particularized groups, as, for instance, specialized social groups. When the centripetal movement is not strong enough to oppose particularization, then the new dialect comes into existence. Since a new dialect is in the process of formation, it accumulates round itself new elements of the psychomental complex and the whole group of individuals may form a particularized group which limits its communications with other groups. In this way dialects and, further, other new ethnoses may be formed. Since this process of specialization of smaller units is one of vital importance for the existence of larger units, and since this process sometimes varies fast, the classification of dialects and ethnoses presents difficulties which cannot be overcome. As a matter of fact, the limits between the existing dialects and ethnoses sometimes cannot be detected at all—the old limits disappear, the new limits appear. This is a continuous process and its representation in a static form may bring the investigator to commit further mistakes. For instance, the limits between the dialects may happen to be in the process of disappearance, while other new limits are in the process of appearance. In the static treatment both will appear of equal value, while they are not so actually. It is naturally true, not only of dialects, but of all existing phenomena in ethnoses, cultural and biological in a narrow sense of the word.

#### 16. Language as Ethnographical Phenomenon in the Process of Ethnical Variations

The language as an ethnographical element of the ethnographical complex has the greatest importance in the process of the establishment of the ethnical and the interethnical equilibrium. We have seen that the centripetal and centrifugal movements and their equilibrium in the ethnos define either the process of consolidation

of the unit or its further disintegration into smaller ones. The mechanism of this process is rather complex, so it requires some additional remarks.

Let us take the instance of a well-adapted ethnos which multiplies very rapidly. It may spread over the territory if the latter is free; but it meets with opposition, if the territory is occupied. Under the pressure of ethnoses, the ethnos pressed may react in different manners. It may oppose the aggression of the neighbour by force, or it may come to a certain agreement which facilitates the existence of its numerous neighbours. So, for instance, it may come into close contact with other ethnoses through the establishment of economic co-operation, or to accept certain functions in a larger economic unit. Such is the case, for instance, when agricultural groups are co-operating with groups living on specialized industries. In certain geographical regions several ethnoses may be involved into such a co-operative system in which the interest of the survival of the ethnoses will be better assured than in an isolated existence. However, this situation bears elements of disappearance of ethnoses, for this is practically the first step for the extension of the former limits of one of the ethnoses involved. In fact, owing to the regular relations, a close contact is soon established and a great impediment for the relations—the language—becomes familiar to the co-operating ethnoses. Then a gradual substitution of one language by another may take place. When the language does not obstruct the influx of other ethnographical elements transmitted through the language, the ethnographical complexes blend together, or some better adapted elements of one complex substitute the elements of another complex. When the difference in language and ethnographical complex does not obstruct the approach of sexes, the process blending the two ethnoses is completed by the establishment of unregulated intermarriage between formerly distinct ethnoses. In this way the limits of biological process, and cultural, as well as linguistical, adaptation are extended, and a large, new ethnos is formed by the process of complete fusion. Naturally, the ethnos, physically better adapted to the given conditions, and being more numerous than the others, may gradually substitute its former rivals. This is one of the most common cases. The process of fusion of ethnoses and the loss of cultural independence is the most frequent occurrence. So the change of language is so frequent a



phenomenon that, for instance, A. Meillet, referring to the languages of the Mediterranean circle, says: "Il n'y a presque pas un peuple qui n'ait changé de langue au moins une fois, et généralement plus d'une fois" ("La Méthode," *op. cit.*, p. 72). Yet the same opinion is shared by most of the linguists who have been interested in the problem. It is quite natural, for the migrations of ethnoses are going on during the whole known history of mankind, and the change of cultural complexes<sup>1</sup> is also a fact which is closely correlated with the change of language, for the spread of large, powerful ethnoses and their new form of adaptation—"civilization"—involves neighbouring ethnoses.

From the analysis of correlation between language and anthropological types, we know that these aspects of ethnoses cannot be connected at all. Yet we also know that the change of the cultural complex may occur without any essential change of language except the increase of vocabulary. So if, in ethnoses, there are occurrences of coexistence of the same anthropological types, cultural phenomena, and languages, they are not indicative of a casual correlation between these phenomena. Such coincidences may be used as good historic evidences, but one cannot infer the common origin of populations from the fact of identic culture, and particularly language, and one cannot postulate the existence of a similar language and culture in general in the past amongst the ethnoses which at present are more or less alike from the physical point of view.

We have already seen that the change of language sometimes occurs partially, as it is with other cultural complexes, and the process of mixing and substitution of different anthropological types is also a common phenomenon. So the formation of ethnoses, also their agglomeration and disintegration, is such that the preservation of language, and generally the cultural complex, and an intact population in one and the same continuing ethnos may have only a theoretical interest, for such occurrences are practically unknown. From three different quarters—linguistical, ethnographical, and anthropological—we have the same ideas; namely,

<sup>1</sup>The connexion between the language and the cultural complex in general for some authors is so evident that they postulate it as a leading idea for further researches. A. Meillet ("La Méthode," *op. cit.*, p. 20) formulates it in reference to the common languages as follows: "Chacune des grandes 'langues communes' du passé doit exprimer un type de civilisation"; and, furthermore, "ce sera l'une des tâches de l'étude de l'homme dans l'avenir que de relier les langues communes aux aires de civilisation" (*id.*, p. 21)—an attempt which has already been made.

unreliability of all these evidences taken alone for showing the origin and the history of ethnoses.<sup>1</sup> It is now evident that it cannot be otherwise, for culture and particularly language are *functions* and *ethnos* is a process, also one of the functional phenomena, so that the actual relations which exist between these functions may not be discovered before we know the material substrata of all these functions, if such differentiated substrata exist. At the present time, we know practically very little about it and probably for this reason the functions are usually considered as evolving matter. Indeed, the ethnoses are concrete units—ethnic units—in so far as they are built up of populations, but as a unit they are functions, while the cultural elements have not even this appearance.

### 17. The Problem of Common Words

In reviewing the conditions of the existence of linguistical and other cultural phenomena, we have seen that the elements are grouped into complexes. The linguistical complexes, just as the economical, technical, and social ones, in the different ethnical groups may have a certain similarity. In a mind which is not armed with a knowledge of the nature of the complexes, the similarity is often mistaken for an indication of a common origin of complexes and even bearers. Referring to the language, we find that the evidences of "common origin" are found in "common words"; so that this problem is quite important, especially in view of the second part of this work where we shall deal with "common words."

I might confine myself in referring to the recent works on language, quoted here, also to many others which have not been quoted, and where the problem of common words and limitations in using them as evidences are perfectly well shown; but since, in the second part of this work, I shall have to refer to the various aspects of the problem of common words, I shall now point out some facts and conclusions. It is rather surprising (later on, we shall see that it is not so) that the analysis of the nature of common words and possibilities in using them as evidences do not reach many investigators who practically ignore what is known as to the nature of "common words." These general linguistical works remain a "pure theory," somewhat impractical.

<sup>1</sup>Still in a lesser degree, they are reliable for the problem of "nations," etc.



The common words met with in two and several languages may be grouped into two sets; namely, common words recognized as "common" and common words which are classed sometimes as loan-words, convergence, etc. So there is a very definite distinction between two groups, for "common" is very often referred to as "genetically common, of the same origin," an idea of which will be better understood when we analyse the complex of evolution and the idea of the organic character of language. However, since a "common" word may happen to be a "loan-word" for the given language, and since the "ancestor-language" is very often absolutely unknown, the labelling of "common" and "not-common" words becomes arbitrary. In a mind which postulates the common origin of languages as organical, evolving entities, the distinction of "common" and "loan-words" is of primary importance, so I shall start from the last type, that of common loan-words. However, since our chief goal is a discussion of the Ural-Altaic hypothesis, we must point out that we have no history of these languages (*vide infra*, Chapter IV) and we have no isoglosses, nor even sufficiently extensive dialectological works for all existing dialects.

It is not easy to give a short definition of a loan-word, because all words are to a certain moment "loaned" by the coming generations. But if we take it in a narrow sense, how may a "loan-word" be recognized actually as one? *A loan-word is one which has not been created by the given ethnos speaking a certain language, but which has been borrowed from another ethnos speaking a different language.* Naturally one always meets with the difficulty of definition, whether the word is a "loan-word" or not, for the starters are borrowed in great number, but the language which loans them disappears, so that if we do not know the history of the language in all its details we cannot say whether the given word is "loaned" or "created" or "received" from ancestors during the existence of the given language. Since historic documentation is lacking, yet the idea of distinction of loan-words from genuine ones is essential, other methods are brought forth for proving this distinction. I shall now point out some of these corroborative evidences. It is said that "the loan-word is one which in all details resembles that found in the other language." But here one needs a series of corroborative evidences, including the presence of a series of words "loaned" from the same language and historic data of that loan. These evidences are often lacking

altogether. It has been suggested that we must consider as genuine (not-loan) words all those which are found for a long time in the language. A. Meillet ("Linguistique," *op. cit.*, p. 103) gives the definition thus: "Les emprunts sont les éléments, pris à des parlers quelconques, et qui ne reposent pas sur une tradition continue." But how may it be recognized when we know no history of the language? This definition is supported by the following: "The loan-word is not one which is assimilated by the language in such a manner that it gives a series of derivatives." But we know foreign words which, during a short period,—less than one generation,—produce a series of derivatives and some other words which in this sense remain "loan-words" for ever, so that this evidence may become entirely misleading. And lastly, "the loan-words cannot be those which designate primary, elementary phenomena." This negative characteristic cannot hold good, for we need an exact definition of "primary" and "elementary," which is often impossible, and we know that such starters are sometimes loaned. Another negative characteristic, rarely used, is that "a word met with in several dialects cannot be a loan-word."<sup>1</sup> But in this case it must be shown that it was not loaned, for the lack of evidence is not indicative that the word was not borrowed by a group of languages from a common source, and in different times, and one language from another. A. Meillet, in his discussion with Schuchardt, points out that the psychological condition is essential in the problem. So he says: "Les sujets qui ont transmis les éléments indigènes ont eu constamment, d'une manière *plus ou moins* [italics are mine] nette, le sentiment et la volonté de parler leur langue traditionnelle" (*id.*, p. 104). He concludes that without this consideration "la doctrine classique me paraît . . . théoriquement insoutenable" (*loc. cit.*). This pessimistic conclusion is the last protection of the "doctrine classique." Further investigations into the psychological complexes have shown that they cannot be used as evidences—they are mere functional mechanisms. The processes and elements may be perceived and reactions may be produced by ethnoses (made of individuals), but they are not always correctly referred to, and the reactions are not always effective enough to be observed.

<sup>1</sup> In fact, it may not be so, for the loan might take place prior to the differentiation of languages in which it is found.



It is thus evident that the operation with "common" and "loan-words" is extremely dangerous, for "loan-words" may easily happen to be included into the misleading series of "common" words. The idea of "loan-word" is simple, however, if one does not give to it particular contents implied by the idea of organic evolution. The words are of different origin and those whose origin may be connected (not arbitrarily, of course) with corresponding words in other languages, yet those whose migration cannot be surely established, must not be included in the series of "common" words; and till the establishing of their history, they may be *conventionally* designated as words of so-and-so origin.

Such a treatment of "loan-words" certainly greatly affects the length of the list of "common" words, and still the actual loan-words may happen to have no traces of having been borrowed, and thus these series of common words may become misleading.

The common words may be different in origin; namely, they may originate from (1) a statistical phenomenon of convergence; (2) spreading in the territory of the certain phenomenon together with the term; (3) terms referred to the local phenomena, i.e., confined to certain territory; (4) words resulting from the imitation of various sounds (onomatopoeic); (5) words transmitted through the mechanism of imitation from one to another ethnical group; (6) words transmitted from one to another generation by the tradition and spreading over the territory together with the bearers. The quantitative side of the problem is also of importance, especially from the point of view of the origin of the commonness of words.

#### 18. Particular Cases of Common Words

Statistical phenomenon of convergence is common. In order to show this, let us suppose that we have stems consisting of one consonant and one vowel, and for producing a new starter, for a new conception, there is a choice of thirty consonants and ten vowels. The combination of these elements and their permutation offer six hundred different cases; so in this condition, the six hundred first case must be the same as one of the previous. However, the occurrence of similar syllables will take place much earlier, for certain combinations are not convenient for the given complex of articulation. Furthermore, the alternation of consonants is so frequent a phenomenon that they are usually compared by groups.

If we take the instance of the so-called Ural-Altaic language, the vowels, being uncertain, are omitted, and the consonants are actually grouped into labials, dentals, glottals, and also liquids and nasals. So the chance of occurrence of the same initial in the monosyllabic words used for the same "ideas" in two "genetically" distinct languages will be as one against five. The comparison of three languages reduces the chance of coincidence, but it will still be high. The combination of two syllables naturally will be more numerous than monosyllabic ones when compared in two "genetically" distinct languages. However, since, in the combination, there are only five elements, the chance of coincidence will be high, for there will be only twenty-five combinations. The chance of coincidence may be increased by the extending of the "meanings" of the stems—the larger the meaning, the greater the chance. When one gives a broad definition of the "meaning of stems" in a group of languages, there is no chance but to find phonetically similar words. Yet, even the fundamental five consonants and vowels are sometimes still reduced, as in the case, for instance, of G. Ramstedt's "law," treated in Part II of the present work, which formulates the alteration in different languages of labials→glottals→zero. The operation with monosyllabic stems in these conditions implies a meeting of similar words with an extended meaning in almost every case, at least in two of the several groups compared. Although the theory of probability and the theory of combinations and permutations might give a good warning against the comparison of monosyllabic words of distant languages, yet the experiments in finding them were very often made and are still practised in spite of the fact that the positive result, i.e., the finding of "common" words, proves nothing as to the common origin. Indeed, the same is true in reference to di-syllabic ones.

The spreading of ethnographical phenomena, together with the names over a certain territory, is quite a common phenomenon. They may belong to the so-called cultural phenomena; e.g., *araq* and its modifications are known from the Atlantic Ocean in Africa up to the Okhotsk Sea. It designates different kinds of alcoholic drinks made of various raw products. It is impossible to establish whether or not the element was originally migrating as in the case of "tea," or only the method of manufacturing the alcoholic drink was migrating. The word in question spread over different groups living in different



conditions, e.g., the agricultural groups of Asia Minor and the reindeer hunters of Siberia. The age of this word and its exact origin cannot be established. It is different with the binding of the phenomenon "tobacco," which spread over an enormous territory, together with its name (B. Laufer, "Tobacco and Its Use in Asia"). The number of such words is naturally numerous, for the elements of culture very often are spread from the centre of their invention; and if there is no great phonetic difficulties in adopting the name, it is adopted. In case the word is modified under the influence of particular phonetic conditions, it sometimes cannot be recognized at all. One cannot naturally say how many words have been modified and lost, and how numerous are the words the origin of which cannot be established, owing to their great age. Naturally, the finding of similar words proves nothing as to the origin of the different languages in which they are found.

There are large groups of words denoting local phenomena, such, for instance, as "tundra," "taiga," names for local winds and storms, names of local plants and animals, and also special methods and implements of hunting and fishing used for local animals in the given territory. These common words belong rather to the ethnographical complexes of geographical areas than to the ethnographical complexes of ethnical groups. The overlapping of these complexes is a well-known fact. In certain favourable conditions, the complex of the local terms may persist in spite of the changes in culture and even ethnical groups (bearers) and languages. As an indication of the genetic connexion of languages, these words show nothing.

The class of words of onomatopoeic origin is not, perhaps, as numerous as it was sometimes thought, but some cases are evident, as, for instance, the name "coocoo" and the like. Some languages are particularly inclined to produce words of this type. The Manchu language is very rich in different expressions (cf. I. Zaxarov, "Grammar of the Manchu Language") which in many cases are responsible for the origin of derivatives not found in other Tungus languages. However, sometimes the onomatopoeic interjection is not borrowed by the Northern Tungus, but the derivative (verbs and nouns) is borrowed. So in Tungus their origin is very confused. The same external sounding phenomena may produce similar sound-

expressions, yet they may be absolutely misleading as to an indication of "common origin."

The class of words transmitted through imitation is too well known to be discussed here in detail. If there is no great phonetic difficulty, any word which belongs to the influential ethnical unit may spread over other units substituting the old equivalents. The mechanism of their transmission is subject to a great variety, and different reasons for it may be found; e.g., simple fashions, imitation of "superiors," etc., not to speak of the needs of transactions. The imitation of single words may proceed from one group to another, so that the source of the origin may be altogether lost. This fact may be easily established in the case of neighbouring groups and the adoption of the voluminous complexes of words (e.g., anatomical terms in Latin) in some European languages; but in the case of single words it is sometimes very risky, even in the case where the origin is known but the transmitting links are lost. This class of words is not indicative of the "common origin," so these words may also become misleading.

The only group of words which can be called "common words" in the sense of the origin from a common language are words directly transmitted from one to another generation within the same group of people. How difficult it is to establish the common words and the operation with them is shown by A. Meillet (cf. "La Méthode," *op. cit.*, pp. 33-42), who says: "Le risque qu'un mot soit emprunté est toujours grand, et l'étymologiste, d'une langue ancienne ou récente, qui raisonne comme si les mots à expliquer avaient a priori toutes chances d'être indigènes s'expose à des erreurs fréquentes" (*id.*, p. 35). The situation with the Ural-Altaic languages is handicapped by another difficulty—the appearance of short stems and a lack of flexion, which "excluent, par leur structure, les démonstrations étymologiques rigoureuses. Il y aura là une méthode nouvelle à trouver si l'on veut parvenir à de véritables démonstrations" (*id.*, p. 39). Indeed, in the Indo-European languages the position of the linguist is rather simple, for he has the history of several "languages" (as complex) and the detailed history of populations; while, for most of the Ural-Altaic languages, the only linguistical corroborative evidence is a not-yet-completed dialectology and other cultural and anthropological evidences, the value of which is doubtful from a strict linguistical point of view, as that of A. Meillet and other theoreticians.



### 19. Correlation Between Ethnoses and Languages

When corroborative evidences are practically defective, one turns on 's eyes to the hypotheses of a very general character. In fact, when the word or language is spoken of as "common," it is presumed that the word is transmitted by the mechanism of direct transmission, and it is presumed that the people and the language are intimately connected. For this reason, the linguists are often looking for people, and when linguistic evidences are supported by other cultural evidences, and especially anthropological evidences, then the former are regarded as definite solids. However, this approach of the problem is erroneous in principle. First of all, the transmission of words from one generation to another is nothing but a form of "loan"; moreover, the language is not transmitted as a whole, but transmitted starter by starter, increased with the new elements or decreased, and very often changing from one generation to another. The complex is thus the same, so long as it is spoken without any change, i.e., perhaps less than during one generation. Second, the influx of alien elements through the adoption (inter-marriage with neighbouring groups, migration, etc.) may result in a complete substitution of one physical population by another genetically distinct, but the language may persist as well. The same is true with reference to the change of other elements of the cultural complex, which may be substituted by an alien complex, the language being preserved. It is, of course, easier when the ethnical unit, or a group of them, spreads over the territory and brings with it the linguistic complex transmitted from one generation to another. This is exactly the case which is before the eyes of most linguists when they speak about the common origin of words and languages. But this case is only one of the possibilities of "common words" and "common languages." This has been observed during the periods of great migrations of large masses, as happened in Europe and Asia during the centuries about the beginning of the present era, and as happened with the migrations of some groups, like the English-speaking people to North America, the Spanish- and Portuguese-speaking people to South America, the Russian-speaking people to Asia, etc. The life of the ethnical units who transmit their languages from one generation to another is not always disturbed by migrations, but the languages, as more or less similar

complexes, may spread through the mechanism of a total or partial imitation by the neighbouring alien groups formerly speaking entirely different languages. So there is nothing which connects the language with the physical bearers, except, perhaps, a certain physical adaptation to the phonetics and a certain psycho-mental complex in which the language forms but an element.

When a group of words and a certain complex of phonetics and methods of showing relationship between the sounding symbols is called by a certain name of a people, it is mere convention, useful merely as a method of classification, but not implying any conclusion regarding "origin" and an actual connexion between the people and the language. On the other hand, the language considered as an entity is also a convention, for no language may be considered as an isolated phenomenon beyond the whole ethnographical and even ethnical complex; but if it is so, it becomes a mere abstraction. Even when the basic contents of a language is well established and it is cleared of the elements enumerated in the first five sources of common words and "loan-words" (a condition absolutely theoretical, for it cannot be practically achieved), it is evident that the projection of a language into the past is an adventurous enterprise which cannot go further than an abstraction, which is dangerous when used as a scientific tool. If another step is made, namely, to connect a hypothetic language with the physical bearers, the chance of mistake is still greater. The search for a language which had been spoken by a certain definite people is certainly easier; for in the equation, out of two unknown, there is at least one known element. It is not surprising, therefore, that if the attempt to establish the place, people, and epoch at which hypothetic pra-languages existed can ever be restored without error, it might never have existed as a phenomenon connected with some physical bearers located in a definite territory. These attempts may be understood as by-products of the theory of the organic character of language and its evolution in the sense known from a modern European complex.

This does not mean, however, that no classification of languages and no attempts at finding hypothetic stems should be made. The classification is needed for further simplification of studies and memorizing facts. Yet the restoration of stems is also helpful, especially when they are used as any other ethnographical elements and with the necessary caution.



### CHAPTER III. THEORY OF EVOLUTION AND LANGUAGE

#### 20. Theory of Evolution and Languages

The function of language as described in previous sections is such in its nature that it cannot be considered as a phenomenon independent of other conditions of ethnoses. Yet, in a lesser degree still, it is possible to consider it as an entity controlled by internal laws of changes characteristic of languages only. Indeed, so far as the language is a function of human adaptation, it does not differ in its process of variation from any other secondary adaptive function. In spite of the great simplicity of the problem, as it stands, the current ideas on languages, their relations, and the laws of variations are extremely complicated, owing to an artificial approach; namely, the consideration of language as an organic entity subject to its evolution. We must here be detained on the term "evolution," which is not always understood as it should be. As a matter of fact, there are different evolutions. The attempt made by A. Lotka has led him to formulate,—“evolution is the history of a system undergoing irreversible changes” (“Elements of Physical Biology,” *op. cit.*, p. 24),—identified with the second law of thermodynamics (*op. cit.*, p. 26).<sup>1</sup> However, K. Pearson's definition of evolution is different: “A casual description of the appearance of successive stages in the history of a system forms a theory of the evolution of that system. If the theory be so satisfactory that it resumes in some simple statement the whole range of *organic change* [italics are mine], we term it the law of evolution.”<sup>2</sup> But even in this definition it is presumed that the system is of a physical nature. The older definitions, so far as they come from works of great biologists, lie within the same range of physical phenomena undergoing the process of irreversible transformations. The great misconception occurred when the idea of evolution was transferred to the functional phenomena. There is no doubt that when a physical system is in a process of transformation its functioning may also change; and, as a matter of fact, in some

<sup>1</sup>A. Lotka is not alone in thinking so. Nearly the same definition was given long ago by J. Chwolson (physicist), J. Perrin (chemist).

<sup>2</sup>K. Pearson, “Grammar of Science,” London, 1900, p. 375.

cases one may guess the process of evolution in looking at the change of function of the system. It cannot be stated, however, that the function will always and exactly reflect the changes which occurred. Without due attention to the nature of *evolution*, e.g., a succession of social changes may be mistaken for an evolutive process and developed (as, for instance, by H. Spencer), into a system of evolution of social (cultural) phenomena.

The theory of evolution has spread very intensively. It was first used as a simple metaphor, afterwards as a Spencerian theory, and later it was incorporated into the European ethnographical complex as an indispensable element. In this stage it has received such deep roots that no attempt at criticizing it has been tolerated in “scientific quarters.” Indeed, in this stage of its influence on the average European mind it began to threaten the very existence of science.

In the process of further variations, the idea of evolution brought us to another conception; namely, the organic character of cultural phenomena—e.g., evolution of social phenomena; since the social phenomena evolve and since the evolution is an organic process (as observed in organisms), the social phenomena are organic phenomena. One had only to disclose where this organism was and what was its morphology and functions. As a matter of fact, this very elementary logical fallacy inspired a great number of students who soon discovered morphology and functions in all cultural phenomena. Another form of success of this idea was that the organic idea of cultural phenomena without hesitation was transferred to the special field of language. The idea was so natural, so in the air, that it was not even verified. In the hands of non-biologists the idea of evolution when applied to the functions has also been modified in the sense that evolution was referred to as any change which was going on to a certain approved or desired end. With the Spencerian addition of the idea of progress, which, according to Spencer, did not essentially differ from that of evolution, it received a pseudoscientific appearance, and the establishment of the fact of “progress” became sufficient to infer an evolutive process and, furthermore, as in an organic phenomenon. Since the definition of “progress” is essentially subjective and emotive, the biological theory did not become a scientific tool, but a justification of human behaviour. Naturally, the original idea of evolution has several times been substituted by other ideas. The symbol “evolution”



referred to new referents; or, in my terminology and conception of language, the sounding starter "evolution" began to function as a starter of an entirely distinct series of conditioned reflexes; probably, owing to the lack of reflexes originally started by "evolution," and in some cases probably because of the suppression of some conditioned reflexes formerly used. Yet semantically, this is a case of change of meaning occurring in different groups of the same ethnical unit in dependence upon the difference of psycho-mental complexes characteristic of the groups. However, what was actually distinct, as compared with older conceptions, was not only the general idea of ever-changing phenomena, but that every change must be conditioned by the laws proper to the living organism, as a complex physical phenomenon. So we may now see that many "evolutions" exist which are entirely distinct, and the starter "evolution" must be used with great caution.

The success of the "idea of evolution" was not of equal duration in various groups. The refutation has come from specialists who, after having scrutinized their respective fields, could not disclose what had been presumed by the "evolution." However, the substitution of old ideas by new ones is long. One of the important hindrances to it are the ideas widely spread and assimilated by the ethnographical complexes, and also the modeling of a special vocabulary which is the only one in general and practical use. At last a reaction came, and students of cultural phenomena, particularly social phenomena, started a search for the "law of evolution" characteristic of their own field. In this search different methods were applied. At last, after a series of failures, "evolution" was declared to be a great misfortune for the science dealing with the cultural phenomena.<sup>1</sup> This movement, especially during the last thirty

<sup>1</sup>It is remarkable that the opposition comes in a very strong form, but it does not affect the whole complex. So, for instance, W. F. Ogburn, who has broken with the old school, in 1922 said: "The significance" (of the biological ideas and naturally the theory of evolution) "was so overshadowing that it seemed to cast something like a hypnotic spell over others doing research. The biological terminology was borrowed quite widely; and it became almost a fad to refer to biological causes" ("Social Change with Respect to Culture and Original Nature," p. 38). An analogous protest was formulated about the same period by R. Lowie ("Primitive Society," New York, 1923) and A. L. Kroeber ("Anthropology," New York, 1923), who denied the existence of "evolution." However the Spencerian organic and super-organic evolutions are developed (W. F. Ogburn), the "progress" figures (R. Lowie's works), and

years, also gained students of linguistics, but the change in the attitude of theoreticians did not at once reach specialists, so that the old conception of evolution still persists amongst some of them. It is especially true of the linguists who are specialized in different groups of languages. When one observes the contradictory attitude of theoreticians on the one hand, and the survival of old evolutionistic conceptions amongst the specialists on the other, the confusing picture of methodological instability appears in its actuality. The question as to why the theoreticians of linguistics are not immediately followed by the specialists in various branches may be answered by pointing to the fact of a very slow change of general ideas, especially those closely correlated with the existing ethnographical complexes. As a matter of fact, the idea of evolution and the organic point of view on language is one which has already long ago received general recognition. The terminology has already been modelled to suit this point of view and it is accepted long before the modern meaning of terms used in the circle of theoreticians becomes familiar to the student. However, the revision of terminology, which is practically much more difficult than a simple acceptance of a new theory, requires great effort and also a certain amount of imagination on the part of the reviser, and can be done only by the theoreticians themselves. It has not yet been done, so

the culture is sometimes regarded as an entity with certain internal mechanisms. (A. Kroeber's idea is to establish a "genetic classification" of linguistic "families.") The same is true of many great opponents of the old evolutionistic school. In one of his latest works, P. Boas, who is greatly responsible for the anti-evolutionistic movement (regarding cultural phenomena) in America, has formulated his attitude in this question, but still he wants to maintain the idea of progress as an addition to previous knowledge and as a phenomenon of social organization, so he says that "progress in social organization refers generally to a better adaptation to economic conditions and ethnical requirements as understood according to the general state of knowledge" (*op. cit.*, p. 103). Indeed, the "general state of knowledge," which itself is a very flexible ethnographical phenomenon and a "better adaptation," reduce the whole construction of a new ethnocentric point of view, which does not differ, methodologically, from the old conception of "progress." By the side of this opposition there are still fervent adepts of the theory of evolution who from time to time try to survive the old conception (e.g., H. A. Ellwood, "Cultural Evolution, etc.," London, 1927). Besides the fact of the persistence of this idea, a great source of difficulties in spreading new conceptions is the terminology itself, which is accepted with the whole existing complex, and implies the acceptance of ideas. Yet I think that before a new system satisfying modern minds is worked out in detail, the reversion to the old ideas is inevitable; for, as stated, they are in the air, i.e., in the European ethnographical complex.



the old terminology still persists in recent linguistic works. The terminology comprising *starters*, such as "family of languages," "evolution of sounds," "progress," etc., may be absolutely misleading, for most of these linguists do not recognize the organic and evolutive nature of languages, but use these "symbols" in referring to entirely different referents. In other words, they are sometimes merely misunderstood. On the other hand, the old ideas are still effective, even amongst theoreticians—linguists. Owing to this, the specialists of the particular linguistic groups usually operate with the old conceptions of organic evolution transplanted to the languages. Although formally they refer to the modern conceptions of language, they follow the old path in their investigations, the "theory" being used only on great occasions as a formal adhesion to the up-to-date movement.

#### 21. Logical Consequences of the Theory of Evolution Applied to Languages

One of the inevitable consequences of the theory of evolution applied to languages is the recognition of the existence of a relationship between the languages as there is between the progeny and ancestors of living organisms. Although the absurdity of such an absolute analogy with the organisms is evident for every one who is familiar with the languages, yet it is silently admitted that the relationship of the same type does exist. The reason is that no other theory exists sufficiently well developed, and especially there is no terminology generally adopted, to take the place of the old conception; while the latter is, so to say, unconsciously adopted by the youth in the schools, together with the present cultural complex of the so-called "civilized nations." It is already so deeply rooted in this complex that in public opinion those who do not agree with the "theory of evolution" are merely backward and uneducated persons.<sup>1</sup> The phenomena disturbing the scheme of linguistic relationship are looked upon as accidental conditions. The list of

<sup>1</sup>A. Meillet gives ("Linguistique," *op. cit.*, p. 102) an ethnographically interesting reason: "L'expression [parenté] est trop établie pour qu'on y renonce; il suffit de la définir pour n'en être pas dupe." His definition is that one must not understand *parenté* as a relationship between mother and daughter, but as that of a "transformation" of one language. This definition is only a paraphrase of an old idea chiefly in its psychological and historical aspects. So A. Meillet again says (*id.*, p. 81), "Ainsi la parenté de langues résulte uniquement de la continuité du sentiment de l'unité linguistique."

these disturbing factors, as, e.g., borrowing, phonetic "degeneration," the loss and mixture of language, etc., together with the increase of knowledge of facts, is becoming longer and longer. At last they come into conflict with the theory of relationship between the languages. The specialists have then either to abstain from undertaking any attempts at a further investigation of the problem of relationship—the attitude characteristic of several groups of linguists—and confine themselves to the minor problems, or to revise the fundamental problem of the nature of language, and thus they come to the same problem—the revision of the theory of evolution in its application to language.

The idea of the organic evolution of languages results in another important element; namely, the *recognition of the existence of a certain pra-language* which was the ancestor of a group of languages. Although the hypothesis as to the existence of such a language in some cases may practically be absolutely useless from the point of view of establishing a relationship between the languages and impossible to ever be shown, yet the search for a pra-language constitutes one of the important items of the linguists' work. If the facts disagree with this presumption, then they must be either unconsciously overlooked or discredited under any handy pretext which does not come into conflict with the given complex. The attempts at the construction of hypothetic pra-languages have been made for all linguistic groups, or "families." Indeed, the controversy between the reconstructors is inevitable, for the existence of such a pra-language itself is only a hypothesis. The connexion between a certain language and culture is one of fundamental postulates adopted by all who practise<sup>1</sup> the application of the organic evolutive principle in linguistics. Owing to this, the search for such a culture (civilization) constitutes one of the important items of reconstruction as made from the languages themselves.<sup>2</sup> Methodological fallacy is indeed evident.

<sup>1</sup>I say "practise," for in some instances the theory denying the connexion of language as organical entities is proved but not applied.

<sup>2</sup>Cf. *supra*, Chap. II, Sec. 16. A. Meillet in this respect goes very far, but he bases himself on combined data, —linguistical and historico-cultural.



Since the existence of pra-languages is presumed, there must also be the existence of some people who spoke it. This forms another inevitable consequence of the original conception. The search for the bearers of language naturally constitutes an important problem of the origin of language. The conflict between the creators of theories is also inevitable, for the existence of a pra-language itself is only a hypothesis. Here the reconstructor and the creator of theory as to the bearers of a pra-language bring forth a new series of facts, namely, other cultural elements and physical characteristics of hypothetical people. Both the ethnographical elements and the anthropological characters of the bearers of a pra-language are a mere hypothesis, for, as shown, the ethnographical elements and the anthropological features are subject to changes in the ethnical units owing to the complex processes of variation and substitution. The hypothesis of a pra-language is thus supported by the hypothesis of the bearers of the language and the hypothesis of the bearers is supported by the hypothesis of the continuity of the ethnographical complex and a presumption (again a hypothesis) of physical characters.

But this is not all. The hypothesis of the bearers of the language results in another consequence; namely, the location of the bearers. Here the vicious circle is closed, for the geographical area is again reconstructed from the reconstruction of a pra-language.<sup>1</sup>

<sup>1</sup>From the above remarks it must not be inferred that my idea is that no people, no culture, no territory, existed with which a certain language in its elements continuing in other languages was connected, but I want to point out that it ought to be first shown that such a language, people, culture, and territory did exist. The same holds good for the reconstruction of cultural complexes, which may not always be connected with definite languages. In the case of the Latin language, which is well known, as well as the history of the Roman nation, the anthropological characters of populations, and cultural variations, the problem of continuity in "Romanic culture" is relatively simple. It is more difficult with the Chinese written language (characters), for here we have a limited language complex confined to the written symbols and the historic anthropological data are not yet complete enough. However, such easy cases as their documental material are rare, and they are not sufficient for building up a general theory regarding laws of correlation, which may have a reverse force.

## 22. Phonetic and Morphological Consequences Implied by the Theory of Evolution

The recognition of the theory of evolution also results in a series of consequences regarding the "laws of evolution of phonetics and morphology," which, according to some current opinions, proceeds along certain definite directions conditioned by the laws which are good for any language, just as with the biological laws which are good for any animal or plant. Again, the establishing of such rigid laws comes very often into conflict with the facts, and the creators of laws have either to revise their fundamental conception of language or to reject the facts. In the soil of the European complex the idea was created that all languages have to go from a complex morphology to a simple one, e.g., from Latin to French or from old German to English. The Chinese language was thus supposed in former days to have had morphological elements which in the course of "evolution" have disappeared. The search for the supposedly lost morphological elements naturally constituted one of the puzzling problems, although their existence in written language was not at all necessary. This has raised up a great discussion which has no great importance if one does not presume the evolution along this line from complex to simple.<sup>1</sup> It is remarkable that another possibility, i.e., the complication of morphology—the fact observed—is often overlooked. Yet the conception of the evolution of language has recently brought to life a numerous group of linguists who restore the old theory with a new phraseology. Here I have in view the theory of evolution of language as professed by N. Marr. According to him, the stages of morphological evolution correspond exactly to the evolutive stages of the economical and social organization of society. In spite of the new phraseology and the seeming modernity,

<sup>1</sup>Although B. Karlgren recognizes that Chinese is a "monosyllabic" and "isolating" language (this is, of course, a theory which is not at the present time shared by all linguists), he points out the facts showing that this language formerly used and still uses the morphological elements. So he begins his paper ("Le Proto-chinois, langue flexionnelle," in *Journal Asiatique*, 1920) by the statement that "cette 'muraille' commence à être sérieusement ébranlée"; i.e., the Chinese language possessed a better developed morphology which has been lost and thus it was not so much different as compared with other languages. As a matter of fact, the "monosyllabic" and "isolating" character of the Chinese language as a theory is in crying contradiction with the facts known from the spoken language. (I do not here have in view the written Chinese language.)



this theory is as old as the earliest scheme of evolution of mankind proposed by L. Morgan.<sup>1</sup> This is a great step back in spite of its revolutionary appearance.<sup>2</sup>

The same kind of instance is found in the theories of phonetic changes. It was often supposed that the movement from *tenuis* to *media lenes* and the complete disappearance of consonants is one of the stable phenomena characteristic of the "evolution" of languages. The explanation of the existence of *tenuis* in the German language was even looked upon as of "Asiatic" influence, for other European languages do not have this phenomenon developed to such a degree. The terminology itself sometimes has a certain influence on linguists, as, for example, the association of the *tenuis* with "strong," "stark," "fortis," etc., in opposition to "weak," "schwach," etc.,<sup>3</sup> produces a complex of ideas regarding "degeneration" from "strength" to "weakness" deeply rooted in the mind of ethnical units.<sup>4</sup> Here is just one step to the theory of the deterioration of the phonetic system as a law; e. g., from *tenuis* to *media lenes* and from "roughness" to "refinement." The fact is, however, that the conceptions of

<sup>1</sup>This theory was picked up for political propaganda some forty years ago and has now reached large masses of population in western Europe. Yet lately it has been adopted for justification of the process undergone by Russia. The fate of L. Morgan's theory is particularly interesting, for it shows how long a time—more than two generations—is required for a scientific theory to reach the mass of population, and in this particular case through the political channel. On the other hand, this fact is interesting, for it shows how an old scientific theory, as that of L. Morgan, when incorporated into the ethnographical complex (folk-lore) and accepted by a government as its credo (together with the political theory) may survive in another scientific field—the linguistics, as shown in the above-mentioned case of N. Marr.

<sup>2</sup>Naturally, here I do not have in view the restoration of a group of languages, which by itself is a great scientific achievement, even in the case where some of the restorations will not appear valid in the future, but I have in view only the "philosophical background" of the japhetology, which, moreover, is not shared by all japhetologists.

<sup>3</sup>Cf. O. Jespersen ("Mankind," *op. cit.*, p. 211), where he discusses "la loi du plus fort," formulated by M. Grammont.

<sup>4</sup>The idea that the previous generation is better than the present one is characteristic of ethnical units in a state of slow variations. The leaders of the units and the creators of folk-lore, who are not usually the coming generation, but the passing generation, come into conflict with the younger generation and always maintain the idea that the older generations were stronger, more valiant, etc. This is one of the psychic elements serving to keep the complex and the high spirit of the units—the good example of the older generations ought to be imitated by the younger ones.

"strength" and "weakness," "roughness" and "refinement," are ethnographical conceptions closely correlated with the other elements of the ethnographical complexes and as such are subject to the variations.<sup>1</sup> The idea of a general phonetic "refinement" and simplification of "primitive methods" of morphology finds good support from the facts of semantics and vocabularies. The differentiation and the increase of the lexic complex are phenomena observed in all languages. It is often taken for granted that this is by itself a character of the language as a phenomenon. However, the semantic variations and the increase of vocabulary are mere functions of a quantitative growth of culture and perhaps in the end are accounted for by increase of population, so that their universality is not a character of language as a phenomenon.<sup>2</sup> However, the movement from paucity to richness of vocabulary is a process which may go on without affecting the system of phonetics and morphology.

### 23. Conception of "Progress"

The theory of evolution also implies another attitude on the part of searchers—the language, being considered as an organic<sup>3</sup> entity, must proceed in its evolution according to a certain definite way in which the increase of vocabulary is correlated with the "progress" in the phonetic and morphological systems. The "progress" is naturally understood according to the prevailing conceptions of "progress" in the given ethnographical milieu. It may be here noted that the analogy of the language with the organism and the

<sup>1</sup>E.g., the palatalization of consonants and the loss of a strong emission of air in one complex may be considered as a sign of "weakening" of the people; while in another case they may be regarded as a sign of "will power" and "self-control" of the people who are strong enough to show a "mild, pleasant pronunciation" and a strong implication of will when necessary. Generally, the ethnical units living under the pressure of the process of increase and positive impulses of variations give the interpretations of their peculiarities (characteristics) in the sense of strengthening the ethnical unity through the increase of love for the complex and consciousness of ethnical cohesion.

<sup>2</sup>The need of new starters in the case of the "wearing out" of the old ones is one of the conditions of producing new starters and semantic changes, but it occupies a very modest place by the side of the powerful impulse of lexic increase under pressure of a need of new terms for the cultural elements.

<sup>3</sup>Although the old theory is not recognized by linguists as their official credo, yet the idea is still living in the mind, for there is no other comprehensive conception of language, and as shown the idea of evolution is one which belongs to the ethnographical complex of European ethnical units.



analogy of the variation of the language with the evolution of living organisms are not referred to as the modern scientific conceptions of organism and the evolution of organism, but to these conceptions as they are understood amongst the laymen, as they are now reflected in the European ethnographical complex, i. e., in an extremely simplified manner. In fact, the treatment of the evolution of linguistic phenomena by analogy with the modern conception of evolution of living organisms might give a very distinct picture of the "development" of languages. But what is characteristic of the prevailing ideas is their correlation with the existing ethnographical complexes, whence the theory of "progress."

#### 24. Element of Tm)

There is one more point to be noted; namely, the idea that the differentiation of languages takes place in relatively recent time. The idea of time is naturally connected with the general advance of knowledge as to human history. First, the linguist had to place the process of evolution within a short period from the Tower of Babel to his own days. Later, the period was extended over a longer period, but still covering a very short period, and the linguist practically never attempted to extend it past historical times. The sight of linguists of the eighteenth century and of the beginning of the last century was limited by a period of a few thousand years. In the meantime, successful archaeological investigations have shown that mankind is at least several tens of thousands of years old. This fact has been usually omitted, for the chance of observing primitive languages has been greatly reduced. Yet the question was put and sometimes answered in the sense that man of the quaternary geological period did not speak at all. This was a hypothesis good only for soothing the mind and thus it discards the inconvenient facts of a possible great age of human speech. Further discoveries along the same line, of which the essential one is a further increase of man's age,—this time to hundreds of thousands of years,—were left unnoticed, better to say, ignored. The avenue of escape was again found in the suggestion that only the species *Homo sapiens* must be considered in the problem of language. However, anthropologists, such as M. Boule, have recognized that the man of the old Palæolithic might have speech, and since it has already been suggested, for example, by E. Smith, that the human ancestors,

such as the *Pithecanthropus erectus*, and other species of man might have a certain ability of speech, the problem of the "origin" of languages ought to be brought perhaps to the tertiary period and even to other animals, for these predecessors of *Homo sapiens* without having been "human" at all, were not, perhaps, speechless. Such a historic remoteness for the origin of language and such hopelessness in finding reliable documents as to "primitive" language were sufficient to discourage the linguists in finding scientific documentation of their theories, so the facts regarding the physical history of man have lost their interest in the eyes of linguists. On the other hand, owing to the shortness of the periods of seizable documents, the period left for "evolution" was thus limited and not by the lack of other documents, but by the lack of historic perspective.<sup>1</sup> In this respect, as in the case of the theory of evolution, most linguists have limited themselves by omitting facts of greatest importance for their own studies.

#### 25. Conception of "Primitive" Language

This idea is based upon the imaginary primitive state of our ancestors, but this conception is more an ethnographical phenomenon than a scientific fact. This idea was quite logical when it was not known how old mankind was or how old might the language be. The highest imagination supplied it with "primitive" consonants and "primitive" vowels, but still containing a very limited number of words for designating the most elementary conceptions. Yet attempts have been made at the reconstruction of primitive mentality, which, according to these reconstructions, looked rather like pure imagination, a distorted image of their creators and not actually existing realities. In fact, no one people may survive in the conditions of "savage" life with the philosophical conceptions and theories regarding the outer world with which "savages" are supplied. Linguists did not want to be behind these pseudo-scientific achievements and did their best to put in the mouth of hypothetic ancestors their own ideas about the primitivity of these ancestors. However, the accumulation of facts regarding non-European languages and facts regarding the history of the Indo-European languages have

<sup>1</sup>Cases like that of P. Rivet, who has a very broad view as to the historic remoteness of some linguistic and ethnical groups, are rather exceptional.



greatly changed the point of view of general linguists.<sup>1</sup> To the great disappointment of the protagonists of primitive languages, it has been found that the most "primitive" people, who do not know the use of metals, may have an extremely complex phonetic system and even a more developed dictionary in terms of vital importance, as, e.g., terms for hunting animals, topography, social institutions, etc. Then, when they retreated to the last rampart, it was suggested that abstract terms must be lacking. However, further investigations have shown that even this suggestion cannot be supported by facts,—the primitive people do possess terms for abstract conceptions, as well as a developed system of phonetics, structure, and a dictionary. The earliest Indo-European languages possessed no less a complexity than some of the most modern languages. However, the idea of finding some primitivity in the people who culturally differed did not leave the searchers, and their attention was turned to the fact that some supposedly primitive groups have languages morphologically highly differentiated, while some other groups of old civilization have languages with a relatively simple and, historically speaking, simplified morphology. It was easy to infer that primitive languages are "complex" and civilized languages are "simple." Other instances of the persistence of the idea of finding characteristics of "primitive" ideal languages may be seen in general linguistic treatises on the history of these theories. With these theoretical presumptions attempts have also been made at the restoration of languages, including the Altaic.

But how lexically simple and phonetically poor, and how complex morphologically might it be if it ever did exist? Of course, it could be as rich as any modern language, except for a special modern terminology which might be richer in some other respects, and it could be more simple from a morphological point of view than modern Turk, Mongol, or Tungus; yet the phonetic system might be something entirely different, for the sounds of a language are subject to variations, and we have no evidence for going further than mediæval Turk, Mongol, and Manchu, which did not differ very much in this respect from the languages spoken nowadays. One thing is evident—the looking for primitive conceptions in Altaic languages is a tribute to an ethnographical European conception

<sup>1</sup>This has been very definitely expressed by J. Vendryes.

regarding the people as a distinct complex and not a logical outcome of scientific inquiry.

## 26. Authors' Attitude and the Theory of Evolution

As I have already pointed out, the study of languages and generalizations drawn by the students of linguistics bring the facts into conflict with the prevailing theories, and especially with what may be called the spirit of epoch or the present ethnographical European complex. This conflict, as reflected by the reactions of investigators, may result (1) in the rejection of facts and their artificial adaptation to the theories; (2) in the discredit of old theories, particularly the theory of evolution; and even (3) in the total rejection of the pressure of the given ethnographical complex. All these reactions are commonly observed but not with the same frequency. First of all, the reaction of investigators depends upon complex conditions; namely, an accumulation of facts contradictory to the existing theories and contradictory to the given ethnographical complex; an individual susceptibility to the reactions; and lastly, an individual way of solving the problems. The fact of the quantitative increase of contradictory facts is naturally a function of knowledge quantitatively considered, so that it does not require a special interpretation, but the individual susceptibility to the reactions and individual ways of solution ought to be treated in a more detailed manner. There is no doubt that amongst investigators types of behaviour are very distinct. Roughly speaking, one may distinguish types of slow and prompt reactions; types inclined to adoption of new ideas, possessing a certain degree of flexibility, and types sticking to the existing ideas, showing a rigidity of the individual complex; types of aggressive complex and types of recessive complex; types of organizers and systematizers and types of critics and destructors; types blindly following the leaders and types with a strong critical spirit; types giving themselves over to the studies under the spirit of simple inquisitiveness and types giving themselves in view of certain benefits resulting from their profession; types of great vanity and types indifferent to it. One may distinguish some other types, but the above-outlined types will suffice to show how variable the reactions may be. In addition to this, it ought to be pointed out that the conditions of social organization in which the investigators are living and the degree of interethnic pressure



ought also to be taken into account. In some cases the individual reaction is checked up by the milieu in such a degree that it cannot be manifested at all. Here the degree of individual independence has the greatest importance. However, this side of the problem, owing to its great delicacy, cannot be discussed now, although in many a case it suffices for understanding the individual reactions, especially in milieus of high pressure, both ethnical and interethnical.

#### 27. Linguistics as One of the Elements of the European Ethnographical Complex

In the history of all other sciences we may observe the same situation. It is not thus characteristic of linguists alone. Conflicts are naturally more frequent in sciences which deal with phenomena, the laws of variation of which are not yet discovered. Particularly, the study of languages is in a special position, for as to the nature of language, that is, the material dealt with, linguists have not yet completely agreed. The success of the search for origin, evolution, and even relationship between the languages, cannot naturally be assured until the fundamental problems are solved. In spite of this, the interest in these problems does exist and the attempts at the solution of these problems are made and probably will be made, for which there are serious reasons. The reasons lie outside of the scientific researches and they cannot thus be checked up by the linguists themselves. First of all, the phenomenon of language attracts the attention of every one who meets with the difference of his own language as compared with that of other people; second, the language attracts the attention of people who realize the value of this cultural achievement; third, the difference in language is one of the fundamental conditions of differentiation of human groups, and thus it is an instrument of attraction and repulsion between the persons; fourth, the language has great political importance in the interethnical relations. With the growth of interethnical pressure and ethnical consciousness, the interest in the language naturally greatly increases. So that besides the general impulse of knowledge,—inquisitiveness,—there are many other impulses for linguistic studies. It may be noted, for instance, that after the last Great War the linguistic and general publications dealing with the problem of

language greatly increased in number.<sup>1</sup> Yet, the ethnical groups which are in the process of consolidation or in the process of extension as a rule pay great attention to their own language. For illustration, I will give some instances. Linguistical studies in Germany preceded the national unification in the nineteenth century. The great effort made in Finland and Hungary for study of the Finno-Ugrian group during the last quarter of the same century was stimulated by the national movement. The great interest in the language in China after the downfall of the empire is connected with the increase of ethnical consciousness and further efforts for the strengthening of the ethnical cohesion. Such instances may be multiplied ad libitum, but one of them is especially demonstrative. This is that of Russia. Prior to the Great War, the interest in the languages was cultivated by the government and scientific circles, so the studies progressed rapidly. However, after the collapse of the old government and the partial disintegration of the nation, the interest in the languages as a distinct ethnical character has enormously increased: hundreds of persons have shown their interest in research work along this particular line. This interest is still more stimulated, owing to the practical importance of linguistic studies in dealing with different ethnical groups whose "autonomy" is now recognized as a result of the partial disintegration of the former unit.<sup>2</sup> It is thus natural, whether the scientists are interested in the problems which cannot yet be solved or not, that the public attention to these problems cannot diminish, and if they refuse to discuss these problems, the same problems will be discussed by still less competent persons. So this movement is actually beyond control.

We have already seen that the linguistic researches are closely bound with the existing ethnographical complexes and the extension and deepening of knowledge is hindered by manifold difficulties of social, ethnical, and in a still greater degree of individual, personal

<sup>1</sup>Naturally their quality is not equal, for most of them were written for the special purpose of proving a certain proposition inspired by the political tastes and interests of the authors. Yet the professional linguists have also contributed to this political movement, e. g., A. Meillet in "Les Langues dans l'Europe nouvelle" (Paris, 1928), and under his and M. Cohen's edition "Les Langues du monde par un groupe de linguistes sous la direction de . . ." (Paris, 1924).

<sup>2</sup>Here we are naturally concerned with the increase of interest and stimuli and not with the actual success in the studies.



character. In spite of all these hindrances, studies into the problem of language and languages are going on as in other sciences which touch too closely the essential problems of the ethnical existence of units.

Our opinion as to what is "good" and what is "bad" in this process is of no importance, for we are now concerned only with the situation that is under our observation. It may be supposed that in the given conditions of science its organization probably requires not only new ideas and new works revealing the mechanisms of phenomena observed, but also a class of linguists contributing little to the actual knowledge of language and languages and slightly arresting, perhaps, a too rapid advance of studies. As a matter of fact, before the next step can be done, the most backward members of [the large family must digest what leaders have done. Naturally, popularization, the adaptation to the average mentality, takes a great part of the energy and time left at the disposal of actual creators of science. This seems to be a general phenomenon characteristic of the process of variation of ethnographical elements. Perhaps too rapid an accumulation of knowledge and too deep a penetration into the phenomena may result in the disrapture between the leaders and their large group of followers.<sup>1</sup> This is one of the curious mechanisms of the equilibrium of ethnographical complexes closely connected with the problem of tempo of variations and ethnical tension.

The psycho-mental state, as pictured in the lines above, is not, of course, characteristic of all linguists. The opposition to this complex is coming, as stated, from different quarters, but the prevailing ideas are still the same, for they are what is called "in the air"; they are still an essential part of the whole modern European complex in which the place of the individual will was occupied at a certain moment by the complex of evolution. So in order to understand the working mechanism of some scientific theories, one must go first into the problem of the ethnographical complex underlying these theories.

<sup>1</sup>However, the instances of disconnexion between the leaders and their followers are common in the history of science; e.g., the case of Mendel's theory is one which could be recently observed, but there are those which are too far from the existing mentality that they cannot even be detected, especially when the leader does not publish the results achieved and sometimes confines himself to oral communications with his nearest friends and colleagues.

## PART II

### THE URAL-ALTAIC HYPOTHESIS

#### CHAPTER IV. THE URAL-ALTAIC HYPOTHESIS AND TUNGUS MATERIAL

##### 28. The Theoretical Background of the Ural-Altaic Hypothesis

The hypothesis of common origin of the Altaic languages, and even the Ural-Altaic languages, dates from the first half of the last century. It has found many defenders and opponents. So the question in the opinion of outsiders is not settled at all. In fact, every one who tries to conciliate contradictory opinions and evidences, and to find a moderate, middle solution, is misled, for in this problem there is only one correct solution, while all other solutions will be proved sooner or later to be erroneous. We do not want to enter into discussion as to which group is wrong and which is right, for, as shown in Part I, the question of such a relationship between the languages cannot even be put to an answer, so that the evidences brought forth to prove or disprove, both limited in number and numerous, cannot be convincing either. So my approach of the problem is essentially different. The present part, devoted to the analysis of positive evidences supporting the idea of existence of a common origin of these languages, does not have in view the disproving of such an existence, but it has in view the showing of the process by which these evidences are produced and which linguistic value may have the parallels found.

I have taken the latest and most complete work by A. Sauvageot, who has used in his work a large number of previous publications dealing with the positive evidences for showing the "genetic" relationship between the Ural-Altaic languages. The controversy between the comparatists is great, indeed. There remain very few parallels which are not contested by one of them. I will not use this controversy for discrediting the idea and work—such a



discrediting as will be later seen has little interest even from the linguistic point of view.<sup>1</sup>

Although the work of A. Sauvageot, from the point of view of technique, as will be later demonstrated also, is not perhaps the best one, yet it has been approved and assisted by such authorities as Z. Gombocz and A. Meillet.<sup>2</sup> This may be considered as guarantee that the ideas developed are not discarded by these authorities and that A. Sauvageot is not alone in his position. Indeed, in principle his position does not differ from that of all recent contributors to the hypothesis of common Ural-Altaic languages, as, for example, P. Pelliot, P. P. Schmidt, and a large group of linguists headed by G. Ramstedt. Yet, historically, this idea began nearly a century ago with the works of M. A. Castrén, followed by a brilliant group of linguists of the same century, so that A. Sauvageot's study ought to be regarded as a collective work.

Let us now restore the process of building up this theory in so far as one may see from this and some other works previously published.

A. Sauvageot begins his credo by putting emphasis on the weak points of the theory, which are: (1) the paucity of documents as to the history of languages; (2) the impossibility of basing one's self on the vocalic harmony formerly supposed to be one of the fundamental characteristics of the Ural-Altaic languages; (3) the regularity and the simplicity of the structure of all these languages, which is not characteristic of these languages only; (4) the insufficiency of evidence of pronouns. These objections, however, fall down in the front of the new considerations; namely, (1) these languages are spoken and were spoken in a geographical area sharply defined, (2) the present grouping of these languages is not different from that of

<sup>1</sup>I do not want to miss this occasion for pointing out that discrediting criticism of each other's work and parallels produces an impression that one's work lacks a serious method. Why this is so will be better seen when the work of A. Sauvageot is analysed. It is inevitable, for it is the goal which implies the choice and treatment of the material and parallels.

<sup>2</sup>I believe that A. Meillet, who is also quoted in the preface by A. Sauvageot, is not directly responsible for the final form of the work here analysed. If one compares the last publications of this very cautious comparatist and general linguist (it is sufficient to remember the cases where his ideas have been quoted in the previous part) with the ideas and methods used by A. Sauvageot, one may see at once that these two authors occupy quite opposite wings of linguistic movement, perhaps it would be better to say different places in the marching column, at the head of which A. Meillet occupies one of the leading positions.

the most ancient time, (3) to the closeness in territory corresponds unity of structure of language, so he asserts that these considerations "portent à conclure qu'il y a eu unité de filiation dans le temps. Les ressemblances de structure interne qui caractérisent les langues ouralo-altaïques forment donc une présomption en faveur de leur unité généalogique" (*op. cit.*, p. xxi); and later on, after having reviewed contradictory data as to the vocalic harmony, the latter is stated to be present: "dans la plupart des langues de la Sibérie et de l'Europe orientale" and "ceci constitue une présomption des plus graves en faveur de leur parenté" (*op. cit.*, p. xxvi). Another reason is the alternation of consonants and vowels definitely established for some languages. (Here he has in view, amongst others, "la loi de Ramstedt," the loss of consonant *p* [φ] in Turk, Mongol, and Tungus, *op. cit.*, pp. 3, 4). At last, the only really reliable method of establishing the relationship between these languages, according to him, is the finding of common stems,—more exactly, the consonants for the vowels are not definite,—and the semantic value of words are considered (*op. cit.*, p. xxxvi). A. Sauvageot brings forth the significance of his labour in comparing a great number of words and he pretends "à mettre un peu d'ordre et de cohésion" and to look for "solutions précises," so he hopes that "son effort produira au moins ce résultat que la question de la parenté ouralo-altaïque ne fera plus désormais l'objet d'une polémique purement verbale, comme il arrive depuis plus de soixante dix ans. Les arguments précis que cet ouvrage présente au public réclament des critiques précises" (*op. cit.*, pp. xxxvii et seq.). After having examined two hundred and fourteen cases of parallels in the conclusion, he supposes that "des recherches ultérieures rendront certainement caduque la majeure partie de ces concordances. N'en subsisterait-il qu'une poignée, cela suffirait à prouver que les langues considérées ne sont pas étrangères les unes aux autres" (*op. cit.*, p. 139). The "laws" of alternations are shown, but in his eyes they have only a "statistical" meaning. Furthermore, since the usual way of refuting the common origin of words is their consideration as loan-words, he refers to the opinion of P. Pelliot ("Les Mots," *op. cit.*, pp. 255, 261) who discredits Altaic parallels from Chinese by P. P. Schmidt and G. Ramstedt (*op. cit.*, p. 255), and who believes that there is no reason for supposing the loan of words for common terms, as, for example, *bouillir*. A. Sauvageot supports him by his own remark, "Nous



nous refusons à croire qu'un peuple emprunte les mots qui expriment l'ensemble des notions primordiales" (*op. cit.*, p. 141). The definition of "loan" has been given in the Introduction; namely, "Quand un mot se trouve dans la plupart des dialectes ouraliens, turk, mongol, etc. . . . il est difficile de croire à un emprunt, ou s'il y a eu emprunt, cet emprunt a toutes les chances de s'être produit à une époque si ancienne que nous sommes en droit de considérer un mot de ce genre comme un vocable indigène" (*op. cit.*, pp. xxxi, xxxii).

With an evident satisfaction as to the results achieved, the author concludes by a remark concerning his senior colleagues: "Nous exprimerons donc en terminant le regret sincère que nous gardons de ne pas voir cette tâche menée à meilleure fin par un de nos éminents confrères en linguistique ouralo-altaïque" (*op. cit.*, p. 142).

## 29. Consequences Resulting from the Fundamental Propositions

From these quotations the whole system of work may be seen quite clearly. Referring to the previous general remarks and A. Sauvageot's direct indications, it may be formulated in the following propositions:

1. The languages may be grouped into the families (filiation, genetic unity) in which the relationship is defined by the fact of their having at a certain time a common ancestor—*pra-language*.

As shown from this presumption, logical consequences follow, which are:

2. There must be bearer-ancestors, which is naturally presumed. In the European complex they are usually called "Mongoloids," or something like that, although their existence has never been shown, and anthropological investigations point to a great variety of types and probably races in Asia.
3. There must be territory, which is already known. (Reference is made to the authority of K. Donner, p. xxi). Hence the value of identic structure, which is not convincing without other evidences, appropriates its full significance.
4. There is a unity of filiation in the Ural-Altaic languages and thus a genetic unity.

These propositions form the backbone of the hypothesis, which is supported by the following reasons:

5. Alternations of consonants and vowels regularly observed in some languages in the same words (stems) and particularly "la loi de Ramstedt."
6. Vocalic harmony, which is far from being a general phenomenon.
7. The parallels which are not numerous according to him and some of which may be rejected altogether.

The method is based upon:

8. Semantic and phonetic parallels.
9. The belief that the people do not loan words for expressing primary notions.
10. The presumption that the same words (stems) found in different languages are not loan-words (*emprunt*).
11. The presumption that if the source of the loan is not established the stem belongs to the given "language," either as an original word or as an old loan which becomes *indigène*. This is not formulated on any particular occasion, but it is a logical conclusion from the previous (9) and (10), and it is practically applied.

There are some other elements of secondary importance which form the same complex. Some of them will be occasionally mentioned in this part.<sup>1</sup>

The comparison of the above characteristics of A. Sauvageot's complex with those discussed in the previous chapter shows that the former do not essentially differ from the latter, the chief reason of which is the recognition of language as an evolving entity. The other three elements are logical consequences of the first one, and hypothetical, too, in their nature. However, all four hypotheses are supposed to support one another, which, from the methodological point of view, cannot be accepted. In fact, the bearers of the

<sup>1</sup>One of these elements is, for instance, the common civilization which follows as a consequence of the fundamental postulate: " 'Langue commune' suppose civilisation commune," as A. Meillet says ("La Méthode," *op. cit.*, p. 17), and "chacune des grandes 'langues communes' du passé doit exprimer un type de civilisation" (*id.*, p. 20), whence a geographical location and physical bearers of civilization and language are nothing but further logical conclusions.



pra-language, if it is a reality, are not found even hypothetically. These might be any of the groups, nowadays existing, or, even more than that, there might be a group now extinct. The anthropological evidences are not in favour of any of these hypotheses, for the population of Asia is mixed. The finding of bearers is hopeless, as has already shown to be the fact in the case of the pra-Indo-Europeans,—any one of existing "races"; i.e., Nordics, Mediterraneans, Alpines, or Dinarics might be the ancestors who spoke the hypothetic Indo-European pra-language.

The finding of territory is based upon lexic evidences. But every one may understand that, in the language of a people living in a certain area, one has a much greater chance to find common words for notions of local milieu and adaptation, especially if the migrations took place prior to the settlement of the people in the given geographical area. The chance of common words is still greater when different groups live for a long time side by side. Since this is so, this proposition must be supported by other evidences. The evidences are of a negative character,—recent (historic) migrations nor clear, definite historic documents pointing to this have not been found, so it is supposed that the groups lived from time immemorial in the same area, and nearly in the same distribution of groups. This is naturally mere guesswork adapted to the main hypothesis, for that there were great migrations in Asia is a fact now established. These hypotheses are supported by evidences of phonetics and again by common words which are hypothetic, and which need a detailed treatment.

### 30. Tungus Comparative Material

Since we are chiefly concerned with Tungus parallels, it will be useful to dwell here on the question of existing Tungus material. Some remarks are needed for showing what the character of the Tungus material is and how far it can be used. The Manchu language, which has attracted the attention of a great number of philologists and linguists, is the best-studied Tungus language. However, A. Sauvageot does not use all the material known—he uses H. G. von der Gabelentz's dictionary. His choice is not motivated. However, the existing Manchu dictionaries are not always reliable, and this is even so with I. Zaxarov's dictionary. The reason is very simple—all Manchurists used to study Manchu through the

medium of the Chinese language, or with the help of Manchus who did not possess this language as their native one. Thus it is seen that the "meaning" of words was not usually received in the Manchu complex, but through the Chinese complex, so that these linguistic materials ought to be regarded as reflecting Chinese complexes which are translated into Russian (Zaxarov), German (Gabelentz), and French (Amiot) in terms corresponding to the Chinese complex, and thus they are greatly distorted. The whole complexes, as, for example, the terminology of social organization, the terminology of shamanism, etc., are represented very often in their Chinese adaptation. As a matter of fact, none of these authors could do it otherwise, for they did not deal with the Manchus who spoke Manchu and thought in Manchu terms. I can illustrate it with hundreds of examples. Any one who will make a new attempt at remodelling these dictionaries without studying the Manchu ethnographical complex will be in the same position. So when the terms refer to the Manchu specific conceptions, one cannot rely upon the dictionaries known.<sup>1</sup>

Fortunately, we have good material for the restoration of the Nuichen language by W. Grube ("Die Sprache und Schrift der Jučen"), who has transcribed Nuichen words from the Chinese transcriptions. The possible defects are evident—the Chinese complex has produced its effect upon the phonetic system and "meaning." The same is true of A. Wylie's list of Nuichen words ("Translation of the Ts'ing Wan K'e Mung, etc.")

The other Tungus material is much poorer. W. Grube has made a heroic attempt at compiling a Goldi dictionary ("Goldisch-Deutsches Wörterverzeichnis") from various materials, both published and unpublished. The material is extremely heterogeneous. Besides the Goldi words, there are also included some words given

<sup>1</sup>I have felt it myself fairly strong when I was translating Manchu epic poems, shamanistic and other "religious" texts, i.e., the material essentially Manchu. It is easier, of course, with the Manchu translations from Chinese, where the meaning of Manchu words may be found from the Chinese text, but these "meanings" will be Chinese and not Manchu. The chance of correcting these dictionaries is decreasing every year, for the Manchu language is becoming a "dead language," even in the remote regions where it was spoken some twenty years ago. True, it is still preserved amongst some Northern Tungus and Dahurs, but one must remember when it passes through the Northern Tungus or Dahur complex, it is subject to modifications. So, practically, one must be very careful in using Manchu material.



in Manchu. Moreover, several dialects are also given. Since the material was not originally gathered by specialist-linguists, it is not always properly recorded from the phonetic point of view. Individual or occasional emphatic pronunciations are often mistaken for usual ones. In many instances the phonetic complex is greatly affected by being represented as reflected in the Russian phonetic system. The translations in W. Grube's dictionary have passed through two semantic complexes; namely, once through the Russian complex and the second time through the German. There are many instances of misunderstandings which have originated from this source. The translations into Russian even are not always exact when they are taken from the texts representing translations into Goldi from Russian (as shown in Protodiakonov's material); they are very often a mere adaptation of the nearest known words to the Russian complex. In many cases the translations of the original Goldi texts are entirely erroneous. W. Grube has very often shown parallels from other Tungus dialects, as well as Manchu. However, these parallels are very often defective, owing to the erroneous translations and erroneous transcriptions. The chief source of the Manchu comparative material was H. G. von der Gabelentz's dictionary. Moreover, the Goldi material covers only a small part of the existing lexic complex, so it is not at all complete. P. P. Schmidt, in his dictionaries, has used the Goldi material in a more or less corrected form. However, the corrections cannot always be regarded as sure records, for they are hypothetic in their nature. The Goldi language in its Olcha dialect has been studied by P. P. Schmidt, who has published a short dictionary. The using of the Goldi material requires, first of all, great caution and a thorough knowledge of published material and possible ways of distorting Tungus words.

Amongst the earlier publications, there are two publications of importance—a dictionary made by M. A. Castrén on the Urulga and Mankova dialects of Transbaikalia. These small dictionaries were recorded a few decades prior to the extinction of the Urulga dialect, at which time it was already saturated with Buriat words used parallel with the Tungus ones or already incorporated into the Urulga dialect. The records of M. A. Castrén are generally good, but in some instances the Tungus phonetic system is adjusted to the Buriat phonetic complex, as shown, for instance, in M. A. Castrén's preference for *ä*, etc. Another publication is that by A. Schiefner,

who presented the material gathered by A. Czekanowsky. This dictionary is not very extensive and naturally suffers from its incidental character and uncertain translations.

Some other collections of Tungus words of different dialects exist, gathered amongst various Tungus groups by T. von Middendorff, R. Maack, A. O. Ivanovskii, Baron Maydell, and others. These collections hardly cover a few hundred words.

In recent years there was a series of four small Tungus dictionaries published by P. P. Schmidt resulting from his own records and those made by incidental travellers and also completed from corrected and revised records previously published. There are presented: (1) the above-mentioned Olcha dialect; (2) the Oroči (with at least two dialects); (3) the Samagir and Samar (one of the Goldi groups); and (4) the Negidal (four dialects, according to P. P. Schmidt) altogether containing about five thousand words. The words are not always free of the suffixes that are not found with the translations; the translations in some cases are not exact which depends chiefly on the persons who gathered the material. Generally speaking, these dictionaries present an important source for Tungus comparative studies. Two more valuable publications, by E. I. Titov ("The Tungus-Russian Dictionary") and N. N. Poppe ("Material for the Study of the Tungus Language. The Dialect of the Barguzin Tungus"), are known. E. I. Titov has published an extensive collection of words (in many cases illustrated with texts) of the Tungus of Transbaikalia and the Irkutsk Government. It contains about thirty-two hundred and fifty words of four or five dialects, unequally represented. Apart from the technical defects, such as transcription and wrong translations, the great defect of this dictionary is that the author himself was affected by a strong influence of the non-Tungus complex, so that many translations reflects his own complex and not the Tungus complex.<sup>1</sup> So this dictionary in some cases may involve into involuntary mistakes and thus it cannot be used before being carefully revised.

<sup>1</sup>I have already pointed out this curious case (cf. "Social Organization of the Northern Tungus"), illustrating in which degree the author's complex if uncontrolled by criticism may affect the outcome of his work. It may be stated without exaggeration that this dictionary may suffice to restore the author's complex. Of course, to do so one must operate with the Tungus words translated with specific meanings or interpretations.



N. N. Poppe has published a collection of about twelve hundred words of the Nomad Tungus of Transbaikalia (Barguzin), which are well transcribed and usually well translated.

From the enumeration of the above-quoted publications one may see that the Tungus material is not numerous nor extensive. However, A. Sauvageot did not use all at his disposal, and in most cases confined himself to quoting parallels from W. Grube's Goldi dictionary.

Since I am going to quote various Tungus dialects not mentioned in the previous lines I must refer to my own and others' unpublished material. Besides about thirty thousand Northern Tungus "words" gathered by myself I have also some unpublished material from other persons and my extensive material regarding the Manchu spoken in the Aigun district. These data are under analysis and will be published, I hope, in due time. The enumeration of dialects and abbreviations are given at the end of this section.

The material published and unpublished which I have at my disposal is not yet sufficient for an exhaustive treatment of the Tungus lexic wealth. There are some dialects so little investigated that it is very hazardous to operate with them. Yet, as stated, the records are not always absolutely reliable. Is it not really premature to use such material for comparative purposes of great scientific responsibility?

In my work on the social organization of the Northern Tungus and others I have shown how ethnical units are formed amongst the Tungus. The distribution of dialects does not exactly correspond to that of the existing ethnical units (ethnoses). So, for instance, amongst the Birarčen, who are opposed to the Kumarčen, one may observe two dialects, one of which is so similar to that of the Kumarčen that it ought not to be regarded as a distinct dialect. Yet the dialect RTM does not differ very much from that spoken in the southern part of the Yakutsk Government. On the other hand, the Goldi language contains at least four dialects. There is no doubt that formerly the language spoken by the Kumarčen and the Khingan Tungus was the same; but the Khingan Tungus, under Mongol influence, have greatly changed their language, so that, compared with that of the Kumarčen, they regard it as their own and a different dialect. The Kumarčen agree with this distinction. The similarity between the Tungus dialects of certain regions, for

instance, northern Transbaikalia and western Manchuria (except RTM), is so great that the groups speaking them can easily understand one another; but the difference, for instance, between the Barguzin dialect and the Khingan dialect is so great that the groups speaking them can hardly understand one another. The dissimilarity between the dialects is still greater when one compares the Anadyr dialects recorded by Baron Maydell (in A. Schiefner's publication) or the Lam. and Tum. dialects with the Transbaikalian Mankova dialect. Yet some Northern Tungus dialects have been so greatly influenced by other languages that, for instance, the Oroči and especially the Goldi, they preserve only traces of their Northern Tungus characters; yet we know that many Tungus groups have changed their languages and have adopted the Yakut, Russian, Mongol, Chinese, and other languages. In spite of this situation, by eliminating the various foreign elements, one may trace a large group of dialects back to their original linguistical complex, which appears to be that of the Northern Tungus, who, with rare exceptions, call themselves *evenki*; and another group of dialects may be traced back to the Southern Tungus as their original linguistical complex. Yet the similarity between the Northern and the Southern groups is so great that after the elimination of the elements characteristic of these two groups, there remains a large number of common elements which may be regarded as pra-Tungus; i.e., as existing prior to the appearance of the Northern and Southern modifications. As to which elements have been lost by one of these two groups, and which elements have been appropriated (e.g., the system of suffixes) by them after the separation, is difficult to say, for we have no evidences as to the pra-Tungus language. The process in the formation of new dialects may now be observed amongst the Northern Tungus groups. This process at the present time is chiefly due to the various non-Tungus influences. If the group is isolated from other Tungus groups, and if it is in contact with some non-Tungus groups, it is very likely to be opposed to other groups under other and distinct influences. Under this condition, the formation of a new dialect is very likely, but it is not certain, for the language is sometimes preserved in spite of the essential changes in the cultural complex, as, for example, the economical system, "religion," etc. This is the case, for instance, with the settled groups of Birarčen and the hunter-horse-breeder Kumarčen.



Cases in which the complex is not changed, but the language is changed, are also known, as among the Mankova Tungus, who preserve their original language, and the Mongolized Tungus of Urulga, who now speak a "Mongol" dialect, but all of them preserve the same cultural complex. Thus, in the case of the Tungus the language and other ethnographical characteristics do not always correlate, as is also true of the anthropological types and various elements of culture. The latter may be more or less preserved, while the language may be lost altogether, and vice versa. The process of formation of dialects, ethnical units, cultural complexes, and anthropological amalgamations and crossed groups may proceed more or less independently one from another. I say more or less, for there is a certain, although very slight, correlation between the elements forming an ethnical complex.

For the convenience of my readers I shall now give a list of abbreviations which I shall use throughout the following pages:

#### A. THE NORTHERN TUNGUS DIALECTS ACCORDING TO THE PRESENT AUTHOR

- Amur. —the dialect of the Reindeer Tungus living in the eastern part of the Amur Government, also spoken by a Tungus group in Sakhalin Island
- Barg. —the dialect of the Reindeer Tungus of Transbaikalia living in the Barguzin *taiga*
- Bir. —the dialect of the Tungus of Manchuria living in the regions lying between the Amur, the Sungari, and the left tributaries of the Nonni (Birarčēn)
- Borz. —the dialect spoken by the Nomad Tungus living in south-eastern Transbaikalia, in the basin of the Borza
- Khin. —the dialect of the Tungus of Manchuria (Mongolia) living in the Khingan Mountains, chiefly in Hulun Buir
- Kum. —the dialect of the Tungus of Manchuria living in the basin of the Kumara, Panga, Albazixa, and Upper Nonni (Kumarčēn)
- Mank. —the dialect of the Nomad Tungus living in

south-eastern Transbaikalia, in the Mankova region (*volost*)

- Ner. —the dialect of the Reindeer Tungus (partly settled) living in the Nerchinsk *taiga*
- RTM. —the dialect of the Reindeer Tungus of Manchuria living in the basin of the Bystraia and Albazixa, and also the Upper Kumara

#### B. THE NORTHERN TUNGUS DIALECTS, ACCORDING TO UNPUBLISHED MATERIAL GATHERED BY OTHER INVESTIGATORS

- Enis. —the dialects of the Enissy Tungus gathered by the missionaries, brought by I. P. Tolmačev, and put at the author's disposal by W. L. Kotwicz
- Lam. —the dialect of the Reindeer Tungus of the Yakutsk Government included into *Lamunxinskii rod* (cf. S. K. Patkanov, "Essay on the Geographical and Statistical Distribution of the Tungus"), gathered and put at the author's disposal by P. V. Olenin
- Tum. —the dialect of the group mentioned above included in the *Tumunxanskii rod*; see Lam.

#### C. THE NORTHERN TUNGUS DIALECTS KNOWN FROM OTHER PUBLICATIONS

- Ang. Tit. —the dialect of the Reindeer Tungus of the Angara region in northern Transbaikalia, according to E. I. Titov
- Irk. Tit. —the dialect of the Tungus of Irkutsk Government, according to E. I. Titov
- Kal. Tit. —the dialect of the Reindeer Tungus living in the basin of the Kalar and Kalakan rivers, according to E. I. Titov
- Mank. Cast. —see Mank. above, according to M. A. Castrén
- Neg. Sch. —the dialect of the Negidals living in the basin of the Amgun, according to P. P. Schmidt
- Nomad Barg. Pop. —the dialect of the Nomad Tungus of the Barguzin district, according to N. N. Poppe



- Oroči —the dialect of the Oroči living in the Maritime Government, according to S. Leontovič, V. P. Margaritov, and P. P. Schmidt
- Solon (*Ivan.*) —the dialect spoken by the Solons of Manchuria, according to A. O. Ivanovskii

#### D. THE SOUTHERN TUNGUS LANGUAGES AND DIALECTS

- Manchu Sp. —the Manchu language as it is spoken by the Manchus of the Aigun district in Manchuria, partly in Peiping and in Sinkiang province, according to the author
- Manchu —writ. according to I. Zaxarov and partly according to the author
- Nuichen —according to W. Grube
- Goldi (*Grube*) (*Sch.*) —according to W. Grube and P. P. Schmidt

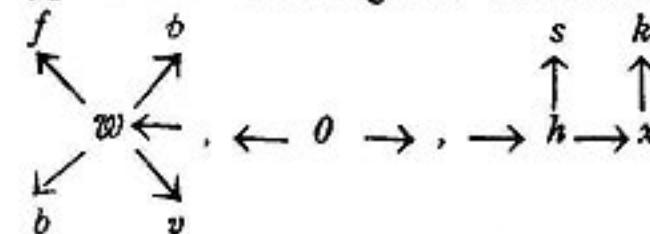
#### E. THE NON-TUNGUS LANGUAGES

- Mongol (*Podg.*) (*Rud.*) —according to I. A. Podgorbunskii and A. D. Rudnev
- Buriat (*Podg.*) (*Rud.*) —according to P. A. Podgorbunskii and A. D. Rudnev
- Dahur (*Ivan.*) —according to A. O. Ivanovskii
- Dahur —according to the author
- Yakut (*Pek.*) —according to E. K. Pekarskii
- Gilak —according to W. Grube
- Various abbreviations as they are found in A. Sauvageot's work.

#### 31. Aspiration and Bilabialization of the Initial Vowels a Phonetic Fashion

In the work of A. Sauvageot, one of the most important evidences in favour of the common origin of the Ural-Altaic languages is the phenomenon of the occurrence of initial bilabial consonants by the side of aspirated and non-aspirated vowels, also by side of glottal consonants. Similar facts have been observed in many linguistical groups, so this phenomenon is not characteristic of the languages called Ural-Altaic. So, for instance, N. Matsumoto, in his parallels, has shown a great number of instances where  $\theta$ ,  $h$ ,  $x$ ,  $f$ , and  $\phi$  are met with in the same words (stems) of different dialects of the Austro-Asiatic group. The Chinese dialects reveal the same picture.

Yet the Indo-European languages know this phenomenon as well. In the Tungus languages this phenomenon was noticed by A. Schiefner, L. Adam, and W. Grube, and was extended to the Mongol and the Turk by P. P. Schmidt;<sup>1</sup> but all of them did not go so far as G. Ramstedt, who supposed this phenomenon to have been a consequence of the transition from the bilabial spirant to the aspiration and zero, as  $\phi > h > \theta$ . A. Sauvageot has called this hypothesis "la loi de Ramstedt." Further considerations have made P. Pelliot suggest, instead of a spirant bilabial, a bilabial occlusive  $*p$ .<sup>2</sup> Indeed, it is only hypothesis backed by the presumption of the common origin of the Ural-Altaic languages, and in this respect it may be regarded as its by-product. In its formulation by G. Ramstedt, we have, perhaps, a case of mere "reasoning by analogy," implied by this hypothesis.<sup>3</sup> Although in the Indo-European languages the loss of the initial labial *tenuis* spirant has been recognized as an important moment in the formation of Armenian



and Celtic, however, there has also been, for instance, in Latin, the "consonantic reinforcement" of  $u > v$ , which is included by A. Meillet in the class of "accidents particuliers" as being in conflict with the above-mentioned "loss." I do not know whether the "loss" is a fact or a hypothesis, but the Latin "accident" is very instructive. I have made<sup>4</sup> a different approach to the same problem in Tungus by pointing out the existence of certain phonetic fashions which I have designated as the "aspiration" and the "bilabialization" of the initial vowels. The increase of incursion and emphasis may result, as they actually do in living dialects, in the formation of bilabial, labio-dental, and glottal consonants, both *tenuis* and *mediae lenes*, also both spirant and occlusive. So the scheme of variations is of the type shown in the accompanying table. Here the original sounds subject to variations are vowels and not consonants. Since

<sup>1</sup>In reference to the Mongol in his paper "Der Lautwandel im Mandschu und Mongolischen" (*Journal of the Peking Oriental Society*, Vol. IV, Peking, 1898) and in reference to the Olcha in a later publication on the Olcha language.

<sup>2</sup>Cf. "Les Mots, etc.," *op. cit.*

<sup>3</sup>He defines it as "bekanten allgemeinen lautentwicklungsgesetz" (*op. cit.*, p. 10).

<sup>4</sup>Cf. "La Méthode, etc.," *op. cit.*, p. 99.

<sup>5</sup>In my papers published in *Rocznik Orientalistyczny*, Vols. IV and VII.



these variations of sound may now be observed in working, as I have observed them in different Tungus dialects, I think the hypothesis of G. Ramstedt is quite unnecessary for the interpretation of the occurrence of *θ*, *h*, *x*, *p*, and *f* in Tungus and Mongol. The situation is complicated by the fact that similar variations are not universal; that is, there are cases when the parallelism is not complete at all. Another fact is that similar variations are known in other languages in which these variations may be due to different causes, including actual loss; also, as is observed in the Tungus, they may be due to the increase through the aspiration and the bilabialization of vowels. Owing to this, I cannot agree with A. Sauvageot's definition of G. Ramstedt's hypothesis as a well-established historic-phonetic law.<sup>1</sup>

Since the above-discussed hypothesis has already been promoted to the grade of law and put at the basis of the hypothesis of common origin of the Altaic languages and, furthermore, the Ural-Altaic languages, I shall, in addition to my former papers, bring

<sup>1</sup>One takes on one's self a great responsibility when one formulates new "laws" of science. First of all, the idea of "law" must be clear; for otherwise, if a certain tendency or a statistically frequent occurrence be taken for a "law," this would merely show how small is one's respect for scientific laws. I believe that G. Ramstedt did not for a moment suppose his hypothesis was a scientific law. Second, the persuasiveness of a hypothesis when the latter is called a "law" can increase only in the eyes of persons who are not familiar with the subject. In this way, such an abuse of scientific terms may become, for them, misleading. Not in a lesser degree one must disagree with A. Sauvageot, who styles his observations, on the regularity of certain linguistic phenomena, "les lois statistiques." That such an inaccuracy exists in the ideas of laymen and their language is a fact, but no one who is familiar with the terminology and scientific meaning of "statistics" and "statistical" would allow himself such an inaccuracy in a serious publication. It is far from me to take on myself a defense of purism, but such an abuse of terminology reveals the methodological side of the work discussed here, and as such it is worthy of attention. In fact, in A. Sauvageot's work one may observe some little (statistically) material which, when checked and recognized as reliable, may perhaps be used as good material for statistical analysis, but in his work there is no minor trace of statistical analysis. How can one speak of "statistical laws"? I point out this detail, for if the abuse of terminology continues it will bring further confusion into the simple problem of the Ural-Altaic languages. It is especially serious in the given case, for the work of A. Sauvageot is not an outcome of his own efforts only, but also that of his senior colleagues, so the abuse of terminology is not a mere *lapsus linguae*. Some authors suppose that the "linguistic laws" are not like other scientific laws—they cannot be accurate. But this is no reason why the term "law" should be applied to mere hypotheses and tendencies observed. What term, then, will be used when the actual laws are discovered?

forth some new facts and considerations, first in the field of Tungus languages.

In Tungus languages I have distinguished two phonetic movements and I have shown that the Southern Tungus, as represented by Manchu, is inclined to bilabialization, while the Northern Tungus is inclined, at least in some dialects, to aspiration. These are two distinct characters of two phonetic complexes correlated, shall I add by the way, with a complex of other phonetic peculiarities, as, for instance, the different behaviour in the palatalization of consonants, the transition to alveo-dental spirants, etc. Besides these two groups, we know several dialects which possess neither aspiration nor bilabialization of vowels. This phenomenon affects different stems too. I shall now give some instances.

1. Aspiration is found in the Goldi, while zero is found in the Northern Tungus and Manchu Writ.; and slight bilabialization in Manchu Sp.

a. *xagdu*, *hagdu* (Goldi, Grube), *agdun* (Bir.), *ukdun* (Manchu Writ.), *wugdun* (Manchu Sp.)—"the pit dwelling"; cf. *agdun* (RTM, Bir.)—"the bear's haunt"; corr. *avdun* (Khin., Ner.) (Irk. Tit.) in the same sense.

b. *hoškta* (Goldi, Grube), *ōšekta* (Bir.), *ōsikta* (Mank.), *ōšikta* (Ner.), *usixa* (Manchu Writ.), *wušiya* (Manchu Sp.)—"the star."

c. *xujun* (Goldi, Grube), *ujun* (Tum., Lam.), *ujun* (Manchu Writ.), *wujun* (Manchu Sp.)—"nine."<sup>2</sup>

2. Aspiration is found in Goldi and some Northern Tungus dialects; zero, in Manchu Writ. and Manchu Sp.

a. *ximana* (Goldi, Grube), *hemanda* (Tum.), *imanda* (Khin.), *emanda* (Barg., Ner.), *emana* (Bir., Kum.), etc. *rimani* (Manchu Writ.) [*yih-mâ-kih* (Nuich. Grube)]—"the snow."

b. *xupi* (Goldi, Grube), *ovi* (Bir., Khin., Ner., Barg., RTM),

<sup>1</sup>Amongst the Manchus, as well as amongst the Birarčēn, and Goldi this kind of dwelling is now built up in the form of a semi-underground house of Chinese type. Amongst the Northern Tungus living on hunting it is never used. According to tradition, this kind of dwelling was not originally of Chinese type. It seems to belong to the Palæasiatic complex.

<sup>2</sup>In most of the Northern Tungus dialects it is *jeyin* (Barg., Ner., Bir., Kum., Khin., RTM, Mank.), *johin*, *jahin*, *jehin* (Irk., Ang. Tit.), *jegin* (Neg. Sch.), *jagin* (Ur. Castr.) (cf. *juyen*, etc. [Buriat, Podg.], *jisun*, etc. [Mongol Rud.]).



*övi* (Neg., Sch.), *efimbi* (Manchu Writ.) *əvimbe* (Manchu Sp.)—"to play."

c. *xyrre* (Goldi, Grube), *èrèki* (Bir.), etc., *erxi* (Manchu Writ.), *èryè* (Manchu Sp.)—"the frog."

3. Aspiration is found in Goldi; aspiration and zero, in Northern Tungus; bilabialization, in Manchu.

*xukte* (Goldi, Grube), *ikta* (Bir., Kum., Khin., Ner., Barg., Mank.), *veixe* (Manchu Writ.), *vèyè* (Manchu Sp.)—"the tooth."

4. Zero is found in Goldi, Northern Tungus, and Manchu Writ.; bilabialization, in Manchu Sp.

*olg'é* (Goldi, Grube), *ulg'an* (Bir.), *ulg'an* (Manchu Writ.), *wulgjan* (Manchu Sp.) [cf. *wúh-lì-yên* (Nuich. Grube)]—"the pig."

5. Zero is found in Goldi and Northern Tungus; bilabialization, in Manchu.

*igá* (Goldi, Grube), *irga* (Ner., Barg., Bir., Kum., Mank., RTM), *fexi* (Manchu Writ.)—"the brain."

6. Zero is found in Goldi; aspiration, in Northern Tungus; zero, in Manchu.

*öri* (Ol. Sch.), *xəuri* (Tum.), *ergembi* (Manchu Writ.)—"to take a breath."

The instances of these six combinations may be increased, almost ad libitum, especially in the cases where Goldi has aspiration. The conclusions which may be safely drawn from these facts are—(1) Goldi is a language where the aspiration is known; (2) the words bilabialized in Manchu do not always have the initial *p* in Goldi, and they can be both aspirated and non-aspirated; (3) the words not bilabialized in Manchu may be found to be aspirated in Goldi; (4) the words aspirated in Goldi may be found aspirated and non-aspirated in Northern Tungus and bilabialized and non-bilabialized in Manchu.

Let us now proceed to the initial *p* and other labial consonants in Goldi and other languages. Since these examples are known from other publications I will not give instances here.

<sup>1</sup>That Manchu *-xi* corresponds to Tungus *-rgi* and Goldi *-ga* can be supported by other evidences. These are not suffixes. In Manchu Sp. we may expect to have *jéye*, or even *vèyè*. I have not happened to find this word in my records.

1. Goldi *p* may correspond to the Manchu *f* in words unknown in Northern Tungus and refer chiefly to the Manchu cultural phenomena (about one third).
2. Goldi *p* may correspond to the Manchu *f* in words known in Northern Tungus with the initial aspiration or zero (more than one third).
3. Goldi *p* may correspond to the aspiration in Northern Tungus (only five cases known by me).
4. Goldi *p* may correspond to the Manchu *b* (I have noticed only two cases).
5. Goldi *f* sometimes corresponds to the Manchu *f* and usually to Goldi doublets with *p* and the same meaning (very rare cases).
6. Goldi *w* and *v* may correspond in rare cases to the Manchu *v* and *f*, to the Goldi *u* (in doublets) and different sounds in Northern Tungus dialects, e.g., *η*, *b*, especially in words unknown in Northern Tungus and designating local terms.
7. Goldi *b* may correspond to the Manchu and Northern Tungus *b*; it occurs frequently so that there are only a few exceptions corresponding to other sounds in Manchu and other Tungus dialects; in Manchu the initial *b* is almost voiceless.

These series of facts allow us to generalize: (1) the Goldi language shows a definite inclination to *tenuis* bilabial occlusive (*p*) occurring in cases where it is met in Manchu with spirant *f* and in rare cases *b* corresponding to the Manchu Sp. *p*; (2) the Goldi *p* in a limited number of cases corresponds to the aspiration and zero in the Northern Tungus dialects; (3) the Goldi *p* in a great number of cases corresponds to the Manchu *f* and to the Northern Tungus aspiration and zero; (4) the Goldi *f* is rare and is never met with in words common only to the Northern Tungus, but it is met with in words in Manchu found with *f*, and sometimes in Goldi with *p* (doublets); (5) the Goldi *w* and *v* are rare and correspond to the Manchu *w*, *v*, and *f*, also the Goldi *u* (bilabialization in doublets); (6) the Goldi *b* is met with as frequently as in other Northern Tungus dialects and in Manchu Writ.

The reaction of the Northern Tungus dialects is such that they alter the initial *f* of foreign words into *p*, for the initial *f* does not exist in the Northern Tungus dialects with the exception of cases like that of the Birarçen dialect where I find three words, all borrowed from Manchu, with the initial *f*. However, in this form



they are used only by persons familiar with the Manchu phonetic peculiarities.

It should be pointed out that the process of bilabialization of the initial vowels is a phenomenon observed in foreign words with the initial vowels borrowed by the Manchus. By the bilabialization are affected first the words with the labialized vowels. Yet the Manchu Sp. is more bilabialized than the Manchu Writ. In fact, all *u*'s and the greatest part of the *o*'s of the Manchu Writ. in Manchu spoken are strongly "bilabialized." That the process of bilabialization is not a very recent phenomenon one may see from the instances of Nuichen and several evidences of formation of new labial and bilabial consonants from the labialized and even non-labialized vowels. Of course, one may reject the last series of facts by explaining them as a result of the loss of consonants, but the fact of recent bilabialization (Manchu Writ. and Sp.) of foreign words and the fact of the process of further bilabialization in Manchu are reasons to incline us to see in the former process a positive movement of increase of consonants and not their loss.

As to the aspiration and further formation of glottal consonants in Northern Tungus, we may observe this phenomenon in the case of "loan-words." So we have, *xuto* (Ang. *Tit.*), *hutō* (Nomad Barg., *Pop.*)—"the tinder," cf. *uṭa* of the Buriats; *hek* (Transb. *Tit.*)—"ex!" from Russian; *hikin* (Tum.)—"the caw," "ox"; cf. *ixan* of Manchu; (it may be noted that in Nuichen the *i* is bilabialized—*wei-han* (*Grube*) (restored by P. Pelliot [*op. cit.*, p. 240] as *vihan*), I might also quote the case of *xorin* (Goldi) corresponding to *orin* (Manchu and Mongol), but I do not want to do it, for in Mongol it is sometimes met with as *xorin* (cf. Dahur, *Ivan.*; Mongol, *Rud.*), and in this form it might be received by the Goldi direct from these sources. However, in Nuichen it is *wo-lin* (*Grube*) (i.e., \**worin*). Indeed, the number of foreign words with the initial vowels aspirated in the Tungus dialects is limited: for (1) not all words "need" to be aspirated; (2) the Tungus usually become familiar with the foreign phonetic systems (cf. the above-quoted case of the Birarčēn *f*), especially in the case of the "non-aspiration" of vowels. The aspiration affects different words in different dialects. This has already been shown in the instances of Goldi aspiration. I have already suggested in my previous publication that the aspiration is correlated with the existence of expiratory and musical accents and length of vowels.

So that the aspiration may be regarded as an unconscious process of "preservation" of vowels, which method functionally corresponds to the accentuation of vowels. I may here give an interesting instance from the Goldi, where we find *xi* (Goldi, *Grube*) corr. to *usi*, *uhi*, *uxi* (of various Tungus dialects), *uše* (Manchu Writ.), *wuže* (Manchu Sp.)—"the thong." The variations of Goldi *xi* may be supposed to be as *usi*→*uši*→*uhi*→*uxi*→perhaps *wxi*→*xi*. This instance shows what may happen if the initial vowel is not "protected" by the aspiration and another vowel is accentuated, as it is in some Northern Tungus dialects.

### 32. The Goldi Language Not a Typical Northern Tungus, Neither a Southern Tungus

I have taken the instance of the Goldi language as a basis for the discussion of the phenomenon of aspiration and bilabialization for two reasons; namely, the Goldi language shows a series of both aspiration and bilabialization of vowels and the Goldi language is always quoted as one in which the hypothetical \**p* is preserved. The aspiration and the bilabialization are so common in Goldi that they are observed in the same words; e.g., *pálga*~*halga*—"the foot"; *pému*~*ximma*—"the lip"; *porro*~*xárrake*—"auerhahn"; *polo*~*xolo*—"the trembling poplar." Particularly in the Ussuri Goldi dialect, the aspiration seems to be stabilized and has *x* instead of *p*. The stabilization of the aspiration is characteristic of Goldi in general. It may be here noted that in good agreement with other Tungus languages affected by the aspiration, the practice of alteration of *s* into *h* and even *x* is very common in Goldi. On the other hand, the presence of the labio-dental *tenuis* spirant *f* and the bilabial *media lenis* spirant *w* is more frequent than in any other Northern Tungus dialect besides the occlusive *p*. These facts point to the meeting of some strong current of words with the initial vowels bilabialized and not altered into *p* with another current of alteration of the initial *f* into *p* in foreign words. Thus, from the phonetic point of view, this language is a mixed one. I say intentionally "mixed," for two phonetic currents of different origin are met with which are clear from the conclusions regarding the Goldi, Manchu, and Northern Tungus languages.

The analysis of the vocabulary reveals the same picture. By the side of the typical Northern Tungus elements there is a large number of typical Southern Tungus elements (Manchu and perhaps Nuichen)



used parallel with the Northern Tungus words, but already modified according to the type of "aspirating" dialects. Besides these modified Southern Tungus elements there are many "loan-words" which have not yet been modified. Furthermore, this language includes a great number of words which are neither Southern nor Northern Tungus. Some of them, but not all, may be connected with the Gilak words. It ought to be noted that most of these mysterious words designate local phenomena, local animals and plants, special methods of fishing, etc.

The morphology of these dialects is also sometimes mixed with two systems—the Southern and Northern Tungus. In fact, the specimens gathered amongst the Sungari Goldi reveals a parallelism in using, for example, the Manchu suffix *-xa* (part. perf.) along with the Northern Tungus *-če*; accumulation of several suffixes for expressing complex relations (complex starters), as, e.g., *jatka + -či + -du + i*, which is typical of the Northern Tungus dialects; the verbal pronominal suffixes are partly preserved; the Northern Tungus suffix *-du* (*locativus* and *dativus*) is used instead of the Southern Tungus *-de*; *wō* (*accusativus*) instead of *be*, etc.; the pronominal declension is preserved.<sup>1</sup> On the other hand, the Olcha dialect<sup>2</sup> has no such typical Northern Tungus elements and generally morphologically it is much poorer than the Sungari dialect. Some differences are also seen when the Ussuri and Amur dialects are compared with the above-mentioned two other dialects.

The facts given above may suffice for stating that (1) the Goldi language cannot be considered as an entity; (2) it is a group of dialects in various degrees containing Southern and Northern Tungus elements, phonetic, lexic, and morphological; (3) it must be classified as a "mixed" language.<sup>3</sup> The history of the formation of the Goldi as an ethnical group may greatly help us in the understanding of the process of "mixing." As I have already shown,<sup>4</sup> the formation of the Goldi has taken a long time. They were originally a Northern Tungus group, or, even better to say, groups, speaking the Northern

<sup>1</sup>The above-quoted facts are taken from the material published by W. L. Kotwicz (cf. "Material for the Study of the Tungus Dialects").

<sup>2</sup>According to the material published by P. P. Schmidt, cf. Olcha.

<sup>3</sup>The idea of the existence of "mixed languages" has been severely criticized, but the above-quoted facts seem to support it in full sense.

<sup>4</sup>Cf. my "Northern Tungus Migrations," and "Social Organization of the Northern Tungus."

Tungus dialects affected by aspiration. In fact, words common for the Goldi, Manchu, and Northern Tungus met with the initial *p*, *f*, and *θ~h~x* denote such elements like "soul" "lip," "palm of the hand," "thumb," "forehead," "wound," "trousers," "hammer," "to follow," "downwards," etc., while the words met with in Goldi with the initial *p* and in Manchu with the initial *f*, but unknown in the Northern Tungus, denote either local phenomena (e. g., animals and plants) or special phenomena unknown amongst the Northern Tungus. On the other hand, we meet with words with the initial consonants *f*, *p*, *w*, *v* in Goldi and Manchu which sometimes correspond with the different Northern Tungus words. Besides these cases, there are a few cases where the initial *p* of the Goldi corresponds to *θ~h~x* of the Northern Tungus dialects. This class of facts may be understood as due to the loss of the old Manchu words, or even in some cases to the formation by analogy through the spreading of the bilabial *tenuis* occlusive over the words with the aspirated initial vowels. This was a secondary phenomenon which might have occurred during the period when Goldi was already under a strong Manchu influence. From the above-mentioned facts, the origin of the initial *p* in Goldi may be better understood as a case of Northern Tungus dialects which have come into contact with the Manchu (also Nuichen) linguistical complex and partly borrowed it, first altering their original phonetic characters (the period when the Manchu-Nuichen *f* became the *p* of the Goldi) and afterwards, the change of the old phonetic complex without any essential change of the Manchu words (the period when Manchu *f* was preserved).<sup>1</sup> The behaviour of the Goldi towards the Northern Tungus phonetic characters has also changed, for, at the present time, it treats the Northern Tungus phonetic peculiarities as foreign, although the old "aspirating" type is still preserved in many words of a probably early origin. In this respect, it is very interesting to know that they now reproduce very well the Northern Tungus words received without aspiration, although the same words in an "aspirating" dialect must be aspirated. It shows that the influx of the second non-aspirated

<sup>1</sup>This is a process which is now observed in some Northern Tungus dialects in Manchuria. Some of them are under Manchu influence and some of them are under Mongol influence. Little by little they become familiar with the foreign phonetic system and do not alter the original (foreign) pronunciation.



Northern Tungus element does not meet with the phonetic modifications characteristic of the "aspirating" dialects.

As to the Northern Tungus dialects, they do possess the initial *p* in rare cases of words for which we have no evidences for showing that these words are loaned from the Manchu, Mongol, and Russian languages, also from some Palæasiatic dialects. These facts seem to show that the Northern Tungus is not hostile to the initial *p*, as it is hostile, for example, to the corresponding labio-dental *tenuis* spirant *f*. On the other hand, the phenomenon of transition from *media lenes* to *tenuis* is not observed in the Northern Tungus dialects, but in Manchu Sp., which, in so far as the present problem is concerned, particularly affects the initial bilabial *media lenis* occlusive *b*, but no tendency as to the transition from spirants to occlusives in bilabials and labio-dentals is observed. The process of transition from *media lenes* to *tenuis* is observed in Goldi, which makes a very great difference when Goldi is compared with Manchu Sp.

Considering all the facts and conclusions to which we have come, it may be formulated thus: (1) The Goldi language being a mixed one, it is not a typical representative of either the Northern Tungus or the Southern Tungus, and, in addition to this, it is greatly affected by some third elements, which, apart from Mongol, might be various Palæasiatic languages now extinct; (2) the frequent occurrence of the initial *p* in Goldi is not due to the preservation of this consonant, but to the phonetic reaction on an alien (Southern Tungus) phonetic system characteristic of the Northern Tungus dialects. Therefore the reference to the Goldi language as one preserving the original initial labial (bilabial) *tenuis* sound is foundless, for it is of a secondary origin.

These inferences are important for our analysis of the problem of the original initial sounds of many Tungus and alien words. Since the process of aspiration and bilabialization is going on in several Tungus dialects forming two distinct phonetic currents of fashion, and since there are no evidences for showing the loss of the *tenuis* bilabial initial consonants, but, just the contrary, reveals their consolidation and spreading, the Tungus languages cannot be taken as evidence of a loss of the above-discussed initial consonants. What has been shown in the previous section and what may be supported by other evidences quoted from the Tungus is that, in a great number of cases, the appearance of the initial consonants—bilabial,

labio-dental and glottal, spirant and occlusive, *tenuis* and *media lenes*—is a relatively recent phenomenon, due to the processes of aspiration and bilabialization, conditioned, perhaps, by the increase of accentuation of other vowels of the words. This conclusion does not imply the hypothesis of the non-existence of the initial *p* in Tungus, as initial *p*'s are known at the present time and so might have been also in previous periods of formation of the dialects. Yet this conclusion does not imply that all words with the initial *f*, *p*, *h*, *x*, *w*, and *v* must formerly have been words with initial vowels.

### 33. Mongol Aspiration and Bilabialization

Such is the situation with the variations of the initial consonants connected with the process of aspiration and bilabialization in Tungus, so that in reference to this language the hypothesis of G. Ramstedt of the initial \**φ* and that of P. Pelliot of the initial \**p* cannot be accepted. In other languages the similar processes might have a place too. The fact of aspiration in the Mongol language is particularly interesting. With all possible reserves as to my competence in Mongol I will now point out some facts.

The present Mongol languages and Mongol written language bear some traces of formerly aspirated initial vowels. Yet the mediæval dialects recorded possessed well-developed aspiration, which is also met with in some living dialects.<sup>1</sup> Some words with stabilized glottals, e.g., *k*, *g*, etc., seem to have come out of the previously

<sup>1</sup>The existence of aspiration in Mongol is a well-known fact. So A. Bobrovnikov ("Grammar of the Mongolo-Kalmuk Language") and A. Pozdnev (Introduction, p. viii) pointed out that the initial vowels as a rule are aspirated. G. Ramstedt ("Comparative Phonetics of the Mongol Written and Xalxa-Urga Dialect," Sec. 46, p. 44) treats this phenomenon in the Urga dialect as a "gradual glottid" (cf. Sievers, Grundzüge, etc., fifth ed., 1901, Sec. 359), and N. N. Poppe ("Mongol Names of the Animals in X. Kasvini's Work," p. 205) repeats it in reference to the mediæval Mongol words. However, this "gradual glottid" is so strong in some Western groups and in Dahur that in the records it appears as *x*. G. Ramstedt, in his later publication ("Ein anlautender, etc.," *op. cit.*), supposes that the *h* of the mediæval Mongol languages, according to Yuan Chao Mi Shi (A.D. 1241) and as recorded by Guiragos, must be "eine damilige entwicklungsstufe des ehemaligen *p* od. *f*-lautes" (*id.*, p. 8). W. L. Kotwicz treats it (*Rocznik Orientalistyczny*, *op. cit.*, Vol. II, p. 247) as an aspiration met with in the mediæval Mongol groups, of which groups speaking the Amdo dialect and their neighbours have been produced. He compares it with the aspiration met with in Dahur. P. Pelliot ("Les Mots, etc.," *op. cit.*) designates it in the mediæval Mongol as an aspiration *h* which is lacking in the present Mongol.



practised aspiration. For safety's sake, it will perhaps be better to say that the "aspiration" and "gradual glottid" also *h* and *x* exist in Mongol dialects just as they exist in the Northern Tungus dialects, but they are unknown in the Mongol written language and in most of the living dialects. If we suppose, then, that the process of aspiration existed during the mediæval ages, it might be confined only to a certain group of Mongol dialects about which we have some information, but the other dialects might have had no aspiration. Indeed, the number of groups speaking an aspirated or a nonaspirated dialect might vary greatly. At the present time, most of the Mongol groups know no strong aspiration which may be perceived as *h* and *x*, but the groups using the aspiration might have been more numerous than the nonaspirating during the Middle Ages. Therefore I think that it is perhaps mere anticipation to consider the initial *h* of the mediæval Mongol as lost nowadays. The dialect laid down as the basis of the Mongol Writ., as well as some modern dialects, might have no aspiration at all, while other dialects might be affected by this phenomenon. If this is so, then the idea that the consonant has been lost is erroneous, and "genetically" *h* has nothing to do with the hypothetical initial bilabial *tenuis*.

Besides the occurrence of the initial *b*, corresponding to the aspiration or zero treated by P. Pelliot as doublets, there are some other facts which point to the existence of bilabialization in Mongol. In fact, P. Pelliot ("Les Mots, etc.," *op. cit.*, p. 236) quotes the following three parallels from a Chinese document, a short dictionary recorded in the sixteenth century: *fünägä~hünägän* (mediæval)~*ünägän*—"the fox"; *füni~hüni* (mediæval)~*ünin* (Buriat)—"smoke"; and *fula'an~hula'an* (mediæval) *uta'an*—"red." These parallels are not very convincing, perhaps, for the Chinese author might percept *h* as *f*. However, we have another series of facts; namely, the Shirongol dialect, which gives *xunisi* by the side of *funisi*—"the ash," "*fulyan*," etc. Thus these dialects possess *f* in the words with initial *h* and zero. P. Pelliot (*id.*, p. 251) considers it, in view of maintaining G. Ramstedt's hypothesis, as a secondary phenomenon due to the alteration *h*→*f*, analogous to the Chinese dialects. Yet he points out that the initial *f* is met with in the words with the labialized vowels, i.e., let us add, as it is in the Southern Tungus language. If this is so, then it is probably not a secondary phenomenon in the sense of *h*→*f*, but it is a secondary phenomenon

in the sense of the "bilabialization of vowels," and in first-hand labialized ones. Indeed, *f* treated as a secondary phenomenon of the *h*→*f* type does not help in establishing the original initial labial *tenuis*.<sup>1</sup>

After the present work had already been handed over to the printers I received two publications concerning Mongol dialects which makes it necessary to insert this additional paragraph. A. Mostaert and A. de Smedt ("Le Dialecte Monguor, etc.") this time treat of the phonetic system of the Monguor dialect of Kansu,<sup>2</sup> in which one may observe both an aspiration and a bilabialization of vowels. In agreement with G. Ramstedt and P. Pelliot, these authors connect the phenomenon of "aspiration" disclosed in this dialect as a preservation of an archaic character of the Kansu dialect (*op. cit.*, p. 146) and as a result of the alteration of the original "labiale fricative dure, ou, plus probablement l'occlusive dure \**p*" (*op. cit.*, p. 804). The facts observed have inclined to make them recognize that *f* and *x* might have originated from "renforcement" and "assimilation" (*op. cit.*, p. 805). It is interesting to note that side by side with this phenomenon one also meets with the initial *š* in cases where we might expect to find glottal and labial consonants (*ibid.*). The series under discussion may be thus represented according to A. Mostaert as shown in the scheme (\**φ* or \**p*) → *h*  $\begin{cases} \nearrow x \text{ (through reinforcement)} \\ \searrow f, š, s \text{ (through assimilation)} \end{cases}$  [hypothetic]

One may also observe the loss of the initial vowel when it is not aspirated or bilabialized (*op. cit.*, p. 807, §21). Yet, in a great number of cases the initial voiced bilabial *b* is found voiceless and even an aspirated *ph*.<sup>3</sup> Moreover, there is a great number of cases when the initial *x* corresponds to the aspiration in mediæval Mongol and zero in Mongol Writ.<sup>4</sup>

<sup>1</sup>P. Pelliot seems to believe that the only way to prove the appearance of the initial *h* is to show its derivation from a labial, for he says that otherwise its origin would be mysterious. As shown, its origin is not mysterious at all, at least in the Tungus. The fashion of bilabialization might affect both the Tungus (Southern) and the Mongol.

<sup>2</sup>A. Mostaert previously published his investigation of the Urdus (South) Mongol dialect, which is free from the aspiration and the bilabialization of the initial vowels.

<sup>3</sup>E. g., in Mongol Writ. *ližig*, *burhan*, *burgiraku*, *bökeiku*, *lurčag*, *betge*, and *bühüli* are found in Monguor with the initial *ph*. This is not a general phenomenon, however, as it is also observed in Manchu Sp.

<sup>4</sup>Most of the cases where the initial vowels are aspirated correspond to the words with the initial *a* and *e* of the Mongol Writ. However, there are also cases where the initial *o* and *u* are aspirated, e. g., *oriyaku*, *ombaku*, *okor*, and *wum*, are aspirated in Monguor.



Cases in which the initial vowels are bilabialized are numerous. In most of them the initial *f* corresponds to *h* of the mediæval and zero of the written Mongol, so that the tendency may be regarded as well established.<sup>1</sup> However, there are some cases where Monguor possesses at the same time parallel forms with zero and bilabialization, with aspiration and bilabialization, as, for instance, in Goldi. In this respect, the analogy with Manchu (especially Manchu Sp.) is going so far that there are cases of parallelism of *b* and *w*, e.g., *baḏzar* ~ *waḏzar* || *basar* (Writ.)—"the city"; *bargu* ~ *wargu* || *bariku* (Writ.)—"to take up"; and alternation *w* ~ *j*, e.g., *withan* ~ *y'ūthan* || *uitan* (Writ.) || *hiutan* (mediæval)—"narrow." These cases are found parallel with zero in Monguor and aspiration in mediæval, e.g., *ači* (Monguor) (Writ.) || *hači* (mediæval)—"the grandson." It may thus be pointed out that not only labialized vowels are increased with a consonant, but also the non-labialized vowels as well.<sup>2</sup> Another point of interest is one in the case of nonbilabialization where the initial vowel sometimes may disappear altogether; e.g., *wesē* ~ *besē* ~ *jesē* || *ebüsin* (Writ.), *öwösu* (Urdus Sp.)—"grass." This is a phenomenon which has been observed in the Tungus language.<sup>3</sup> The comparison of these phenomena as they are observed in the Mongol and the Tungus language leads us to the conclusion that the analogy is rather complete. It may be thus supposed that in Mongol the occurrence of bilabialization and aspiration is not due to the presence and degeneration of the initial labial consonant, but to the phonetic fashions of bilabialization and aspiration, which some time ago affected the southern Mongol.

<sup>1</sup>Some of these cases are particularly interesting. There are some monosyllabic words, e. g., *fan* (Monguor) — *on* (Writ.) — *hon* (mediæval) — "the year" (*op. cit.*, p. 151) *fē* (Monguor) — *oi* (Writ.) — *hoi* (mediæval) — "the forest" (*op. cit.*, p. 155). Some of them have already been treated in the present work, e. g., *fōr* (Monguor) — *egur* (Writ.) — *he'ūr* (mediæval) — "the nest" (*op. cit.*, p. 810) (cf. *ūr* of the Urdus S. and *infra*, Chapter V); *f'ūda* (Monguor) — *uguta* (Writ.) — *huguta* (mediæval) — "the bag," "sack" (cf. *infra*, Chapter V). A. Mostaert's transcription here and further is given in slightly simplified form.

<sup>2</sup>The above quoted *withan* may be regarded as a case of the loss of the vowel *u*. The situation is complicated, however, by the fact that sometimes the vowels, like *o* and *ö*, are not labialized at all (cf. A. D. Rudnev, "Xori-Buriat Dialect," *op. cit.*, p. 11). What kind of vowels were originally found in the words where the initial vowels are now found to be "bilabialized" is not always ascertainable.

<sup>3</sup>Cf. *supra*, Section 31; also "Notes on the Bilabialization and Aspiration in the Tungus Languages."

Another publication is that by N. N. Poppe on the Dahur language, Xailar dialect. This dialect seems to show certain characters which support my thesis as well. N. N. Poppe has found the phenomenon of increase of the initial vowels with bilabialization (*vide* pp. 72, 73 of his vocabulary), and some traces of aspiration which he has not reproduced in his work. Another Dahur dialect, recorded by A. O. Ivanovskii, is strongly affected by aspiration, while bilabialization is little known.<sup>1</sup> N. N. Poppe makes a suggestion that the Dahur *w* and the Manchu *v* might originate from vowels (*op. cit.*, p. 112),<sup>2</sup> yet he also suggests that *w* and *v* might have been preserved from the hypothetic initial *\*w*, by analogy with the hypothetic initial *\*p*. The phenomenon of bilabialization in the Dahur language seems to be quite recent,<sup>3</sup> as it is also shown in Manchu Sp., where most of the labialized vowels and all the *u*'s are supplied with *w*. Thus, in Dahur, as well as in Monguor, this process is still alive. The difference between the two languages is that the Monguor language also bears traces of an old process of bilabialization, while in the Dahur language it seems to be only of recent fashion.

If one does not presume the process of the loss of *\*p* in the Mongol language, perhaps the whole situation would appear as simple as it

<sup>1</sup>In the series of "bilabialized" cases may be included the following words given by A. O. Ivanovskii *vantabei* || *untaxu* (Writ.)—"to sleep"; *vairo* || *oira* (Writ.)—"near"; *wakar* || *oxor* (Writ.), *axar* (Kalm.)—"short": also perhaps some more cases may be classed in this group. However, these cases are not so numerous as they are in the Manchu and the Monguor dialect.

<sup>2</sup>Indeed, the word *uas* (Dahur), *vasa* (Manchu), as shown, is Chinese (cf. *infra*, Section 47). The same is perhaps true of another instance which he gives; namely, *woa* (Dahur), *va* (Manchu)—"the smell" (cf. "Notes on the Bilabialization and Aspiration in the Tungus Languages"). In "Notes on the Bilabialization and Aspiration in the Tungus Languages," I gave several other instances which show that *w* and *v* are merely particular cases of bilabialization.

<sup>3</sup>The recentness of this phenomenon in the Dahur language (in the Xailar dialect and also in the dialect recorded by A. O. Ivanovskii) is especially evident, for this language still behaves like nonbilabializing in the case of foreign words with the initial *f*. In fact, the latter is not always correctly reproduced, being altered into *p*. So two cases are reproduced in N. N. Poppe's vocabulary [*faida* (Manchu) and *fan-se* (Chinese)], and a case in A. O. Ivanovskii's [*f* (Manchu) = *bi* (Chinese)]. However, the initial *f* is sometimes correctly reproduced by the Dahurs [e.g., Manchu, *fanambi*, *f*, *fulxu*; Chinese, *fu'en*, according to A. O. Ivanovskii; Manchu, *fafula*, *furdan*, according to N. N. Poppe]. This phenomenon is analogous to what I have observed in the Bir. dialect, including the fact that both the Tungus and the Dahurs pronounce correctly when familiar with Manchu. The behaviour of some Goldi dialects is the same.



is in Tungus; namely, that there existed, and still exists in some dialects, a tendency of bilabialization and in other dialects a tendency of aspiration, while some third dialects remained beyond these phonetic "fashions." The southern Tungus (the Nuichen) and the southern Mongol (perhaps the Toba, and as preserved in Kansu) were long ago affected by bilabialization, while some Northern Tungus and some northern Mongol dialects were affected by aspiration (prior to the first records of the mediæval Mongol; cf., also, variations of these phenomena in Tungus connected with the migratory waves, which, under a certain probability, may be connected with definite historic moments). Both phenomena might or might not be synchronous, and there might be no connexion between them. As I have already supposed, the fashion of bilabialization might originate under Chinese influence, or even under the influences coming from Central Asia. This fashion still persists in Dahur and Manchu Sp. supported chiefly by Chinese influence.

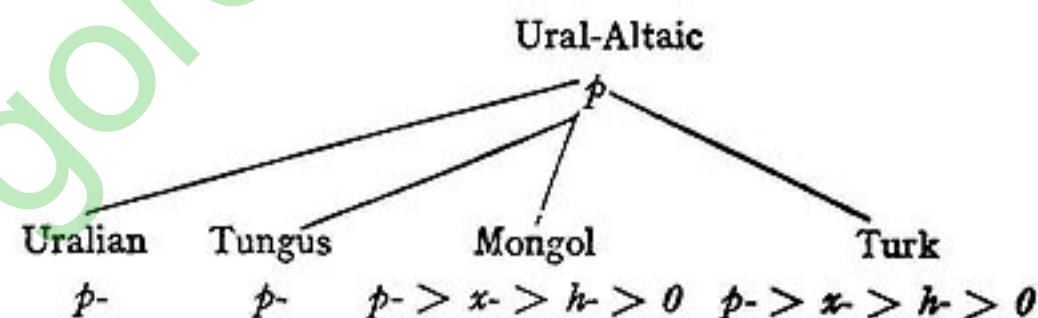
The Dahur language, considered in the light of this interpretation, appears to be recently affected by bilabialization and formerly to have been strongly affected by aspiration, while the Monguor dialect was strongly affected by both; yet the dialects which led to the basis of the Mongol written language were not affected by these phonetic fashions.

In my earlier paper ("Notes on the Bilabialization and Aspiration in the Tungus Languages," written in 1927) I hesitated more than at present as to the possibility of generalizing my hypothesis concerning the phenomenon of bilabialization and aspiration in Mongol. The new facts expounded above dissipate my hesitation as to spreading my hypothesis, which I am now inclined to consider as a theory.

#### 34. Linguistic Importance of a Correct Solution of the Problem of Aspiration and Bilabialization

Still greater difficulty in solving this problem is met with in Turk where no *h* or *x* are found. However, some words of the supposedly common origin are found with the initial *b*. These occurrences are explained as due to the preservation of the original initial bilabial of the Altaic pra-language. However, what the actual origin of this consonant in these words is is not yet established. The process of bilabialization in Turk might have the same place as it is in Mongol or as it is in Tungus. And lastly, the group of languages known

under the name of Uralian has been brought by A. Sauvageot to join the Mongol, Turk, and Tungus under the same idea of the hypothetical loss of the initial labial *tenuis* in the "Altaic" languages. In the work of this author, the history of the occlusive *tenuis* is represented in an absolutely definite manner, as he says that "l'histoire de l'occlusive sourde en position initiale a été la suivante" (*op. cit.*, p. 62), as shown in the table below. I quote it in full as the most completely and definitely formulated statement based upon the work of several predecessors and his own, and also as a case of a consonant which in the "Ural-Altaic" languages, according to this hypothesis (promoted to the grade of "law") had a very complex history



The analysis of the phenomenon of aspiration and bilabialization in Tungus has already brought us to the conclusion that the phenomenon of occurrence of the initial *f*, *p*, *v*, *w*, *h*, *x*, and zero in the words of the same meaning does not require the hypothesis that there was such a complex history of the loss of the initial bilabial *tenuis*. On the other hand, we have also seen that the similar phenomenon of aspiration in Mongol is quite possible. Perhaps the case of bilabials in Turk may also be connected with the process of bilabialization, if it is shown that the words are really of common origin. Indeed, my generalization is not spread over Turk, for I am not familiar with this language in the same degree as with the Tungus language, and the material that may be compared is rather limited. This may be due to two conditions; namely, the remoteness of similar processes in this language, or perhaps an insufficient development of any of them in the Turk. Here it must be pointed out that the remoteness must be understood, not only absolutely, in terms of centuries, but also in the relative sense; namely, how many phonetic changes had occurred since these phenomena, if any, had taken place.



Besides the fact that the phenomena of aspiration and bilabialization may nowadays be observed in Tungus, we are in an extremely good position, owing to the existence of Goldi-Oлча, where, as shown, owing to the transplantation of bilabialized elements to an alien soil, the character of bilabialization has received a very strong and sharp expression through the modification of even a slight bilabialization into a stable element like *p*. It is curious to note that this language has been greatly responsible for the hypothesis of the loss of this consonant. As a matter of fact, without the accidental preservation of Goldi, it would be difficult if not impossible to build up this hypothesis. In Mongol and Turk it is applied by the simple method of analogy.<sup>1</sup> Yet with the loss of Manchu the hypothesis would be absolutely impossible. However, the preservation of Manchu is also a mere historic accident. The Goldi and Manchu languages are fortunately preserved, but how many other languages are lost for ever? How many phonetic processes have escaped the attention of now living observers? But the facts which are lost might perhaps bring the observers to entirely different conceptions. The hypothesis of the loss of the initial *p* is based, not upon the facts, but chiefly upon certain theoretical reasoning for illustration of which the facts are picked up here and there. One of the presumptions which is usually left without discussion is that the languages under discussion were formed from a common ancestor in rather recent time, and this moment is referred to the historic period. The fact that there are little changes in Turk as seen in Uigur, in Mongol as seen in the mediæval language, and in Southern Tungus as seen in Nuichen produces a certain disappointment, for one has to remove the moment of hypothetical separation to the periods when there were no documents. This is a reasoning by analogy with the present Indo-European languages, most of which were formed in historic time witnessed by numerous written documents. Another important presumption is that the languages lose their phonetic elements implied by the transition from *tenues* to *mediæ lenes* and zero. Indeed, these phenomena do exist in some

<sup>1</sup> My approach to this problem in Mongol is also made by the analogy with Tungus, so I do not insist upon my theory in Mongol as a "law" but as a simple working hypothesis. In reference to Turk I cannot classify it even as a suggestion. The processes of loss are not rare, indeed. What is good for a particular case of Tungus may happen to be invalid for Turk.

particular languages which supplied facts for this conclusion, but the generalization of this conclusion cannot be supported by the contrary facts; namely, the alteration of *mediæ lenes* into *tenues*, which also exists in some other languages. However, the former alteration is in good agreement with the ethnographical complex of the gradual "weakening and deterioration of human species." Such terms as "fortis," "stark," and "strong" as opposed to "weak" support the idea of "weakening." This conception remains effective even in the case of a change of terminology. The third important presumption is that the changes in languages always take definite direction conditioned by the factors appearing in the language as an organic entity. This is also a reasoning by analogy with the organisms conceived in a simplified manner.

If we now summarize what has been previously stated, we may see that the formation of this hypothesis is in some way independent on the facts and the latter originally were used not as facts needed for conclusions but as illustration, just as the artist in mosaic work uses differently coloured materials for combining them in such a way as to express his own idea. Such is the work of G. Ramstedt in which this idea was illustrated with a series of incidentally picked-up facts. The choice of facts was not always fortunate, so the mosaic work appeared but as a simple essay. The same methodology is seen in other works. G. Ramstedt has started his work with forty-two parallels from "Altaic" languages, P. P. Schmidt has added some more, P. Pelliot has also added some, and A. Sauvageot, who has included the "Uralian" languages, has brought forth eighty-five parallels (cases) to illustrate G. Ramstedt's hypothesis. In this form the hypothesis seems to come out of facts and its origin is unintentionally camouflaged from the eyes of laymen. In its present form it has a new function; namely, to prove in the most daring form the existence of an ancestor language—Ural-Altaic. So that *volens nolens*, we have now to proceed together with these authors to the analysis of evidences.



## CHAPTER V

## ANALYSIS OF PARALLELS

## 35. The Scope of the Present Analysis

From the previous chapter we have seen that in the analysis of parallels we must be ready to meet with several difficulties due to the acceptance of a series of hypotheses. Yet, as has been shown, A. Sauvageot is not alone in accepting them. In fact, a large portion of parallels is taken by this author from previously published works. I will confine the analysis of these parallels to the Tungus language only. Yet since the problem of the aspiration and the bilabialization of vowels in Tungus is a phenomenon disconnected with that of the Ural-Altaic hypothetical language, I will not mix it with the hypothesis of the Tungus connexion with this hypothetical language. The specialists in Mongol and Turk will perhaps show some day what the relationship is between the process of aspiration and bilabialization, if they ever existed in these languages, and the hypothetical pra-language. So the problem which I have before me is this: How far do the evidences shown by A. Sauvageot and his predecessors prove the existence of a pra-language from which the Tungus language is supposed to have originated? I will use the evidences from other languages only in connexion with the Tungus parallels. So I will leave out of the discussion the problem, warmly discussed, as to the existence of a special Altaic group of languages. The latter is a point in which there is a serious discrepancy of opinions of different authors. As will be seen later, this controversy has no bearing upon the problem of relationship between the Tungus and other languages.

In the following analysis of the parallels I shall go according to the order of parallels given by A. Sauvageot, and I shall preserve his numeration of "cases." He has classed his parallels into nine groups according to the initial and intervocalic consonants, as shown:

<i>p</i> initial . . . . .	41 cases
<i>p</i> intervocalic . . . . .	22 cases
<i>b</i> initial . . . . .	21 cases
<i>t</i> initial . . . . .	20 cases

<i>f</i> intervocalic . . . . .	8 cases
<i>h</i> initial . . . . .	53 cases
<i>h</i> intervocalic . . . . .	23 cases
<i>ŋ</i> initial . . . . .	8 cases
<i>ŋ</i> intervocalic . . . . .	16 cases

The initial and intervocalic consonants are practically understood as a series of actual and hypothetical sounds of labial, dental, glottal, and nasal types. Yet, since the series of *f~p~h~x~θ* is postulated, the parallels of the labial type are extended over the words with the initial glottal and vowels. The same extension is seen in the case of the nasal group, in which the words with the initial glottal and also *n* and *m* are included. This series is not yet complete, and A. Sauvageot promises to give another series with the initial and intervocalic frontal alveolar affricates and spirants, the parallels with which, according to him, are more complex and difficult than the one discussed here. I must agree with him, for the alternations of these consonants with dental, also labial and glottal, are very common.

Unfortunately, these two hundred and fourteen cases, as material good for comparison and conclusions, are not equal from the point of view of Tungus parallels. In fact, there are some cases of Tungus parallels lacking, and some cases lack all parallels except Samoyed and Tungus. Yet in some cases the Tungus words are misinterpreted, or wrongly used as Tungus ones. And lastly, in some cases the semantic limits are so wide that the Tungus words cannot be accepted as reliable material for comparative purposes. In analysing the Tungus parallels, I have found a certain number of them out of use, which may be clear for every one who is a little familiar with Tungus; a certain number of cases which cannot be admitted for comparison; and a certain number of cases which are worthy of analysis. I will here give only cases of the last group, for a discussion of all the cases would take too much space and is of no practical use for my present purpose. However, I feel that I must give the idea of how the cases have been rejected. For this purpose,



I will analyse three cases of the first group and six cases of the second group in order of their appearance in A. Sauvageot's work.<sup>1</sup>

### 36. Examples of Cases Rejected Without a Detailed Analysis

The following three cases have been marked by me as unworthy of a detailed analysis:

Case 3. G. Ramstedt's hypothetic Altaic stem \**pɜrɜ*, the same stem for Finno-Ugrian and the following sense,—in Altaic "*fond*," "*base*," "*sol*" and in Finno-Ugrian,—"*kopf*," "*haupt*," "*hinterteil*," "*zurück*," "*schwanz*," "*arrière*," "*derrière*," etc., while in Tungus all of them are correlated with the idea of "bottom." If one clears up the problem of Tungus parallels a little, then it may be seen that *rV* is a suffix which may be replaced by other suffixes in other words of the same stem. The latter is "down," found as a certain vowel in different Tungus dialects (it may be found as *a*, *o*, *e*, *u*, *i*) and may be designated as *V*. In some dialects it is aspirated, while in some other dialects it is bilabialized.<sup>2</sup> Of course it may be supposed that the stem with the suffix was received from a Tungus language inclined to bilabialization (Southern Tungus), but this supposition would make of it a "loan-word" in all other languages, while it is supposed to be one of important facts of uralaltaicology, if I am allowed so to style the theory of the Ural-Altaic common language. In order to save this important word for other languages, it will be advisable to exclude the Tungus word from the series. Indeed, this case is rather complex.

Case 6. The idea of "wheel," "circle," "round," from the idea of "turn," etc., according to A. Sauvageot is expressed by

<sup>1</sup>These cases are not the most striking ones in A. Sauvageot's work. I might have selected some cases of a really startling type, but I think they will not give the correct idea of the reasons of rejection. Therefore I will follow A. Sauvageot's order of cases mechanically.

<sup>2</sup>Some details are given in my paper "Tungus Terms," *op. cit.*, p. 178 *et seq.* (p. 12 *et seq.* of the reprints).

the stem \**pɜrɜ* (i. e., just the same as the previous one); in Altaic, it is even *pɜrɜ* and has about the same meaning,<sup>1</sup> but the Tungus words quoted are *pyrrxa* (Goldi) and *urgoptun* (Man. *Ivan.*), which means exactly "the ring put on the thumb." The origin of the word is very simple. *pyrrxa* (I think it is *pɜrxe*)=*ferxe* (Manchu Writ.)—"the thumb," which has already been indicated by W. Grube; and *urgoptun* is a derivative of *urugun*<sup>2</sup>—"the thumb," increased with the suffixes + *Vp* + *tun* widely used in most of the Northern Tungus dialects for the formation of "nouns." The "thumb" can thus be easily dismissed. However, the Tungus dialects possess some words which may help, e. g., *urga* (Khin.)—"the lasso"; *urka* (Bir.)—"to make a running knot," "lasso." Unfortunately, they are not bilabialized and are recently borrowed from the Mongols (together with the horse-breeding complex). There are, of course, some other words which may be included, as, e. g., the network for the door (the cover for the entrance) made of a system of loops (the "idea of circle," etc.) and which is *urkaptun* (several dialects), but it has derived from *urka* ("the entrance," "door," and not "round") and the suffix *-ptun*, already discussed (*vide* Case 6). In so far as I know, there is no such stem with such a meaning in Tungus.

Case 7. The stem is \**pɜrɜ* and means "the bark." The Tungus parallels are *feri* (Manchu Writ.), which is translated as "sk n," but which actually means the "excoriation," particularly "saddle gall" (on horses), and which is connected, for example, with *irči* (Bir.)—"to excoriate," "to produce collosities," etc. The stem is probably *ir*, and *-či* is a suffix. These words may be connected perhaps, with the stem *ir*—"to rub," "to make even," also, perhaps, found in Mongol.

<sup>1</sup>Although I disagree with the use of such terms as "meaning," "word," etc., as they are here used, and in spite of the fact that I have pointed out the great undesirability of confounding "starter," "symbol," "meaning," etc., I continue to use them in this part in the conventional "meanings" as practised in most of the special linguistic publications. The reason is that I fear that a new misunderstanding in the technical matter of the analysis of parallels will occur, for the reading of Part One will be omitted by many a reader.

<sup>2</sup>Details regarding *urugun* may be found in my paper on "Bilabialization and Aspiration," *op. cit.*, p. 256.



The parallel has evidently nothing to do with "bark." Another word compared by A. Sauyageot is the Goldi *xyrrekta* (*xirekta*, S. Sh.) and *erikta* translated by W. Grube as "haut" and compared with Manchu *feri*. I have some hesitation as to the correctness of translation given by W. Grube. I think it is merely "bark of the larch-tree," for in other Tungus dialects we have *irakta*, *hirakta* (Bir., Kum., Khin., Mank., Barg.), and even *irikta* (Ner.) is exactly the "bark of the larch-tree," and "larch-tree." The Goldi language for the name of tree uses terms unknown in other Tungus dialects; namely, *sése*, *xese*, *issi*, *sisi*, connected with the Manchu Writ. *isi*, whence it may be supposed that *erikta*, *xirakta*, have been preserved in the sense of the "bark of the larch-tree" and thus connected with "tree" and not with the "bark." All known Tungus dialects possess a special stem for "skin"; namely, *nanda*, with modifications, while the stem *irakta* is not met with in any word connected with "skin."

In these three cases the reasons for rejection are different. So in Case 3 the chief reason is the impossibility of reducing the Tungus words to the hypothetic stem without speaking of too broad semantic limits. In Case 6 the reason is the wrong etymology of Tungus words. In Case 7 the reason is an artificial interpretation of Tungus words, a wrong analysis of stems, and a wrong translation. In other rejected cases, the same reasons are met with as well as other serious reasons, but it will be of no interest here to give an enumeration of the types of reasons.

### 37. Examples of Cases Rejected After a Minute Analysis

In this group of cases the corresponding words are found in Tungus, but for various reasons they cannot be maintained for the parallels.

Case 1. G. Ramstedt's parallels and hypothetic stem  $*p\delta$  or  $*p\delta$  in Finno-Ugrian means "tree," in Tungus "birch-tree," etc. The equation seems to confirm the existence of the same stem in Goldi and Manchu  $\beta a$ ,  $\beta \bar{a}$  (Goldi) which is *fa* (Manchu). Theoretically, if we suppose this stem to be a Tungus one, it may be reduced to *ja*, and even *a*,  $\bar{a}$ , unless

it is borrowed by the Manchu in the form *fa* from some other language; e.g., Palæasiatic.<sup>1</sup> However, such a stem with the meaning of "birch" and "tree" is unknown in Northern Tungus. The "birch" is *čatban* (Bir., Kum., Khin., RTM, Ner., Barg., Tum., Lam.), etc.; cf. also *čalfa* (Manchu Writ.) with a special meaning—"the raw birch-bark." The other parallels are inconsistent; namely, *hijika* (W)—"wald," and *hijela* (W)—"im dickicht."<sup>2</sup> Both of them are well-known words in this particular dialect being modified, namely, *siyi* (RTM, Khin., Ner.), *sigi* (Neg., Sch.), *hegi* (Tum.) and even *egi* (Lam.), whence *siji* with the alteration  $s \rightarrow h$  *hiji* (W). The suffixes are *-ka* and *-la*, the first one perhaps *diminutivus*<sup>3</sup> and the second one *directivus*. These words are invariably used for "dense, thick forest," "thickness in forest," etc., occupying a usually limited area and consisting usually of young larch and cedar ("spreading") trees. This cannot refer to the birch-tree forest, for the birch-tree never forms such a thick forest, yet it cannot refer to the "forest" in general. Being different as to the meaning (thickness) and stem (*sigi*), it cannot be connected with  $p\delta$  and  $p\delta$ . The only possible connexion is Manchu *fa*, which is not perhaps a Tungus word at all, for it is confined to the Manchu Writ. only, whence borrowed by the Goldi.

Case 4. G. Ramstedt's stem  $*p\delta i\delta\eta\delta$   $*p\delta l\delta\eta\delta$ , which is compared with the Finno-Ugrian stem  $*p\delta\eta\delta$ —"the palm of the hand." The Tungus parallels are *falaŋo* (Manchu Writ.) (the spelling is rather complex), *βajna*, *xana*, *hana* quoted by

<sup>1</sup>With certain imagination it may perhaps be connected with Gilak "birch-bark" *xiv*, *xip* (Gilak, W. Grube), cf. *hivo* (Ner.) *kuva* (Lam.), *hiwā* (Tum.), *kiwa* (Ur., Mank., Castr)—"the birch-bark" (*k* appears instead of Gilak *x* according to the phonetic character of the Northern Tungus and in Gilak the final vowel very often disappears). Besides this stem there is another stem in Tungus; namely, *talū* (Bir., Kum., RTM, Mank.) (Goldi, Orcchi, Sch.), *talo* (Khin.) (Neg. Sch.), cf. Manchu Writ. *tolxon*. However, it must be first shown that *xiv*, *xip* may be brought to *ja*, *a*, or  $\bar{a}$ . Indeed, it is absolutely uncertain and I do not naturally propose it. The fact is that we have two stems in Tungus.

<sup>2</sup>These parallels are brought forth probably owing to the Manchu orthography, which renders *f'a* as *fiya* (or *fija*).

<sup>3</sup>It is possible that in *hijika* *-ka* means something else, e.g., "shrubs," as is met with in some other dialects. However, in the given case it is not essential for us.



A. Sauvageot and compared with Mongol *halagan* (P. Pelliot), *alaya*, etc. However, the situation is not so simple as that. Other Tungus dialects give us two series; namely, *ongan*, *angačan* (Bir.), *anga* (Neg., Sch.), by the side of *xanga* (Neg., Sch.), *xan̄i* (Tum.), which can be reduced to the stem *an̄V* or even *an̄*—in some dialects aspirated, in some other dialects bilabialized. Another series is *aliya* (Mank.), *aliga* (Mank., Castr.), probably Manchu *falaño* (cf. *aliy* of Buriat, Castr.), which ought to be compared with the Mongol stem *alay*, etc. So that if we leave aside the hypothesis of the loss of *p*, which is a mere hypothesis, then we have to deal with two stems *an̄V* and *alVg*. To identify these two stems as one and the same, it would mean a building up of a new hypothesis; namely, *n̄l*, or *l̄n̄*, etc. Of course, it is not impossible, but it must be supported by solid evidences. On the other hand, the Finno-Ugrian series has words with the initial *p* forming the first syllable with *i* or *e*, and the second syllable varying between *v* and *η* reduced to the stem *p̄ñ̄δ*. Owing to the fact that this reconstruction and connexion is possible only on the condition of adopting several hypotheses, I think that the common stem, if it ever existed, is not definitely shown.

- Case 5. A. Sauvageot produces the stem *\*p̄δr̄δ*, i.e., always the same as in Cases 3 and 6, this time with a broad meaning "*toute armature de bois*." Naturally, the finding of words is not difficult at all and so they are found in all possible languages (e.g., in the sense "*grenier*," "*combles d'une maison*," "*schwimmende insel*," "*runder kasten*," "*floss am netz*," "*sledge*," "*island*," "*archipelago*," "*steppeninsel*," "*groove*," etc.). In this series are found *fara* (Manchu Writ.), *para* (Goldi, W. Grube) (Tungus, Gerstf.), the latter being naturally borrowed from Manchu. This case is rather analogous to Case I, for in various Tungus languages different words are found, e.g., *tolgoki* (Ner., Mank.) (Irk., Tit.), *tolgoki* (Neg., Sch.) [*tuki* (Or., Sch.), *ti.či* (Olcha, Sch.), *toki* (Goldi, Sch.), which P. P. Schmidt compares with Gilak *tu*]. Yet there are several other words for

designating the reindeer sledge; e.g., *nolima* (Lam.), *sirga* (Tum.) (cf. Yakut *syrga*, *syarya*, Pek. compared with *sor*, *sorga*, *čarga*, and *čirga*, some of Turk groups and some of Mongol groups). If the Gilak word *tu* is a contracted form of *tuki*, etc., and the latter is a contracted form of *tolgoki*, etc., it is not perhaps a Tungus stem, but a Palæasiatic (Gilak) one. Some other suggestions can be made, but since they are not sure I will leave the question where it is. Let us remark that the sledge is an element confined to the geographical areas where there is good snow and draught animals. Not all Tungus use the sledge, and amongst some of them the method of harness is the same as that used for dogs. Yet the words compared, *fara~para*, are found only in Manchu and in dialects influenced by Manchu, so the Goldi possesses two terms. Since the Manchu word is connected through a series of extremely extensive semantic operations, and since no such word is known in other Tungus languages and the parallels may be accepted only on the agreement with G. Ramstedt's hypothesis, I consider that these parallels are not convincingly shown.

- Case 10. The stem *\*p̄δr̄-* means "black" in Samoyed. The Tungus parallels quoted are *farxun* (Manchu Writ.)—"dark," and a series of words of the stem *akt*—"twilight," well known in most of the Northern Tungus dialects. The comparison of *farxun* and *akt* is reached by advancing another hypothesis: namely, *r* before *k* is lost in Tungus and the original stem must be *p̄δrk̄*. What *t* is, and where it is, is not shown, but it is merely dismissed. However, *farxun* may be understood as *far* + suffix *-xun* (cf. I. Zaxarov, "Grammar," *op. cit.*, p. 83, Sec. 53), and in the original form it might be *ar*. In this form it may be compared with the Mongol *härü* (proposed by P. Pelliot, "Les Mots, etc." *op. cit.*, p. 216), while the Northern Tungus stem *akt* will remain as it is. Since this stem is found only in Samoyed, cannot be connected with the Tungus Northern Tungus stem, and the Manchu stem may perhaps be connected with the Mongol stem, I cannot accept A. Sauvageot's parallels as valid ones.



Case 12. The same stem \**p8r*—this time with the new meaning “tranchant,” “coupant.” Only one Tungus word is found—*furu* (Manchu Writ.)—“to reduce to pieces,” e.g., “meat,” “noodles,” “tissue into small pieces of thread,” etc., i.e., “to mince,” etc., and compared with Mongol *hūrtāsun* (P. Pelliot), *örtāsān*—“chiffons,” “morceaux d’étoffe de soie coupés en fragments,” whence *urtahun* (Ur., Castr.), *urtasun* (Mank., Castr.), evidently borrowed from Mongol in the sense of “patch,” “piece.” Since no words are found in other Tungus dialects and the Manchu word has rather a different meaning, the Ural-Altaic value of the Manchu word seems to be very little if none.

Case 13. A new stem \**p8l* for rendering the idea of “touffu,” “dru”; if the hypothesis of the loss of the initial *p* is accepted, then the Tungus and Mongol parallels are valid. However, in the Northern Tungus and Southern Tungus dialects it seems to be somewhat confined to a limited number of words and dialects, in which it looks to be of Mongol origin when met in the form *utek*, and of Manchu origin when it has the form of *fuli*, *pule*, etc. It is difficult to show whether all of them may be brought to one and the same source or not. I. Zaxarov, I think wrongly, compared Manchu *fulu* with a Chinese word, but even in European languages it is met with as containing about the same consonants, as, e.g., “full,” “plenus,” “*poľnyĩ*,” etc. However, if the Manchu stem and the Northern Tungus stem are borrowed from the Mongol, then it is very likely that the actual stem is *ul* and thus the whole discussion is confined to the consonant *l*. In addition to this, it ought to be pointed out that in the Tungus language there are other stems (e.g., *žat*) with the meaning “full,” etc.; while the stem *ul* figures in the words with the general idea of “exceeding.”

The above-demonstrated six cases may suffice to show the reasons why the class of these cases is not analysed in the coming sections. My intention does not consist in correcting, from the point of view of Tungus languages, the authors who have proposed various parallels, and therefore I do not need to go through all the cases.

For the time being, I need only those cases which may be suspected of being common for the Tungus and some other languages. Yet they will suffice for penetrating the analytical methods of theoreticians of uralaltaicology.

### 38. Cases with Initial and Intervocalic Labial Consonants

A. Sauvageot starts an analysis of the series of words and parallels found in other authors' publications and he increases them with his own parallels. The first three series (*vide supra*, p. 123), containing eighty-five cases, are closely connected with the problem of aspiration and bilabialization in Tungus. Since this problem in Tungus has nothing to do with the loss of the initial bilabial *tenuis*, the whole series might be dismissed from the analysis; but I will not do it, for parallels are brought forth to prove the common origin of words, a problem of certain interest by itself. Yet if we leave out of the discussion the problem of the loss of this consonant in all other languages except the Tungus language and temporarily agree with the creators of this hypothesis, then in the Tungus language, even the pre-Tungus language, these words might be received already without the initial consonant and afterwards pass through the process of aspiration and bilabialization.<sup>1</sup> Therefore I shall compare these series too.

After a minute analysis of the first eighty-five cases I have found that there are nineteen cases of Tungus parallels lacking; in six cases there are parallels only from Tungus and Samoyed, so that they have little value as evidence for the Ural-Altaic hypothesis; in twenty-three cases it was impossible to agree with the validity of the Tungus parallels; in twenty-four cases the parallels are present, but for various reasons are found to be invalid; and in thirteen cases the parallels are found to be worthy of further analysis and use.

So that, in so far as the Tungus language is concerned, in the problem of the Ural-Altaic hypothesis the outcome of the work done is not great. However, the cases rejected, perhaps, in some instances, may be saved when the parallels are completed from Tungus and other materials. In the cases to be discussed there are some met with which have been discussed twice under different numbers, so that the actual number of stems is below eighty-four.

<sup>1</sup>Indeed, such a hypothesis is not needed if we do not postulate the common origin of the Ural-Altaic languages.



The thirteen cases here analysed may be regarded as actually common words, but what their origin is and why they are common need special investigation which cannot be successful in all cases. Yet, when the common character of the words is established, I give some additional remarks when it seems to me desirable.

Case 2. G. Ramstedt's stem \**pδjδ*, \**pδsδ*, which means "the nest." In fact, the Manchu *feje* has corresponding words in the Northern Tungus; e.g., *uri*, *ujin* (Bir.), which in Tungus might be derived from the stem *VjV* or *VgV*; but whether *uri* and *feje* is the same stem or not cannot be asserted, for *uri* may be explained in a different manner. Yet the difficulty of this case is also that several other Tungus dialects possess other stems; namely, as shown below: (1) *ur* (Ur., *Castr.*) and *or* (Mank.) are borrowed from Buriat (Ur., *Castr.*); (2) *omuk*, *omunin*, *xamun* (Neg., *Sch.*), *umuk* (Ner.), *omokon*, *omok* (Tungus quoted by P. P. Schmidt), *omo* (Goldi, *W. Grube*); (3) (several other words found in more than one dialect, also "nest" of special description.) *Omuk* cannot be connected with *feje* nor *pδjδ*, but it is the most numerous in Tungus. After all, can we assert that the Tungus language ought to be connected through Manchu *feje* with other languages? On the other hand, the Uralian languages have, instead of *j*, different sounds, as *s*, *z*, *t*, *d*, etc., so that the comparison is confined to the first initial *p*, which itself is a hypothesis.

Case 6. The stem \**pδrδ* expresses the idea of "wheel," "circle," "round," "turn," etc.<sup>1</sup> *Forombi* (Manchu Writ.)—"to turn," is found, as well as a series of words like *foron*—"the turn," etc., *forgon*—"the rotation," "cycle," etc., and various Northern Tungus words, as *orol* (Ner., Bir.), *orul*, *arul* (Bir.), *orolko* (Ner.)—"to turn oneself"; *oroli* (Khin.) (Ur., *Castr.*), *horoli* (Ur., *Castr.*)—"the circle." This may be regarded as an established fact, but the stem is *oro*, and not *poro*, so that what is actually compared is *VrV*

<sup>1</sup>This case has already been rejected (*vide* Sec. 36). As a matter of fact, it contains more than one stem to be discussed and for this reason I have dissected it into two parts, one of which has been rejected and another here analysed as good for comparison.

or *r*. However, the Mongol stem *urb* has a different meaning<sup>1</sup>

Case 9. The stem includes the initial labial consonant and *r* with a broad meaning "das obere," "die höhe," "kopf," "spitze," "ende" in Samoyed and there are compared *foron* (Manchu Writ.)—"the top," e.g., "the top of the head," also "the top of a mountain" (P. P. Schmidt), etc. This is connected with *horon*, *oron*, of various Northern Tungus dialects, namely, *oron* (Bir., Kum., Khin., RTM, Ner., Barg., Mank.), *horon* (RTM) (Ur., *Castr.*), *oro* (Khin.) also *poro* (Olcha, *Sch.*) compared by all authors with Mongol *oro* and *horoi* (P. Pelliot). Thus the stem is *oro*. However, further steps are made and Mongol *oro* is compared with *orgil*, etc., of the same meaning. Moreover, in Tungus there is another stem, namely, *ojo* (Manchu Writ.) (Bir., Kum., RTM, Ner.) (Mank. *Castr.*), *pojo* (Olcha, *Sch.*), which has its parallels in Bargut *ojjo* (*Rudnev*). The case is not simple if we do not regard *oro* and *ojo* as one and the same stem. Such a possibility does exist, but the equation must be proved. A. Sauvageot concludes his parallels by a remark,—*"L'absence de toute étymologie fgr. sûre ne permet pas de décider si les mots considérés remontent à une origine ouralo-altaïque, d'autant plus qu'en turk, il ne nous a pas été possible d'établir une équation équivalente"* (*op. cit.*, p. 14). As compared with other conclusions, this one is unusually wise.

Case 20. The stem \**pδjδ* (Uralian) and \**fδjδ* (Mongol and Tungus) means "trembling poplar." The Tungus words which may be compared are *fulxa* (Manchu Writ.), *ulan* (Ner.), *uloka*

<sup>1</sup>It means "to change oneself," etc., and *hurba* (P. Pelliot)—*"retourner," "renverser."* Semantically, it may be compared with *ubai* (Manchu Writ.), *obai*, *obol* (Kum., Bir.)—"to transform oneself." The latter is perhaps found in Buriat (*Podg.*) in an "aspirated" form *xubila*, borrowed in Ner. as *kuvil* (*ga*), also known with the stabilized glottal *k* in Mongol and Turk. In this modification it is found in Manchu Writ. *kubulimbi* by the side of the bilabialized form *uubalambe* of Manchu Sp. with a slightly different shade of meaning. This case is interesting, for it shows the traces of diffusion of the idea of transformation practised by the shamans, amongst the Tungus. In this connexion it may be pointed out that the Tungus of Manchuria have *obai*, etc., from Manchu Writ. *ubai*, and the Tungus of Transbaikalia have *kuvil* from Buriat *-xubila*.



(Bir.), *huldan* (Neg., Sch.), *hulugdan* (RTM), *xulu* (Oroc., Sch.), *polo* (Goldi, W. Grube), *oleson* (Ur., Castr.). The latter may be compared with the Mongol series—*ulaso* (Buriat, Castr.), *uljasun*, *ulisun*, *ulasu*, etc. (Mongol). Since -*xa*, -*n*, -*dan*, -*gdan*, also -*so*, -*sun*, -*son* are probably suffixes, the stem is *ulV*, or even *VlV* in Tungus and Mongol. The question as to what the relationship between *p8j8* and *VlV* is answered in a positive sense by A. Sauvageot in the reason of the two hypotheses; namely, the original one of the loss of *p* and the new one of *l = j*. Indeed, as to the last one in principle there is nothing impossible in it, for, as is well known, *j* may appear in any combination as one of the moments of palatalization. A. Sauvageot gives another similar instance in Case 21, where the Uralian *j* is supposed to correspond to the Tungus *d*, which is not also a fact but a hypothesis.<sup>1</sup>

Case 32. The stem is *\*p8y* and means "souffler." In the Uralian languages *y* alternates with *b*, *v*, *w*, *u*, and zero. Amongst the Tungus parallels are found *fuka* (Manchu Writ.) and *fukanambi* (Manchu Writ.), which semantically seem to be connected with the idea of "round," "circle" and as, perhaps a secondary meaning, "bubble," also perhaps "protrude" and not "to blow." The Northern Tungus is also figured in the form *uvu*, already discussed by me.<sup>2</sup> A. Sauvageot has omitted the inconvenient parallel brought forth by P. P. Schmidt—*fulgjembi* (Manchu Writ.)—"to blow," connected by me with another stem *pul*, even perhaps *ul*. The Tungus words have the intervocalic

<sup>1</sup>The question is about *fodo*, *fodoxo* (Manchu Writ.) = *uda*, *udu*, etc. (Mongol dial.)—"the willow," "salix," which, in so far as I know, is not known in the Northern Tungus dialects using different words for the same plant. In Manchu, *fodoxo* refers to a specified kind of salix unknown in the territory of the Northern Tungus. Furthermore, A. Sauvageot agrees with the parallels given by P. Pelliot; namely, Mongol and Buriat *hicäsün*, *išäghün*, etc.—"the willow," through Turk *usak*, etc.—"the poplar." Leaving aside the question whether *ud* and *iš*, *ič* are of the same origin, as well as the question of semantic generalization (the willow and poplar are certainly plants), I want to point out that A. Sauvageot admits the possibility of *d = š = c*, so that the series of alternations is extended ad infinitum *š*, *ž*, *h*, *x*, *h*, etc. What remains as a fact is *d* in Zyrian and Votjak and Mongol, for Manchu *fodoxo* seems to be a non-Tungus stem and the initial *p* is a hypothesis.

<sup>2</sup>Cf. "Notes on the Bilabialization and Aspiration in the Tungus Languages."

*v*, which does not alternate with *y* and *l*, if the stem *ul* is also a Tungus stem, so that here we again have two hypotheses—the loss of *p* and *v*. But the Turk words do not seem to be convincing to A. Sauvageot. On this occasion he expresses his opinion as to the grammarians who have a tendency to overestimate the importance of onomatopoeic origin of words. This time, however, to every one's mind comes the idea of such an origin of many European words. This idea does not seem to be so repugnant, as it is to A. Sauvageot, that the parallels from the so-called Ural-Altaic languages come to the same line.

Case 46. The stem is supposed to be *jVpV* and means "wet" followed by a series of other equivalents like "macerare," "to wash," "le lavoir," "marshy place," etc. Since, as shown by A. Sauvageot, *j ~ v* and *j ~ l*, also *j ~ d*, the parallels are easily found. From Tungus are given only two parallels, namely, *cekpa* (Goldi, which needs to be checked up, for it is not found in other Tungus dialects) and *deptom* (Ur. Castr.), which is borrowed from Mongol, so that this case could be easily dismissed. However, in Tungus there is a stem which would not be omitted by A. Sauvageot, so I will use it too. It is *ulapkun* (Bir., Kum., Khin.) (W. T. C. Schiefner), *ulapkun* (Ner., Khin.), *ulapča* (as part. perf. from *ulap*) (Ner., Bir., Kum., Khin.). The serious objection is the initial *u*, but it may be easily dismissed under some pretext and under similar circumstances. In fact, the case is not so simple as that. We have *ula* (Ang., Samog. Tit.)—"to make wet"; *ulikča* (Tum.)—"wet"; and *ulgambi* (Manchu Writ.), *wulgamče* (Manchu Sp.)—"to wet," "to soak." In the first series containing *p*, we have it as a suffix of transitive "verbal" forms, which in some dialects is not needed, for the "transitiveness" is understood without a special suffix.<sup>1</sup> Yet -*kun*, -*ča*, and probably the Manchu -*ga* are suffixes too. So the stem is *ulV* and it has nothing to do with *jVpV*. The stem *dept*

<sup>1</sup>The suffix of the transitive form of verbs is *va* (*e*, *i*) (*V*), which changes into a tenuis occlusive bilabial through the assimilation *u*. It is a very common suffix, indeed.



I dismiss as one recently borrowed from Mongol. The Goldi word *cekpa* must be first checked up.<sup>1</sup>

Case 48. The stem is \**kəp* with the meaning "*l'écorce*," "*la croûte*," and afterwards "*la peau*." A. Sauvageot has lost the good opportunity of comparing Tungus *kiwa*, etc., already discussed by me on the occasion of Case I (*vide supra*, p. 126) and which in all Tungus dialects and Gilak means "birch-bark." However, it looks like a local phenomenon naturally correlated with the geographical area of *betula alba*.

Case 54. The stem is \**kəp*, with the meaning "*plat*" and with the further extension of "*handfläche*," "*fussblatt*," "*niedrig*," etc. The Tungus *həpsə* (Olcha), *həpsə* (Goldi), about which one must have a certain caution, for *h* is a mere aspiration, are also found; they are translated (W. Grube)—"*flach ansteigend*." It is not mentioned by P. P. Schmidt. In several Tungus dialects we have *afsa*, *hafsa*, etc.—"the low standing box" (cf. Mongol *apsa*). There is another word in Tungus which comes near to *kəp* and the idea of "*plat*"; namely, *kaptaka* (Bir.)—"the low flat box," but *kaptaka* is made of wooden planks which are in the Tungus dialects *kaptasun* (Bir., Khin.) (Khin. is from Bargut), *kaptayon* (Bir., cf. Dahur *kaptayon*), *kaptayin* (RTM from Yakut *xaptasyn*), *kaptagun* (Neg., Sch.), etc., which is not perhaps Tungus but probably Mongol, where it receives a great variety of derivatives from the stem *xabta* (cf. also Yakut derivatives), while in Tungus they are limited. However, there is one more stem in Tungus which might serve A. Sauvageot; namely, *kapaxun* (Manchu Writ.)—"flat," e.g., "nose"; *kaparambi* (Manchu Writ.)—"to become flat"; also *kapahi* (Oroči, Sch.) *kapasu* (Goldi, Sch.)—"the plank," "the board." With these additions it may be recognized that we have the stem *kəp*; but whether the Tungus stem is of Tungus origin or Mongol, and whether *kapa*, met with in Manchu, Goldi, and Oroči, is the same stem as *kaptaka* or not, are questions to be answered by further investigations. However, the Tungus dialects possess other stems for "flat." In this case,

<sup>1</sup>In *cekpa* if it is a Tungus correctly recorded word; *-pa* may also be a suffix.

as in other similar cases, the parallelism of different stems with the same meaning is an extremely complex matter.

Case 58. The stem is \**təp*, which means "*clou*." In Tungus a stem *tipk*, which originally meant "the nail," and probably "wooden nail" is met with. So we have *tipka* (Bir., Khin.)—"to fasten" (e.g., the cover of a box with the nails); *tipkasun* (Khin.), *tipkōn* (Neg., Sch.), *typhon* (Goldi, Sch.), *tipa* (Olcha, Sch.)—"the nail"; *tipkočīn* (Lam.)—"the wooden nail" (for fastening the tent).<sup>1</sup> Besides this series there is another series, still more numerous than the former one, of words designating "nail," but it should be connected with Mongol. These are: *kadasin* (Ner.), *kadasun* (Khin.), *kadahun* (Mank., Ur., Castr.), corresponding to *kadahan* (Buriat, Castr.), *kadasun* (Xalxa, Podg.), *xadayan* (Buriat, Podg.), connected with *gada*, *kada* || *kadayasun* (Mongol, Rudnev), whence *kada* (Manchu Writ.)—"the nail"; *kadambi* (Manchu Writ.)—"to fasten with the nail." There is one more word in RTM which is interesting—*toyohol* (RTM)—"wooden nails" (for fastening the board of the birch-bark canoe) connected with *toyoso* (Yakut, Pek.)—"the nail," "the wooden nail," "stake," etc. These three series of words are interesting, for all of them originally meant "the wooden nail," "stake," "pile," etc. All three are now met with in Tungus. A. Sauvageot has compared his series with Mongol *tab*—"tête d'un clou." A. D. Rudnev gives nearly the same meaning *tap* (Gorlos) || *tab*. However, various meanings of *tap* may exist; e.g., with the semantic emphasis on "head" and not "nail." Anyhow, in Tungus *tipk* *k* cannot be dismissed and the Mongol vowel is *a* and not *i*.

Case 65. The stem is \**pəxə* || *βəxə*, with a wide meaning; namely, "*gravier*," "*sable*," "*argile*," "*poussière*," "*suillure*," "*cendre*," etc. The Tungus parallels shown are *buraki* (Manchu

<sup>1</sup>The Goldi parallels from W. Grube's work, as *tukpa*, *tukpe*, perhaps need some correcting, as seems to be done by P. P. Schmidt.



Writ.)—"the dust"; *berten* (Manchu Writ.)—"the stain," none of which have corresponding words in other Tungus dialects. A. Sauvageot also gives *buru*—"flint." In fact, *buru* (Ur. *Castr.*) (W. Okhot.) (Barg., Ner.), *bur* (Bir., Kum.)—"flint," perhaps, in general, "stone which may be used for production of sparks," but its connexion with "*poussière*," etc., is rather artificial. *Buraki* being isolated in Tungus finds, perhaps, its best etymology in Mongol. In Tungus the words for the above-indicated semantic groups ("*poussière*," etc.) have nothing to do with \**pör* and *βör*.

Case 66. The stem is \**pör* (and *bör*), which is given a number of semantic equivalents, e.g., "storm," "steam," "fog," "rain," "snow," etc. There are good parallels from Manchu Writ. e.g., *burga*, *burgan*—"dunst," "*dampf*,"<sup>2</sup> and *burgasambi*—"to drift"; e.g., "dust," "clouds," "smoke," etc., and some other words of the same stem. However, in other Tungus dialects I do not know it. A. Sauvageot points out that the words for the designation of these phenomena were subject to borrowing from one group to another. For those who want to find "common words" with European languages, one may quote a long series of Russian words like "*bur'a*," "*buran*," "*purga*," "*uragan*," etc., which are of various origin, also "*bourrasque*," "*brouillard*," "*hurricane*," and many others. It is evident that here the question is more complex than the simple case of "common words."

Case 72. The stem is \**pör* and \**bör*, which means "*excroissance*," "*noeud*." As to the Tungus words quoted, we have different initial words; namely, in Manchu Writ. stem *bukt* with the meaning of "curved," "bent," "hunch," etc., with other derivatives like *mukč*, well known in several other Tungus dialects, and "the wart," "spunk"—*boyono* (Kum., Khin.), *boyoto* (Bir.), *moyo* (Bir.); while *bokoto* (RTM), (Neg., Sch.),

<sup>1</sup>There ought also to be added *borton*, *bortonombi*—"dirt," to dirt which probably must not be connected with *buraki*, but perhaps compared with *boršin* (Mongol, Rudnev)—"dirt," "dust."

<sup>2</sup>This word is lacking in I. Zaxarov's dictionary. The translation does not seem to be absolutely exact; cf. *burgasambi*. This word looks like that well known in Asia for "snowstorm" and "dust storm."

*bogoto* (Neg., Sch.), *boxto* (Goldi), *bokto* (Ner.), etc.—"the cone" (of a coniferous tree). Yet there is a series of words, like *bukačan* (Bir., Kum.)—"a small island," *bukačan* (Ner.)—"a small hill," "hillock," "heap"; *bukčan* (Lam.)—"a small island"; in all these words we have thus the idea of something protruding, coming out. It ought to be noted that the "spunk," "cone," and even "hillock" in their translations into the local (Siberian) Russian spoken by the natives is usually rendered as *šiška*. I do admit that such a misunderstanding was possible in many a case. One may close one's eyes to the phonetic differences and semantic discrepancies only when one leaves oneself to the charm of finding common words. They do, indeed, need careful and accurate analysis.

Case 83. The hypothetical Tungus stem \**börk*, *lk* of which is admitted to be Samoyed *l*, means "*gosier*." In Tungus the usual form is *biłga*, *biłya*, whence Goldi *biłža*. The Manchu Writ. *biłxa* (*biłya* of Manchu Sp.). However, in Samoyed the stem is *fal*.

Case 84. The stems are \**βör*, *βör*, with the meaning "*conduire*," "*mener*." The case is said to be "*le plus singulier*." The Tungus parallels are *fude* (Manchu Writ.) (let us remark that they are accepted by G. Ramstedt and P. Pelliot), which have a very special meaning connected with the customs of meeting persons, sending presents, dowry, etc., also expedition of official documents, etc., all of which, with a certain effort, may be brought to the idea of "*conduire*." Another word is *udā* (Ur. *Castr.*)—"to escort," already connected by M. A. Castrén with Buriat *udenep*, and compared by P. Pelliot with *hüda* (mediæv. Mongol). The Manchu word seems to be a Mongol word used for ceremonial and office occasions which is frequently met with in Manchu. Whether the Mongol stem *udV* or *ud* may be brought to the hypothetical stem \**βör* or not will be shown by further investigations in Mongol; but before it is



shown, the stem must be considered as *ud(V)* and not *βδ18*. The parallels from Turk are lacking. It may also be noted that specialists in the Uralian languages have already connected this stem with the Indo-European *\*wedh*. Theoretically speaking, such instances must be rather frequent.

The analysis of the above-mentioned cases with the initial and intervocalic labial consonants has shown that out of thirteen cases which could be analysed as instances where the possibility, both from the phonetic and semantic points of view, of common origin or at least common character may be suspected. However, in Cases 2, 6, 9, 20, 32, and 84 the hypothesis of the loss of the initial *p* greatly reduces the validity of parallels from Tungus. Some other hypotheses as to the alternation of consonants still more reduce the validity of these parallels. In some cases, as, for instance, Cases 6 and 9, the semantic extension is so broad that the value of parallels becomes very doubtful. Practically speaking, in all cases except Case 84, there are at least two hypotheses brought forth. However, even under these conditions A. Sauvageot could not find parallels for all "Ural-Altaic" languages; e.g., in Case 9 the Turk and Finn parallels are lacking, in Case 32 the Mongol parallels are lacking, and in Case 84 the Turk parallels are lacking.

If we agree with A. Sauvageot as to the validity of his hypotheses and ignore the fact of data in some cases, then we may have the following series:

- Case 2, Manchu word *feje*—"the nest"
- Case 6, Tungus *oro*—"the idea of circle"
- Case 9, Tungus *oro*—"the top," "summit"; Turk and Finn parallels lacking
- Case 20, Tungus *ula*—the name of a tree
- Case 32, probably onomatopoeic; Mongol parallels lacking
- Case 84, Mongol *uda*; Tungus parallels lacking

Cases 46, 48, 54, 65, 72, 58, and 83 are free from the hypothesis of the loss of the initial *p*, but hypothetically established alternations are met with in Cases 46, 72, and 83; the semantic hypotheses are met with in Cases 46, 65, and 66; in the following cases the parallels

are lacking: 72 (Turk), 58, and 83 (where only Tungus and Samoyed are found); in Cases 48 and 54 the Tungus is given by me. If we agree with all the hypotheses proposed by A. Sauvageot, we shall have:

- Case 46, Mongol *dep*—"to wet"
- Case 48, Tungus *kiva*—"birch-bark"
- Case 54, uncertain Tungus *kapla*—"the plank"
- Case 65, uncertain Manchu *buraki*—"dust"
- Case 66, Manchu *burga*, considered by A. Sauvageot as a loan-word
- Case 72, Tungus *boko*—"the idea of protruding"
- Case 58, Tungus *tipk*—"the (wooden) nail"; only Samoyed parallels
- Case 83, Tungus *bilg*—"the oesophagus"; only Samoyed (doubtful) parallels

So if we agree with all A. Sauvageot's hypotheses, but exclude all cases which are not Tungus and met with only "like Tungus" words (stems) and all cases where there are no parallels in other languages, we shall receive a short list of five stems for the following words and ideas,—“circle,” “top,” “name of a tree,” “birch-bark,” and “protruding” which may be regarded as “common” with the Ural-Altaic languages. The conclusion which may be drawn from these facts, in so far as the Tungus language is concerned, is evident, on which I could drop the analysis, but since these eighty-five cases represent only a part of all the cases I shall here analyse other cases too.

### 39. Cases with Initial and Intervocalic Dental Consonants

As shown, this series in A. Sauvageot's work is represented by twenty-eight cases. Out of these cases there are six cases in which there are no Tungus parallels; four cases which are out of discussion; twelve cases which have been analysed but were found untenable for further analysis, and six cases worthy of being discussed here.

Case 87. The stem is *\*l8r8*, which means “appui,” “soutien.” However, the semantic limits are extremely wide; e.g., “stark,” “kraftig,” “hart,” “vis,” “robur,” “stutzen,” “sich gründen,” “pfeiler,” “ursache,” “grund,” etc. The Tungus words quoted are *ture* (Goldi, W. Grube) (translated as “sich



*gründen*," "*fussen auf*") and which has been derived from Manchu Writ. *tura*—"the pillar." Other Tungus dialects possess this word too, so we have it in the sense of "post," "pillar," etc., used in shamanism for the spirits called on to descend or climb on the "post"<sup>1</sup> and in general when the spirits are called—*turu* (Barg., Ner., Kum.), *toro* (Bir.), *toru* (Mank.), which also give a series of verbs. In this sense it is also used in Manchu Sp. It is very likely, however, that this word is not of Tungus origin at all but Mongol. In fact, the particular "religious" complex connected with *tura* is not Tungus, but almost surely Mongol-Buriat. In Buriat we have *toron* used in the same sense.<sup>2</sup> The other words quoted by A. Sauvageot from W. Grube's work seems to be of Mongol origin, namely, *turgun* (Goldi)—"*ursache*," "*grund*," connected with *türü* || *terigun* (Mongol, Rudnev), compared, shall I add, by A. Sauvageot on another occasion ("*extrémité*," "*visage*," vide Case 88). Tungus *tura*, etc., is used as a technical term and in Manchu also as a "support for the beam" in the Chinese-type house and in the tent. The Turk parallels are lacking.

Case 88. The stem is *tVr* with an extensive meaning; namely, "*extrémité*," "*visage*," "*lèvre*," "*museau*," and even "*origine*," "*baumgipfel*," and "*épi*." In spite of this, in Tungus there is only one stem *dere* (*dèrè*, *därä*, *dara*), etc. (Manchu Writ. Sp.) (Bir., Kum., Khin., RTM, Ner., Barg., Mank.) (Ur., *Castr.*) (Neg., *Sch.*), etc.—"the face," which cannot be connected with Mongol *terigun*, but which may, perhaps, be with *čira* (Manchu, Writ.)—"the face." The latter may be connected

<sup>1</sup>Sometimes two posts with a cross-beam are fixed, as is done amongst the Tungus of Nerchinsk and Mankova and also other groups, which practice is still more common amongst the Buriats. Amongst the above-mentioned and other groups only one post or two posts without a cross-beam are erected.

<sup>2</sup>However, the question is complicated by the fact of Manchu *surun*—"the banner," which may also be a post with a top head, tuft, etc., i. e., like that used in Tungus shamanism. Cf. also expressions *surun vecen*, *surun vecembi* (Manchu Writ.). As may be seen, two different words of different stems and origin may be suspected.

with Mongol *čirai* (Xalxa, *Podg.*). However, which of them is Tungus and which is Mongol is difficult to say.<sup>3</sup> At any rate, *dere* does not seem to be originally a Tungus stem. If one desires, one may find some Tungus words under some of the above-shown semantic equivalents corresponding to *tVr*. Since it cannot be very promising from a linguistical point of view, I shall abstain from it.

Case 97. The stem is *\*tšj8*, for "Altaic" is given *tšl8* with the meaning "*écorce de bouleau*" (cf. Case 48, where the stem *k8p* is given). In Northern Tungus and Southern Tungus the stem seems to be *tVl*. The Mongol and Turk parallels and also Finn parallels are lacking, so that the question is about the comparison of the Tungus and Ugrian groups. Let us remark that both groups are living in the regions where birch-bark is a characteristic tree, and that there is another Tungus word for birch bark, as shown in Case 48.

Case 98. The stem is *\*tšl8* and *tšg* in which *l*, *g*, *v*, *w*, *u*, alternate and which means "*feu*" and many other phenomena (chiefly

<sup>3</sup>I will stop here on this question just to show its complexity. In Tungus we have a series of words for designating "the face"; namely, besides *dere*, we have *bada* (Ner., Barg.) (Ang. *Tit.*), etc.—"the face"; *bada* (Khin.)—"the handsome face"; *badi* (Bir.), *bada* (Ner.)—the placing for spirits in the form of a face (made of wood and sometimes of iron) (one must not mistake it for masks, as some ethnographers do, for besides "placing," masks are also sometimes used), i. e.—"the face." On the other hand, we have *derboki* (Bir., RTM), *derboki* (Khin.)—"the cover for the forehead" (woman's dress) (widely known in China and amongst all Tungus including the Manchus), *čirbikin* (Ner., Barg.)—"the reindeer bridle put on the forehead." No doubt we have here the stem *dVrb* with the meaning "forehead" or even perhaps "head." In some dialects we find *derebu* (Bir., Kum.)—"the pillow for the head," which is Mongol *der* || *dere* (Mongol, Rudnev) with the same sense. However, the Tungus words for "pillow" are different; namely, *tiru* (in most of the dialects), besides *čiru* (Manchu Writ.), *čiriptu*, *čiptu* (Goldi, W. Grube). There is a certain connexion between "face," "head," and "pillow." So in Tungus for "face" we now have *dere*, which seems to be a later stem; *bada*, which seems to be an old stem, and *čira*. The latter may also be compared with *čirai* (Mongol), *syrai*, *čyrai*, etc., of Turk. Perhaps the Tungus stem for pillow *tiru* may be connected with this series, which is not Tungus. It may also be pointed out that into the Tungus dialects now penetrate another term for pillow—*dere*, *derebu* (from Mongol), and *dere* for "face," besides the stem *dVrb* with a remote meaning "head." For "head" the Tungus dialects have *dili* and the Manchu *u3i* (*u3i*—Manchu Sp.) (in Goldi the Northern Tungus is preserved in the form *dili*, *šle*, etc.). Let us also point out that the Mongol stem *nur* is used for "face," besides *čiru*. These facts are sufficient to demonstrate that the question is not so simple as it is shown to be by A. Sauvageot.



verbs) including "flint." There are Tungus words varying *togo~tuwa*—"the fire," and another stem *dul* with the meaning "warm" (cf. Mongol *dulayakan*—"warm"), however in Manchu *dulembi* (Writ.), *tulembe* (Sp.) is "to burn," "to set fire to," "to make a fire," etc., which may be connected with the Mongol stem *dVlV* (Rudnev). The latter is met with in Turk; e.g., *tula* and in Yakut *tölön*—"the flame" (E. K. Pekarskii, p. 2768). The Mongol and the Tungus possess a great number of other stems for fire-burn idea; e.g., *gal*, *dörö*, *žorg*, *kul*, *žan*, *niŋ*, *il*. The problem to decide is which of the above-indicated stems are Mongol and Tungus, and in which other languages they are found, also which of them are borrowed and which of them are not.

Case 104. The stem is not shown, but it seems to be *tVnV*, with the semantic extension—"campus runcatus," "sarritus," "inhabitus," "contrée," "humus firmior," "erde," "stelle," "tundra." In Tungus there are at least two stems near *tVnV*; namely, *dunda* (compared by M. A. Castrén with Buriat *dunda*), met with, for example, in the forms *dunda* (Ner. Mank.) [*dunda* (Mank., Castr.)], *dunna* (RTM) (WT), and *dundra* (Spassky.), etc.,<sup>1</sup> and *tur* (Bir., Kum., Khin.), *turu* (Borz.), *tui* (Neg., Sch.), which correspond to two different complexes at least; namely, the earth is the middle world and opposed to the sea (*dunda* of the Mongols) and *tur*, *turu*—"the world in general" and "earth." The Goldi-Oлча parallels ought to be dismissed from the discussion, for the meaning of *dúenta* is not established.<sup>2</sup> The comparison with *dett* (Czek. Schiefner)—"the tundra," ought also to be dismissed, for we do not know the origin and exact meaning of *dett*.

Case 110. Only Tungus and Samoyed words are quoted. The question is only about the intervocalic *t*,<sup>3</sup> for the initial consonants are variable, being *β*, *b*, *k*, in Samoyed, and

zero, *w*, and *b* in Tungus. Of course, such alternations are possible, so I shall analyse Tungus parallels. A. Sauvageot quotes Manchu *atan* (Writ.) (bilabialized *watan*)—"the hook in general," particularly "trident" in "fish-hook," "harpoon," "fish-spear," etc., while the "fish-hook" is called *dexe* (Manchu Writ.); *buta* (Manchu Writ.)—"to hunt," "to carry on the industry of hunting," which may be referred to fishing as well (cf. *butxa*), *buta*, *bota*, *bata* (Bir.)—"to fish with the harpoon"; *butan* (Olcha, Sch.), *botori* (Goldi, Sch.)—"to chase," "to fish." The Bir. word is borrowed from the Goldi-Manchu in the sense of "hunting fish" (the only way to hunt is to take *t* with the harpoon); for other methods of fishing Bir. possesses different words.<sup>4</sup> This word, so far as I know, is lacking in other Tungus dialects. Its original meaning is certainly "hunting" and not "fishing with the harpoon." The connexion of *atan~watan* with *buta* is thus doubtful. Yet the Tungus word *boitten* (Czek. Schiefner)—"jagt," must first be made free of suffixes, which may be *-t+ten=ren*. As to the stem, it is probably *boju* when "worn out"—*boi*, in the emphatic pronunciation *boji*. Here another hunting term, however, interferes; namely, *boŋgo* (e.g., Ner.)—"to hunt *boŋga*" (some cervines). The term *boju* looks like one connected with *boju(n)*—the name for cervines, which constitute the greatest part of the hunting spoil amongst the Tungus who may be called "reindeer hunters." Besides *boju*, *boi*, there are some other words connected with other kinds of animals and other methods of hunting. As to the methods, they are sometimes expressed with various suffixes, e.g. *-s*, *-as*, etc., which give a new meaning—"hunting on short distance." One must know exactly the functions of these suffixes. What suffix *-t* means in the above-quoted *boitten* I do not know exactly. There is one more fact to be considered. There is a word *boji* (Bir.), which means "to steal up to" the game, people, etc.; i.e., it designates one of the methods of hunting. From the remarks given above it is evident that the comparison

<sup>1</sup>The consonant *r* is probably of secondary origin, as it is characteristic of this dialect in other similar cases.

<sup>2</sup>Perhaps it is merely *taiga*, forest, for *dúenta na* (Goldi, W. Grube), *dúenta anuo* (id.), and *dúenta* is associated with various animals (in shamanism, cf. I. Lopatin's and Šimkevič's publications).

<sup>3</sup>As a matter of fact, in Samoyed *t* also alternates with *r*.

<sup>4</sup>In the Khin. dialect *boju*, "to hunt with the rifle," is used for "hunting the fish" when the hunter uses the rifle. Indeed, it is a rare occurrence in their practice.



may be confined to Samoyed and Manchu, and practically to the intervocalic *t*, yet with the semantic supposition of identity between "harpoon" and "indent," etc.

If we agree with all the hypotheses proposed by A. Sauvageot, the analysis of the cases with the initial and intervocalic dental may be summarized as shown.

Case 87, only Manchu *tura*—"the pillar" and perhaps *turu*; Turk parallels lacking

Case 88, indefinitely Tungus *dere*—"the face"

Case 97, Tungus *talo*—"the birch-bark"; the Turk and Mongol parallels lacking

Case 98, Tungus *togo*—"the fire; Turk is lacking (according to A. Sauvageot)

Case 104, Mongol in different meanings; Manchu, Finn, and Turk are lacking

Case 110, Manchu *ata*—with a different meaning; all lacking except Samoyed

From the series of twenty-eight cases, it has been possible to save two cases; namely, *talo*—"the birch-bark" and *togo*—"the fire,"—which are defective as to the parallels with other "Altaic" languages.

#### 40. Cases with Initial and Intervocalic Glottal Consonants

The series with the initial and intervocalic glottal are numerous—seventy-six cases—the Tungus parallels of which are lacking in eighteen cases, twenty cases are dismissed after a brief analysis, twenty cases are dismissed after a detailed analysis, two cases are regarded by A. Sauvageot as loan-words, and fifteen cases must be analysed here. In three cases there are parallels only with Samoyed.

Case 116. The stem is \**k̆r̆r̆*, which means "instrument contondant," "l'action de couper," "de raser." In Tungus parallels three words are figured; namely, *kargimbi* (Manchu Writ.), *girim* (Ur., *Castr.*), and *girko* (Manchu Writ.). However, there is one more parallel omitted by A. Sauvageot, *girimbi* (Manchu Writ.)—to make an even line by cutting. So we have thus *giri* and *kargi*, both with a particular meaning of "cutting." The fact is that none of them is met with

in other Tungus dialects. Considering a very limited dialectal distribution of the stem *giri*, its special meaning (*girko* is a special knife for cutting paper, tissues, etc.), one naturally turns one's eyes to other languages. Perhaps this stem may be connected with the Mongol and Turk quoted by A. Sauvageot. [Perhaps *hirki* (Tum.)—the iron knife is of the same origin?] Two stems *giri* and *kargi*, are perhaps of different origin too. Owing to the reason shown above, these words cannot be regarded as original Tungus words, but as words borrowed, I think rather recently, from Turk and Mongol.

Case 117. The stem is \**k̆r̆r̆*, which means "cortex," "linderinde," "kruste," "korb aus lindenbast," "schorf," "schinden," etc. The Tungus words on this occasion are wrongly interpreted. First of all, *xorakta* and *xoromsa* are merely aspirated; second, they have particular meaning of bark from certain trees, i. e., these are names of trees. So this case might be easily dismissed, but I want to save it by bringing another stem—*kora* (Khin.), *kara* (Bir., Kum.)—the bark in general. I do not know this stem in other Tungus dialects as well as its origin. Perhaps it means "cover," "protective layer," etc., for in an epic poem I find it in the sense of walls surrounding a city.<sup>1</sup> However, considering the fact of its limited use (only two dialects), it will be much safer to look for its affinities in other languages.

Case 119. The stem seems to be \**k̆Vr̆*, which means "l'idée de tresser," very enlarged (Cases 118, 119, and 120 deal with the same stems and semantic groups); e. g., "coudre," "fishing nest," "spindel," "rouler," "tourner," "faire un détour," "éviter," "rôder," even "wheel" and "circle," and finally (Cases 119 and 120) "schief," "krumm," "courber les doigts," "plier," "retourner," etc. On this condition one may guess no one language of the world may escape being included in the "Ural-Altaic family." The European languages are very rich in such parallels. In Tungus one may find many words which can be ranged under such a wide meaning of

<sup>1</sup>The city walls cannot be considered as an element of the Tungus complex.



the stem *kVr*, but A. Sauvageot gives only Goldi *xeriligu* (Grube)—“*um etwas herumgehen*,”<sup>1</sup> and two Manchu Writ. words,—*kurbabmi* and *karkambi*, which must be carefully analysed; *kurbabmi*—“to turn from one side to another in the bed in sleeplessness”; it may also be referred to the horse when it rolls, for example, in the grass after being unsaddled; it is known in the latter sense in Mongol,—*xurbū- xurgu* || *koryo-*, *kürbe* (Rudnev) (cf. Buriat *xulbu*, *xulbe*, Podg.) and Bir. and Khin. when it is referred to the drunken people rolling down the earth. The idea is clear—the rolling from one side to another. In Northern Tungus and Manchu it has a very limited use. *Karkambi-sich winden* is differently translated by I. Zaxarov and in a sense which has nothing to do with *kurbumbi* and even the idea of “tresser.” Thus the only word is *kurbu*, which looks like a Mongol word used in a limited group of dialects and in a particular sense.<sup>2</sup>

**Case 121.** The stem seems to be *kVr*, which means *bord*, *rive*, *berge*. The Tungus words given are Goldi *kera*, *Kerá* (W. Grube) [*kira*, *kera* (Sch.)]—“*ufer*,” “*küste*,” “*rand*” (the latter seems to be an extension of meaning), corresponding to *gere*, *geren*—“*steiles*,” “*unterwachstenes ufer*” (this has not been compared by P. P. Schmidt). The latter is met with in other dialects; e.g., *giri* (Neg., Sch.), *geri* (Bir.)—“the shore,” “coast,” “bank.” Another word is met with too; namely, *kera* (RTM)—“the side,” “by the side of,” etc., probably borrowed from Yakut *kyry* (Pek.)—“side,” “board,” perhaps corresponding to *kira* (Bir.)—“the side,” “slope.” A. Sauvageot also quotes *girdan* (Manchu Writ.)—“*raum*,” “*saum*,” etc., which evidently has an entirely different meaning and probably origin. It can be connected with the above-discussed (Case 116) *girimbi*, for *girdan* is referred to the pieces of meat, tissue, etc. Cf. also *girsamda* (Ner.)—“long pieces of tissue, attached to

<sup>1</sup>From the text given by W. Grube it is evident that “*herumgehen*” is referred to *pulsi* (“to go”) and not only to “*heriligu*.” The latter is the “gerundial” form, I think, of a certain aspirated stem (*erili*) which has nothing to do with the stem *kVr*.

<sup>2</sup>Cf. Case 6, p. 133. The stem *urb* seems to go together with *kurbn*, as it has been shown in the case of *ubal-kubil*; cf. also *hurba* (Mediaev. Mongol).

the placings for spirits” (shamanistic), etc.—“the ribbon,” if it is not a recent loan-word.

**Case 122.** The stem is \**k8r8*, which means “*brûler*,” “*roussir*.” The Tungus parallels are not well selected, so *kirumbi* (Manchu Writ.) is translated “*in der Brunst sein*.” Indeed, in the European complex and in a metaphoric expressions “*Brunst*” presumes “hot,” “burning” desire, while in the Tungus complex it is not so, and it has a simple meaning; namely, “to chase a female” or what a male is doing with a female (the *kirume baĩmbi* expression in Manchu is clear) and just owing to this Manchu Writ. in the dictionaries is poor in derivatives, while it is not so with the Manchu Sp. The verb is isolated. At any rate it has nothing to do with “*brûler*” and “*roussir*.” The Tungus complex must not be looked at through the prism of the European complex. The Goldi *xuru* and its derivatives are well known in other Tungus dialects, as formed with the stem *ur*, in aspirated (*hur*, *xur*) and bilabialized (*fur*) forms (also known in Turk and Mongol) connected with the idea of growth of plants and animals and their maturation. So this word must be dismissed for the reasons of its phonetic character and meaning.<sup>1</sup> Some other parallels from Manchu Writ. are also found, namely, *xarimbi* and *xarkasi*. The first one may be supported by *xariku*—“the iron” (for ironing). Its meaning is not “*brûler*,” which is rather a secondary meaning, as the effect of an application of a hot iron, but “hot,” as it is perhaps in *xarkasi*—“a kind of fever.” In Bir. it is met with as *hariv* (*v* is suffix)—“to make warm” (not “*brûler*”), but it is borrowed from Manchu. Yet, in so far as I know, it is not met with in other Northern Tungus dialects. Some parallels perhaps may be brought forth from other languages, but I like better to abstain from introducing them in order to avoid mistakes.

<sup>1</sup>Indeed, only some and not all fruits when ripe become red and brown-coloured. Many of them, and especially green plants and animals, do ripen without “*roussir*.” The Goldi use the stem *ur* (*xur*) in the sense of “ripe.” By the way, the European idea of “ripe,” also, is not so closely connected with the idea of “*roussir*” and “*brûler*.”



Case 125. The stem is \**k8r8*, which means "*insectes volants*." What is actually seen from the parallels is that in the Finno-Ugrian languages the stem is *kar* for "the fly" and in Tungus the stem *garm* is used for "the mosquito." The series given by A. Sauvageot must be completed by *galma* (Manchu)—"the mosquito," also a series of words,—*monmaktá* (Ner.), *manmakta* (Bir.), *nánmakta* (Ner.), *nganmakta* (Neg., Sch.). In this series it is possible to see the original stem *nanma*, for the series with *r* or omission of *l* seems to be a further modification of a certain original stem in which the second consonant was alternating (*r~l~zero*). The initial *η*, so frequently met with in languages of this part of Asia,<sup>1</sup> where the Tungus are living, might easily give the forms with the initial *g*, *n*, and *m*. Indeed, on my part it is a mere hypothesis. Although the fly and the mosquito are both insects, yet the difference between them is very essential, especially for people living in the forest. The stems *kár* and *nanm* seem to be different too. Yet the Turk and Mongol parallels are lacking.

Case 127. The stem seems to be *kVr*, which means "*la notion de ceindre*." The Tungus evidences are represented by *korre*, *korreča* (Goldi), and *kurča* (Olcha)—"wand." Other Northern Tungus dialects and Manchu evidences, also their connexion with Mongol, are shown in my other work ("*Social Organization of the Northern Tungus*," *op. cit.*, p. 34, footnote). The term in Tungus and Manchu is connected with the cattle-breeding complex, so it has not possibly originated on the Tungus soil. It may here be added that the European languages possess a great number of words which may be attached to the same stem. Some of them may be regarded as simple borrowings; but for most of them we have no evidences, so they are often regarded as "Indo-European."

Case 129. The stem seems to be *kVr*, which means "*la grue*." Tungus words may be classed into two groups; namely, *karaf* and

<sup>1</sup>The initial *η* has been supposed by some authors to be a Palæasiatic consonant. Personally, I cannot share this opinion, for the reasons given in Part One of this study.

*kurčan* (Manchu Writ.) Although both of them contain *k* and *r*, it is not clear whether they have originated from the same stem or not, especially in the presence of Mongol parallels. The Turk parallels are lacking.

Case 135. The stem seems to be *kVl*, which means *larus*. The Tungus parallels given by A. Sauvageot may be increased to four instances; namely, *kilerka* (Lam.)—"the sea lark"; *kular* (Lam.)—"the gull"; *költökan* (Neg., Sch.), *käldäski* (Tung. Sch.)—"Gulo borealis." However, in other dialects the words are produced from other and numerous stems. The Turk parallels are lacking.

Case 139. The stem seems to be *kVi*, and *kVl*, the meaning of which is very wide; e.g., "*stria lucis*," "*morgenröte*," "*aurore*," "*sonne*," "*hell*," "*clar*," "*hitze*," "*wärmen*," "*abgekocht*," "*rein*," "*er glänzt*," "*lumière*," "*demain*," "*matin*," "*sauber*," "*heiter*," "*blenden*," "*weiss*," "*rayonner*," "*reflect*," etc. The Tungus parallels are not all well selected. (I) *xulžu*, *xulžuxa* (Goldi, W. Grube)—"*wärmen*," "*abgekocht*," requires several remarks. The Tungus stem is *ulV* (in Goldi sometimes reduced from *xul* to *ul*, cf. *ulsi*, *ulsire*—"kochen"); e.g., *uŋo* (Mank.), *ulo* (Ner., Bir.) (Neg., Sch.), *ula* (Ur., Castr.), *ulo* (Ur., Castr.), *olo* (Khin.), *ulo* (Barg., Poppe), etc., used in a narrow sense of "warming" for cooking. Another series is *holičen* (Neg., Sch.)—"to warm"; *xuliči* (Orochi, Sch.)—"to warm"; *xulu* (Tit.)—"to melt" (e.g., lead) with derivative *xulužin*—"warmed," "warm," etc., where the stem is *hol~xul*. The Goldi words when made free of (verbal) suffixes may be reduced to the stem *xul* with the above-given meaning "to warm." Here the stem and meaning are the same.<sup>1</sup> I now assert that the words of the second series have originated from the first one, *ulV*, through the process of aspiration. To show it, we have the following evidences: (1) The Tungus dialects

<sup>1</sup>I have not mentioned here another Goldi variation which is found in P. P. Schmidt's vocabulary; namely, *pulči* (Olcha), *poki* (Goldi)—"to warm." Such a parallelism of aspiration and bilabialization in Goldi has already been discussed. If we agree with A. Sauvageot's parallels and hypotheses, then the stem would be *pul* and not *kul*, and for this reason alone the parallels should be dismissed.



which do not practise aspiration of the initial vowels for the idea of "warming" (without specification of "cooking") use different words, e.g., of the stems *oku*, etc., *nama*, and others; (2) these dialects might have the stem *xul* in the form of *kul* (when borrowed). However, they have no such words in the sense of "warm";<sup>1</sup> (3) the initial *h*, besides *x* and even *p*, which evidently point to the process of aspiration and bilabialization are also met with. The history of the stem may be restored as shown—*xul* has appeared in the "aspirating" dialects with the meaning "warm" (and not "cook"); the old stem *ulV* persisted in all dialects in the sense of "warming for cooking," or "to cook"; i.e., this is a case of "doublets" through the "aspiration" in the "aspirating" dialects. Therefore the Tungus stem *xul* cannot be compared with *hV*. (II) The Tungus parallels *xajéma*—"rein," and *xajakatten*—"er glänzt" (Czek., Schiefner) are mere misunderstandings. Both of them have originated from the well-known Tungus stem *aja* (aspirated *haja*, *xaja*)—"good." In the first case, the suffix *-ma* may be compared with the "verbal" adjective; *ajama* referred to weather that was good. In the second case, *aja* served as a "verbal" stem in the sense "it is (weather) keeping good," the sign of which was "er glänzt." Indeed, these Tungus expressions are heard every day when the weather is good, and such misunderstandings are also known from the records of persons who are not very familiar with the language. (III) *Galga*, *galgan* (Manchu Writ.), *gal-gal* (Goldi, Grube)—"rein," "sauber," "hell," "heiter," "heiteres wetter"—is merely "good weather"; *galga* (Khin.)—*id.*; *galambi* (Manchu Writ.), *gala* (Kum.)—"to become good weather." These words cannot be interpreted as originated from "stria

<sup>1</sup>In an aspirating dialect, RTM, I find a word *kulu*, *kulli*—"the soil mixed with the ashes after the forest fire." This stem is also met with in the Ner. dialect in the word *kulorošo*, evidently from the verb *kuloro*—"to burn," so the stem is *kulV*. In fact, *kuli* (RTM) (Tum)—"to burn," "to strike a spark from the flint," etc. In order to clear up some possible misunderstandings I shall now bring another Tungus word of the stem *kul* and idea of "warm," *kulan* (Bir.)—"the warmed stove-bed" (cf. Chinese *kan*, Manchu *nayan*) from *xulan* (Manchu Writ.)—"the chimney," "smoke-pipe," cf. *kulan* (Mongol). The "stove-bed" is warmed by a system of smoke pipes.

*lucis*," etc., for good weather is called *galga* during the night-time as well. (IV) *Gilxa* (Manchu Writ.)—"the clear, good weather"; perhaps this word is formed by analogy with *galga* from *giltaxa* (*vide infra*), or it is of the type of double words, as is common in Asiatic languages. However *gilta* (Manchu Writ.), with all its derivatives, definitely means the idea of "glitter," "shine," "gleam," etc. The same stem, and with the same meaning, is met with in other Tungus dialects, e.g., *gilta* (Khin., Ner., Bir.), *kelta* (Neg., Sch.)—"to gleam," "to shine" (perhaps, *kelurin* of RTM is connected with it), *giltas* (Barg. Poppe)—"shine," "glitter." All these words are connected with Mongol *gilte*, *gilbel*, Buriat *gelelet*, *gilbel*, etc.<sup>1</sup> It may also be added that in Yakut *kilbiän* (Pek.) is "glitter" "shine," etc. Besides the words shown above, the Tungus dialects possess some other words of different stems; e.g., *ila*, *ɳari*, *šasana*, and others with the meaning of "shine," "glitter," etc. Yet there are also many other words of different stems for designating special kinds of shining, gleaming, etc. Considering the geographical distribution of the Tungus dialects which use *gilt*, *kelt*, *gilb*, as stems, the meaning and partial parallelism in Tungus, also the fact that this stem is met with in Mongol, Buriat, and Yakut, I think in Tungus it has been rather recently introduced from the Mongol language, direct or through the Manchus. It may be here added that the translations given by A. Sauvageot and myself are numerous enough for choosing some of them which may suit the stem *hV*. Yet attempts have already been made at the comparison of the Finno-Ugrian words with the Indo-European words.

Case 146. The stem is not shown, but it is supposed that the Finno-Ugrian *y*, *j*, *i*, *g*, correspond to Tungus *l*. The Tungus words are *kolla*, *kulin*, *kulikan*—"the snake." *Kolla* is an emphatic pronunciation of *kuli*, *koli*, also used in other Tungus dialects when the speaker is surprised, or wants to impress the hearer. *Kuli* is "snake" and other crawling

<sup>1</sup>*Gilbal* is known in the Tungus dialects neighbouring the Mongol-speaking groups in the sense of *gill*.



creatures like the snake; e. g., large worms. It is met with in all Tungus dialects, *-a-n*, and *-kan*, sometimes *-kačan*, *katkan*, *-mā*, etc., are suffixes. Indeed, the comparison of Finno-Ugrian with Tungus is doubtful. Why not compare, for example, the coluber?

**Case 147.** The stem is *\*kšmš*, which means "volite," "position renversée, sans dessus dessous," etc. In Tungus too many words may be found with nearly the same meaning and nearly with the same stem, but it would take too much space here to enumerate them. In the parallels given by A. Sauvageot, some of them have originated from different stems. This is the case of the stem (1) *kumt* in which *l~č~s* and *m~n* may alternate, with the meaning "to cover" (Bir.) (Czek., Schiefner); (2) *kumč*—"bunch baked" (Manchu Writ. and several other dialects which have this stem from Manchu); (3) *kumd* (Manchu Writ.)—"hollow," but, according to I. Zaxarov, of Chinese origin and is not met with in other dialects. So at least two stems in Tungus seem to exist with a different meaning, and only one of them has received certain popularity. The question is still complicated by the presence of *t*, which cannot be dismissed from the stem *kumt*, which was originally perhaps *kut* and meant "to cover," without interfering with the idea of "volite" and "renverser."

**Case 182.** The stem is *\*jškš*, which means "sapin." However, in Samoyed it is *je*, *küe*, *tüe*, *čwe*, which are treated as the result of the loss of *k*. Yet it must be remembered that the stem *\*jškš* was found from a broad semantic series comprising "pine-tree," "fuel-tree," "forest," etc., and the variations in Finno-Ugrian and Turk are not insignificant. In Tungus the stem is *žagd*, in all dialects and in the same sense—"the pine-tree"—except Tum., where it is *žald* (as to the record, I am not sure of it). So the identification of *žagd* with *\*jškš* presumes a series of hypotheses, but since *ž* is not met with in other languages and it is not shown that it must correspond to other various sounds, also since *gd* is an essential element of the Tungus stem,

it will be much safer to leave *žagd* with the Tungus languages.

**Case 186.** In Samoyed and Manchu (whence in Goldi, Olcha, Orochi), "black" is expressed by the stem *sax* (Tungus) = *sag* (Samoyed). It ought to be noted that in almost all other Tungus dialects we have the stem *hoŋo* (with modifications).<sup>1</sup>

**Case 190.** In Samoyed and Tungus, *IVkV* is found as "arrow." However, the Turk words compared have no initial consonant.

If we agree with all phonetic and semantic hypotheses proposed by A. Sauvageot in reference to the Tungus parallels, the analysis of the cases shown above brings us to the following results:

Case 116, Mongol or Turk *giri*—"to cut"

Case 117, doubtful Tungus *kVra*—"the birch-bark"; Mongol and Manchu parallels lacking

Case 119, doubtful Tungus *kurbu*—"to roll from one to another side"; Turk lacking

Case 121, Tungus *k(g)VrV*—"the side," "board," "shore"

Case 122, Manchu *xar*—"hot," to "burn"; Mongol lacking

Case 125, to be dismissed; Mongol and Turk lacking

Case 127, doubtful Tungus *kVrV*—"the fence"

Case 129, Tungus *kara*—"the crane"; Turk lacking

Case 135, Tungus *kVlV*—"the lark," "the gull"; Turk lacking

Case 139, doubtful Tungus *gVlt*—"to shine," "to gleam"

Case 146, Tungus *kVlV*—"the snake," "snake-like"

Case 147, doubtful Tungus *kVmt*—"to cover"

Case 182, Tungus *žagd*—"the pine-tree"; Mongol lacking

Case 186, Manchu *saxa*—"black"; rare in Northern Tungus; only Samoyed

Case 190, Tungus *IVkV*—"the arrow"; only Samoyed

<sup>1</sup>In Northern Tungus dialects the stem *sax* seems to be preserved and used in the sense of "dirty," "muddy" (e.g., water), so we have *sāhi* (Ner.), *s'ohi* (Khin.), *soku* (Bir.), *siki* (Ur., Caste.). The stem *hoŋo* was not perhaps originally a Tungus stem. I will abstain now from other interesting parallels and suggestions.



There are thus found five Tungus stems which are common with other languages; namely, *k(g)VrV*—"the side," "shore"; *kara*—"the crane"; *kVIV*—"the lark," "the gull"; *kVIV*—"the snake"; *ʒagd*—"the pine-tree."

#### 41. Cases with the Initial and Intervocalic *ŋ*

A. Sauvageot has given twenty-four cases of stems with the initial and intervocalic *ŋ*. Six cases have been found with Tungus parallels lacking, four cases which ought to be dismissed from further analysis without detailed analysis, eleven cases which ought to be dismissed after a minute analysis, and four cases as worthy of further discussion.

Case 196. The stem is like *ŋVIV*, which means "to be frightened," "to fear." On this occasion G. Ramstedt has formulated that Samoyed *w*~Tungus *ŋ*~Mongol *g*~Turk *k*. However, it ought to be pointed out that in Tungus the alternation of the type *n*~*g*~*w* is known too,<sup>1</sup> so that the parallels must be taken with great caution. This time, as a semantic link between the languages "wolf" and its particular name *ŋaluki* is used. In fact, some dialects may use it in reference to the wolf—*ŋokuli* (Goldi), *nèteku* (Tum). However, its meaning is not "wolf," but "coward," "poltroon," "fearful," "faint-hearted." In Ner. and Barg. we have *ŋoloki*—"the coward." Indeed, this is a very common case of the formation of "nouns" with the help of the suffix *-ki*. During hunting and in children's stories the animals are very often called by such a "name."<sup>2</sup> The wolf is also called *amyačín* (Bir.)—"one who has a mouth"—also other "names," including "robber," and other humiliating expressions. For example, the bear is called *sopitaran* (RTM)—"one who goes to stool with berries"; *kongnorjo* (Mank.)—"heavy-black," and by other names of the same

<sup>1</sup>Cf. my "Bilabialization and Aspiration," *op. cit.*

<sup>2</sup>These terms may have nothing to do with the "religious" complex "fear," etc., as it is often pictured by ethnographers little familiar with the actual life and mentality of these groups, and who build up theories and give descriptions of groups adapted to their general conceptions by mere misinterpretation of fragments of facts known.

type. The use of the term based upon the stem *ŋale* (*gele*) is accidental, indeed.<sup>1</sup> The whole reasoning about the derivation of the word "wolf" from the verb "to fear" has a very learned appearance, but actually it is a mere misunderstanding. Many other names for "wolf" may be found. Even Goldi *ŋöla* may have an entirely different origin and a particularly local use. The Samoyed name for wolf, *ŋulada*, may only artificially be connected with *ŋale*, *gale*, etc.,—"to fear"—and for safety sake it would be much better to leave it alone, as A. Sauvageot proposes to G. Ramstedt when discussing in Samoyed soil *ŋulada* and *wuenoltau*. As a matter of fact, in all languages there are special terms for all existing animals, including "wolf." These names are often borrowed, lost, and changed. The names for animals sometimes exist for a very short time, like a kind of temporary fashion.

Cases 199 and 200. I have put them together, for the stems are postulated and the meanings are the same in these two cases. The stem is \**8ŋ8* which means "*la bouche ou les parties voisines de la bouche*" and "*ouverture*." The series comprises a wide range of words, e. g., "cheeks," "lips," "jaw," "hollow of the handle," "*porte*," "anus," "*béant*," "*ouvert*," "*avoir faim*," "*bailler*," "*mâcher*," etc. A great variety of sounds corresponds to the great variety of meanings. The analysis of all of them would bring us to the analysis of the anatomical terminology in Tungus, the problem not less complex than in some European languages (especially if the Latin language is unknown), so I shall point out only that the Tungus language possesses several words used as anatomical terms

<sup>1</sup>However, this stem in its "verbal" form is used for a hunting term; namely, *gaigda*, *ŋalegda*. They call by this word things and arms touched or destroyed by bears and tigers when they attack the hunter. The latter is also called by the same term. According to the custom, if a man fails to kill the animal, he must not hunt these animals and must not use the things touched by them during the hunting. These things are "unlucky" and the hunter is merely a "coward" and being so he is advised to avoid these animals. Some Tungus say that the animals smell the things they have touched and therefore they are not afraid to attack the second time.



and containing in their stems the intervocalic  $\eta$ . The question whether the intervocalic  $\eta$  was or was not in the original stem is a question of conviction or faith, for the history of Tungus is not yet known and no general laws of variations of these sounds exist. Let us take an instance discussed by A. Sauvageot; e.g., Tungus *amya*, *amya*, *amma*, *anya*, *anga*—"the mouth," may be produced by the process of formation from a certain hypothetic stem, as well as from different words (stems), e.g., *aman* (Mongol)—"the mouth," perhaps already engaged as *omun* (Tung.)—"the lips," also *amt~ams*—"to taste" ("to take into the mouth"), and lastly *anyar* (Mongol)—"the hole," "opening," etc. (in Mongol a "descriptive" term for "anus," has been given, while in most of the Northern Tungus dialects *amun* is "the excrement," "to go to stool," etc.), probably connected with Tungus *sanar*, whence in joking and "descriptive" language any opening in the human body may be called so. But *sanar* is not the only word used in anatomical terminology for "opening." In fact, for example, the nostrils are called *ulleka* (Bir.)—"the hole when something is broken"; *tupukta* (Mank.)—"the hole due to the perforation." Moreover, the words used for anatomical parts are found very often to be under prohibition, or mere avoidance, which is true not only of the sexual organs, but also of the anus and urinary organs, and not only among the "civilized" people, but also true of other organs and "barbarians." With the living dialects, it is difficult to establish at which moment a "polite" "nice," "descriptive," and "joking" term becomes a stable substitute of an older word. The semantic migration of terms from one to another organ is quite a common phenomenon, indeed. If we presume *ama* to be a modification of *anya*, perhaps it will be too risky to lose the original stem *ama*. Yet the words for "nose" from the stem *ono* (e.g., *onokto*—"the nose"; *onolo*—"the beck," "neb"), identified, perhaps absolutely wrongly, with

*oforo* (Manchu) (whence Goldi, etc.),<sup>1</sup> may be connected with *ongol* (Goldi)—"the hole," etc. Of course, it must be shown, for "the hole" in Northern Tungus is *sanar*, but *ongo* is met with in the sense of "spacious," "hollow," "empty," etc.,<sup>2</sup> while *ongo*, *ungu*, *no*, etc., is "the smell" (cf. my "Bilabialization and Aspiration," *op. cit.*). The latter etymology is more credible, but I do not propose it. The question whether *anya* ("entrenchment"~"mouth") is the same as *ongo* ("smell"~"nose") or not cannot be perhaps decided. I will not now go into further details, which are unnecessary for insisting upon the semantic and phonetic differences of the words quoted by A. Sauvageot. It is evident that these parallels cannot be used without being carefully checked up.

Case 202. The stem is not shown, but it is supposed to be an intervocalic  $\eta$  varying as *m*, *n*, and zero. It means "to forget." The Tungus stem may be brought to *omgo* in its variations analogous to *amga* (vide Cases 199 and 200)

Case 209. The stem is \*š $\eta$ š $\eta$ , which means *souris*, and may be regarded as similar to the Tungus stem used for designation of the same animal.

From the analysis of these four cases given above it may be seen that with the agreement as to the hypotheses proposed by A. Sauvageot we have—

Case 196, Tungus  $\eta$ VIV (*gVIV*)—"to fear"; Turk lacking

Cases 199 and 200, doubtful Tungus *anya~amga*—"the mouth," "opening"

Case 202, Tungus *omgo*—"to forget"

Case 209, Tungus *sinir*—"the rat," "mouse"; Mongol and Turk lacking

<sup>1</sup>Let us remark that in some Northern Tungus dialects the word for "nose" is found in an aspirated form, namely, *hono*, whence, according to A. Sauvageot's theory, the original (before the loss of the initial *p*) form must be *pono*, which is indeed not so.

<sup>2</sup>*Ungala* (Manchu Writ.)—"the ear hole," is disconnected with the idea of "hole," but it seems to be connected with the idea of "hollow"; e.g., "gun," "empty wood."



So there are three Tungus stems left,—*ŋVIV*—"to fear"; *omgo*—"to forget"; and *sinir*—"the rat," "the mouse."

#### 42. Conclusions

From the inspection of the above-given lists of words which conventionally may be recognized as being Tungus words, it may be seen that the outcome is not so great as the work that has been required for such an elaborate and large construction which greatly satisfied A. Sauvageot when he refers to a great number of parallels. One must not expect me to reproach A. Sauvageot for his lack of familiarity with the Tungus material and knowledge of dialects, which is evident from the previous sections, for this phase of his failure is a minor phase as compared with other phases. Yet the question whether A. Sauvageot is in full possession of the language or not has even no importance from the point of view of further investigations along this line, for to study one more language, even as a group of dialects, for a person devoting himself to this class of work is only a question of time. My idea is that even with a perfect knowledge of dialects one cannot accomplish the task put forward by A. Sauvageot by himself. The task itself would also be of little interest if proposed by A. Sauvageot alone, for theoretically it is hopeless; yet the interest in this task cannot increase owing to a great number of other contributors; but there is another side of the task which attracts my attention; namely, the mechanism that is responsible for giving life to the idea of such a task—an ethnographical subject—and the positive results obtained, i.e., correctly established common words in a group of languages, which throw light on the problem of the formation of ethnographical complexes and the history of ethnical groups and units. Owing to this, I shall proceed with my further analysis of the case discussed.

In the Introduction A. Sauvageot has addressed his reproach to the opponents of the Ural-Altaic hypothesis who, according to him, confine themselves to the *polémique verbale*. However, in his conclusion he goes further. True, he admits and expects that the majority of his parallels will not be valid; but, according to him, even a small number of them (*une poignée*) will be sufficient "à

prouver que les langues considérées ne sont pas étrangères les unes aux autres" (*op. cit.*, p. 139). Later he becomes aggressive towards his imaginary opponents and requests that the theoreticians must give a more satisfactory explanation of facts than that proposed by himself. Yet he admits that they may be successful even in this task, but he covers himself with a new warning to them "qu'ils n'oublient pas que c'est la totalité de nos rapprochements qu'ils devront interpréter d'une autre manière" (*op. cit.*, p. 141); i.e., he practically refers to "the great number of parallels" and his phonetic "laws." He says: "Nous ne croyons pas qu'il soit possible de trouver entre deux ou plusieurs groupes d'idiomes totalement étrangers les uns aux autres un ensemble de 'coïncidences' aussi considérable et aussi développé que l'ensemble des rapports lexicaux présentés ici" (*op. cit.*, p. 141).

In this statement of the problem there is nothing new in so far as argumentation is concerned. If there is any difference, it is that of quantity of cases. However, as shown, *polémique verbale* has a certain reason too. Under this name is evidently figured the general theoretical treatment of the problem which for A. Sauvageot is as clear as it may be to a believer. However, just from this point of view his position is not safe, and I shall touch this point once more. The above-given analysis of his parallels was not a *polémique verbale* and it has shown that the fundamental idea of A. Sauvageot as to the *parenté des langues* ought to be taken under suspicion more than it has ever been taken before, at least in reference to the Tungus languages.

From the very beginning of the present work, I have kept before myself the aim of defining the relationship between the Tungus languages and other languages in basing my work on the parallels brought forth by A. Sauvageot. This relation is confined to a few words which are common. From the series presented by A. Sauvageot we are facing fifteen words which are common for Tungus and other languages, but, let us add, which have been admitted to be so on the condition of agreeing with the hypotheses of A. Sauvageot. If the latter are not so, then the number of common words will be still smaller. In fact, I have shown that



G. Ramstedt's hypothesis in reference to the Tungus languages cannot be admitted as valid, so that five cases out of the fifteen must be excluded. Some other cases where the phonetic alterations are only hypothetic and where the semantic variations are too wide must also be dismissed. Yet, in most cases (nine out of fifteen) the parallels from Turk are lacking. In a third of the cases the Mongol parallels are lacking. After all, there remain two words; namely, "side" and "snake," which are met with in all compared languages and which do not need any preliminary hypotheses to be admitted.

Such a small result is not surprising at all if we remember what has been formulated in reference to the common words in different languages. These two common words (there may be twenty and even fifty, as well) may have their common origin not only from the direct transmission through the mechanism of tradition from the populations spreading over the territory, but they may be a result of a mere chance of combinations and permutations, without speaking of migrations of words, etc. So that as a proof of "common origin" of languages compared they are not only too scanty (they may be much more numerous without having any value as evidences showing common origin of languages); but they cannot be convincing, for their history is unknown. It is evident that there will be many more "common words" if the Finno-Ugrian languages are eliminated; yet their number will be still greater if the Turk languages are eliminated. Their number will still increase if the Mongol language is eliminated, yet again increase if the Southern Tungus and the Northern Tungus are considered separately.

An impartial analysis of the parallels brought forth by A. Sauvageot leads us to the conclusion that the Tungus languages, in so far as the lexic analysis goes, must not be included into the "Ural-Altaic family" on the ground of existence of common words.

#### 43. Influence of Theoretical Presumptions of Evolution

The conclusion shown above is in a crying contradiction with the theory of A. Sauvageot and many other contributors to the hypothesis of common origin of the Ural-Altaic languages, so I have now to show what mechanism produces this theory.

We have already seen that the idea of the common origin of these languages did not result from the comparative analysis of the material, but it did come out of a general conception of language and prevailing ideas on evolution of cultural and biological (in a narrow sense) phenomena which were used as scientific guide. In this respect, the history of the Ural-Altaic hypothesis is very instructive, indeed. We will not go into the details of this history, which is known from works of other writers on this subject. It will suffice to point out that during the last and the present century there were several groups of evidences proposed and rejected. So, for instance, the original proof of common origin—the vocalic harmony—could not stand the criticism and appeared at last as a particular case of phonetic fashion in different degree affecting the languages here discussed without being a general phenomenon.<sup>1</sup> Another serious attempt was made by using the morphological elements as evidence of common origin, but after a minute analysis of facts, it had to give place to the evidences of lexic complex of these languages. What is typical of this situation is *the persistence of a tendency to find proofs* in spite of previous attempts which subsequently failed. This may be understood as due to the *desire of finding* conditioned by certain factors lying beyond the linguistic facts. Such a factor was the idea of an organic evolution of cultural phenomena as one opposite to the ideas prevailing during the previous period. So the linguistic facts were brought forth as evidence or even a simple illustration for proving a preëxisting idea,

<sup>1</sup>This problem is not yet cleared up and the phenomenon is not yet thoroughly investigated. The lack of historic data is one of the great hindrances for making up the picture of the process.



a theory. When the facts appeared to have been insufficient or inconsistent, other facts were brought forth maintaining the same idea and with the same success. The failure of these attempts and change of kinds of proofs result from two sets of conditions; namely, an impartial scientific analysis of evidences based chiefly upon the general theory of linguistics in front of which the evidences brought forth cannot be regarded as valid ones, and a critical attitude, which is not limited by the prevailing ideas as to the evolution of cultural phenomena. Both of them belong to the mechanism of "growth" of science and knowledge in general without which the science could not exist as a cultural phenomenon well adapted to the needs of ever-changing ethnical units (societies). In this respect, the "conservative" minds (the minds that simply stick to the existing ethnographical complexes and particular conceptions show a very typical attitude) are particularly hostile to the methodological and theoretical criticism and treatment of the problems and make, as, for instance, A. Sauvageot does, an appeal against *polémique verbale*, call for the facts and insist upon receiving other explanations if his own are rejected. As a *modus operandi* it calls to mind the old discussion of the evolutionists with the protagonists of the old theories of whom the latter requested the first ones "to deal with the facts" and "to explain."

#### 44. Phonetic Laws and Their Application

In the course of the analysis we have seen that there are two methods used which greatly help the selection of facts; namely, the phonetic "laws," which leave a free hand in choosing comparative material, and semantic extensions, which permit the inclusion of related ideas. Indeed, both methods are powerful tools when properly used, but the question is as to the method of their application,—How far may they be applied? Of course, the limits of practical application of the phonetic "laws" are strictly outlined in general treatises, but it is very often forgotten, so we shall remember them.

We shall use the instances of the languages here discussed. The correspondence of the initial Manchu *f*, to Goldi *p*, to some Northern Tungus *x*, *h*, and zero, found out of the observation of facts,

is beyond any doubt. However, as shown, there are many "exceptions" in which Manchu words with the initial bilabial correspond to Goldi words with the initial *x*, *h*, and zero; also the Manchu words without the initial bilabial correspond to Goldi words with the initial *p*. The number of facts disturbing the "ideal" correspondence is so great that one cannot consider it as a "law," as a law is understood in science. The facts cannot thus be explained and predicted by the reference to this "law," for the simple reason that it does not exist as a "law." Furthermore, when the facts are explained by these types of occurrences in addition to being increased with other hypotheses, the explanation may happen to be absolutely erroneous and misleading, as it actually happens with all cases when Tungus *h* is explained as the loss of the initial *p*. When such a "law" is practically applied to the establishment of parallels, it results in the production of an absolutely artificial picture of actually non-existing relations. Let us take another instance—the frequent correspondence, in Tungus, of *η*, *ηg*, *g*, *w*, *u*, etc., which also greatly facilitates the finding of common words. The initial *g* of Manchu in many instances corresponds to *η* of some Northern Tungus dialects, but not all Manchu *g*'s correspond to the *η* of the same Northern Tungus dialects. Owing to this, one cannot postulate that the word found in the Northern Tungus with the initial *η* will be found in Manchu with the initial *g*, and vice versa. This type of alternation does exist, but it is not a law, which may have force when one goes from law to the particular cases for establishing original sounds. Some Tungus dialects have developed another peculiarity; namely, the alteration of the initial *s* into *h* and *x*, but some other dialects have jealously preserved the original *s*. However, some dialects are not so particular about these sounds and use both of them in parallel. Let us take a "sibilating" dialect and compare it with a "glottaling" one. When we need to find parallels, we can go straight by comparing the words with the initial *s* with that of the other dialect with the initial *x* or *h* and thus practically to expect such an occurrence in all cases. However, this regularity has no reverse power,—*s* will not correspond in all cases to *x*, *x~h*, for there may be a different origin of *x~h*, e.g., through



"aspiration." Yet if one compares a "sibilating" dialect with another one which is not definitely "sibilating" nor definitely "glottaling," the principle of regularity observed cannot be applied. It is evident that every such correspondence of sounds may have scientific value when applied to the well-studied dialects, and it cannot be applied to the dialects where the alternation of sounds is not yet carefully studied. Of course, one may expect to find such a correspondence, but one cannot postulate it and hypothetically restore the stems for their further application as proof of anything. With the extension of principles of variations over other pairs of dialects, one may naturally come to some general tendencies characteristic of groups of dialects. However, they do not become valid for other groups of dialects. Let us suppose that we have such a well-established alternation of sounds, and we meet with another alternating series—for example  $s \rightarrow \xi$ , where every  $s$  of one dialect becomes  $\xi$  in another dialect; but if we compare the latter with a "glottaling" dialect not all  $\xi$ 's will correspond to  $x \sim h$ . What is practically observed is that the alternation of consonants affects almost all consonants, but every group of dialects have certain phonetic peculiarities. In so far as one confines oneself to the groups of the same type of phonetic variations, the "law" may have value; but when one goes from known to unknown languages, the "law" loses its value altogether and in still greater degree than in the case of a single pair of alternating sounds, for the chance of error is greater as more numerous are the alternating sounds. Without speaking of principles regulating "laws" of phonetic changes, which at the present time are hypothetic and as such can never be used for "proving" anything, we have to state that we meet with a great variety of sound alteration. In the case of aspiration and bilabialization, a long list of sounds are involved in the process. Yet the same sounds are also found to be subject to other alterations under the influence, for example, of palatalization, voicing, etc. The process of alteration of almost all consonants into  $j$  and a further alteration of  $j$  into other sounds makes it theoretically true that every consonant in certain conditions may change into any other consonant. So that if we extend the validity of "laws" good for one

pair of languages (or dialects) over other languages, and if we extend the "law," the latter will lose its scientific value altogether. *The more numerous the dialects compared, the less sure the practical value of such "laws,"* for what is practically observed in languages is that if we generalize "laws," then the series of alternating sounds may be connected between themselves in such a manner that any consonant may change into any other consonant. Thus the practical application of phonetic regular changes may be admitted as valid only for the group of languages (or dialects) in which they have been found. So that the nature of phonetic changes is such that *the narrower the application of the phonetic "laws," the higher their practical value.*

What is done by A. Sauvageot has nothing to do with the strict scientific method of using empiric principles of sound variations. First of all, he promotes cases of simply frequent occurrences into "laws" which methodologically cannot be justified; second, he uses his "laws" in a reverse sense, i. e., going from the "laws" to the facts, he "restores" the facts, collects them to suit the "laws"; third, he extends "laws" over the languages for which these "laws" have not been established. Such a methodology nowadays is never used in the studies into the Indo-European language. Why should it be applied to the "Ural-Altaic" languages?

#### 45. Restoration of Stems and Difficulties in Operating Them

I am not competent to discuss how far the stems may be actually restored in the Finno-Ugrian languages; but since they are put at the basis of comparison, we must give ourselves a clear idea of their nature. First of all, a stem restored is a certain hypothesis which ought to be based upon strict phonetic variations characteristic of a group of languages supposed to have been issued from one and the same language. The introduction of such a hypothesis as a pre-language makes the whole reconstruction of stems little reliable. Yet it cannot be practically applied for further research and comparison of the languages where the historic variations of sounds are not established. Again, the elementary requirement is that



the hypothesis may have no reverse force.<sup>1</sup> In spite of this, A. Sauvageot does it all the time and "stems" are put as a basis of comparison. Let us illustrate our proposition. The variations of sounds may go into different directions; e.g.,  $s \rightarrow s' \rightarrow s'' \rightarrow h \rightarrow x \rightarrow k$  and  $k \rightarrow k' \rightarrow c \rightarrow ts \rightarrow s$ , both phenomena observed in the "Altaic" languages, so that the change of consonants  $s$  and  $k$  cannot be presumed as one going in one and the same definite direction. If this is so, then the restoration of stems extending over a long period of time (in ages, centuries, perhaps millenniums), where the historic variations are not established, cannot be considered as reliable. In fact, we have already seen that the restoration of Tungus  $p$  is a mere misunderstanding. There are no facts to show that such an initial consonant ever existed in pra-Tungus. Let us now suppose that in the Finno-Ugrian pra-language  $k$  is restored as the initial consonant of some stem. We may be allowed to compare this stem with the Tungus words with the initial  $k$  only on the condition that this  $k$  is preserved from the hypothetical Tungus pra-language. However, we have seen that  $k$  may appear as an alteration of the original  $x$ , which in its turn may appear from the alteration of  $k$  and the latter may exist as a simple aspiration of the vowel. So that the initial  $k$  in Tungus may happen to be of a secondary origin and the original consonant might be zero. As shown, it might also be  $s$ , and  $t$  with their variations. Since the finding of a stem is the finding of a complex transmitted, for scientific safety one must have synchronous data (stems) for all groups compared.

Again, I am not competent to discuss the question of how reliable are Finno-Ugrian stems restored, but from the point of view of their comparison with the Tungus material it should be pointed out that

<sup>1</sup>Such was, for example, the case with J. Németh, who postulated that the alteration  $s \rightarrow x$  in Tungus may have a reverse force in the sense of  $k \rightarrow s$  which has permitted the restoration of  $ham \sim sam$  to prove that *saman* has originated from the Turk *ham*. To be more exact, the unfortunate *saman*  $\sim$  *ham* had to prove the phonetic law  $k \sim s$ . Such an elementary lapsus on the part of J. Németh has led B. Laufer to further aberration with the problem of the word *saman*. This fact shows how dangerous it is to operate with the restorations and further using of hypotheses. (Cf. "Śramana-Shaman," pp. 111-113.) On the other hand, this fact also shows that A. Sauvageot's methodology is not accidental, for J. Németh is stated to be also responsible for A. Sauvageot's work (vide his "Avant-Propos").

the stems compared include only four groups of consonants; namely, labial, dental, glottal, and nasal,<sup>2</sup> which are shown to be alternate. As a matter of fact, such a generalization is not yet complete, for labials alternate with the glottals and glottals alternate with the dentals which might take place in the hypothetic pra-languages. In the instance of the lateral spirant *tenuis*  $L$  in the Lolo languages, I have shown that  $L$  may alternate with a series of consonants including  $r$ , dental, guttural, etc.<sup>3</sup> So if one wants to have an ideal generalization and a really scientific one, it must be restored merely as a consonant and a vowel. Indeed, such a generalization brings to an absurdity the whole construction and for this reason one has to stop at a certain moment of generalization. However, whether one stops earlier or later, the scientific value of the hypothesis does not change. Such a grouping of stems according to the consonants, like labials, dentals, and glottals, is certainly artificial, for these groups of consonants alternate. Yet, to distinguish voiced and non-voiced, occlusive and spirant, etc., is also impossible for the practical achievement of proving the existence of common words. Therefore the generalization is arrested at a certain point where it may still have a scientific appearance and not keep comparatists very restricted by the rigidity of the system.

In this position it is not noticed that another theoretical objection comes from a different side; namely, since the stems are actually reduced to five types and two syllables, and since the number of groups of languages compared is not numerous (Finno-Ugrian, Turk, Mongol, and Tungus), the chance of finding common words is greatly increased because the number of combinations in these conditions is very limited. In order to demonstrate the common words, one must bring a very large number of cases exceeding at least two or even three times the number of probable coincidences mathematically calculated and predicted. Indeed, this is an elementary scientific requirement which must be observed by any one who is looking for reliable conclusions. This side of the problem is not

<sup>2</sup>A. Sauvageot has promised to bring forth another series of parallels with the initials of other groups.

<sup>3</sup>"Phonetic Notes on a Lolo Dialect and Consonant  $L$ ," 1930.



discussed nor probably foreseen by A. Sauvageot. It is merely rejected without discussion.

#### 46. Semantic Parallelism and Its Application

In the case of operation with the stems, we may see the same methodological process as in the case of the phonetic variations, which is made with the intention of extending pseudo-scientific possibility of comparing a great number of words from different languages. With the same view the semantic parallels are extremely extended. In fact, if one confines the comparison of the words with the restricted meaning, e.g., the pine-tree, leg, cloud, etc., the number of possible words will not be very great. For this there is a semantic reason. The fact is well established that the words (sounding starters) change their meaning (complex of conditioned reflexes) and vice versa in the most capricious manner—from part to total and vice versa, from one to another complex, etc. If a certain semantic complex is well established for a certain group of dialects (language) in its historic aspect, and if it may hold for a series of languages, it may be and must be used in the analysis, but the same complex cannot be transferred into another language without being checked up. From this point of view, the analogy with the phonetic variations is very close. The semantic connexions may have no reverse force. It does not mean that one must not try, for instance, to find a common word in one of the semantic modifications, but it means that the finding not supported by other evidences, e.g., historic, is not convincing at all, and when largely used it may bring one to an absolutely erroneous inference. So we may say that *the closer the meaning, the surer the result of comparison*. It is true that such a limitation results in a great limitation of material which may be compared, but at the same time it makes the comparison reliable. These elementary rules are ignored by A. Sauvageot, who freely transplants the semantic complexes from one language to another in a great number of his cases.<sup>1</sup> In some cases, *hundreds* of notions, verbs, nouns, and adjectives are covered, and they are supposed

<sup>1</sup>At the basis of this methodology is the same theory of evolution which recognizes a similarity of semantic variations as one of the fundamental characters of the psychomental complex organically conceived.

to be alike in all languages. Of course, without such a methodological liberty, the number of cases would be greatly reduced.

#### 47. Operation with the Morphological Elements and the Using of Foreign Words

A. Sauvageot also uses other methods for increasing the number of parallels. They are very doubtful when he operates with the morphological elements. In order to show this, I will quote two instances which do not exhaust all types of his proceedings, but which may give an idea of the method used.<sup>1</sup>

Case 34. The word *pikta* (Go'di)—“the son,” has been shown to originate from *pila-ita*, etc.<sup>2</sup> In order to show that the Tungus stem is *p8* A. Sauvageot cuts off *-kta* as a suffix. In fact, in Tungus there is such a suffix (even two suffixes of the same phonetic complex *-kta*), but it has an entirely different function in the formation of “nouns” and cannot be used in the terms of relationship. On which ground has *-kta* been considered as a suffix? The reason is very simple,—A. Sauvageot did not know the origin of *k* and the syllable *ta* did not suit the hypothesis *p8*, so he decided to suppress it.

<sup>1</sup>It is true that the using of the Tungus published material meets with great difficulties, owing to the fact that the records are not always ready for comparative analysis. In fact, the Tungus languages, and especially the Northern Tungus dialects, are very rich in suffixes. This is well known from the earlier publications (M. A. Castrén, L. Adam, A. Schiefner, I. Zaxarov, and others). Yet some of the suffixes have already been fused with the stems to form new “starters,” as occurs, for instance, with some terms of orientation which have been formed from a certain stem containing one vowel (*V*) (cf. “Northern Tungus Terms,” *op. cit.*, p. 180). In such cases the isolation of stems requires great caution and sometimes is even impossible if the lexic material is limited. On the other hand, in the material published one may often see stems together with the suffixes of relation, time, space, etc., which *must* be separated when they do not form the new starters. The most complex cases are those in which the suffixes have a double function,—semantic and morphological. I point out these difficulties here, for even experienced comparatists who are familiar with the Tungus published material often become victims of prematureness of these attempts.

<sup>2</sup>The case is not a simple one. Cf. my “Bilabialization and Aspiration,” *op. cit.*, also “Social Organization of the Northern Tungus,” *op. cit.*, pp. 177–179.



Case 156. The word *iča* (Solon, Ivan)—“the elbow,” which is well known in Tungus [*ičan* (Bir., Kum.) (Neg., Sch.), *iča* (Khin.) (Neg., Sch.), *ičo* (Oroči, Sch.), *ičon* (Ner.), *iičan* (Tum.), *uče* (Olcha, Grube), *xuča* (Goldi)—“the elbow”]. However, A. Sauvageot wants to connect a certain stem (*kV?V*) with *iča* through the aspirated Goldi form (this form is not perhaps an original form) *xuča*, so he does not want *č* to be in the stem and he dismisses it under the pretext that *-ča, -čan*, are met with in Tungus as suffixes. Such suffixes do exist—*ča* as suffix of “part. perf.” and *-čan* as “diminutivus” (the same phonetic complexes may also have other functions as suffixes), both of which have nothing to do with *iča*—“the elbow.”

When A. Sauvageot needs new parallels, he sometimes uses curious etymological suggestions. I will illustrate it with a case which is rather serious—Case 27. A. Sauvageot is now looking for a Tungus initial *p* in the semantic complex of “brûler,” etc., and *faute de mieux* brings forth “to boil” (*kochen*), etc. This is the well-known Tungus *uju*—“to spout,” “to bubble,” whence “to boil” but never *kochen*, for the water may “boil” (bubble), e.g., the sources, without being *gekocht* and the Tungus use *uju* in reference to the water in sources and in the kettle.<sup>1</sup> The Manchu word is *fujembi* (in which W. Grube has wrongly printed or read *š* instead of *j*; however, A. Sauvageot could see it from both I. Zaxarov’s and P. P. Schmidt’s dictionaries). In some Tungus dialects the initial vowel is aspirated, in some other dialects it is bilabialized, and in some of them it is left intact, e.g., *uju*, *huju*, *xuju*, *fuje*, but A. Sauvageot considered it opportune to reproduce an instance quoted by W. Grube; namely, *muju ujuren*, which has only one possible translation—“the water (he or she) is boiling,” or, in English, “he (or she) is boiling water” (*-ju* in *muju* is a suffix of *accusativus*, the stem is *mu*—“the water,” well known in all Tungus dialects). However, A. Sauvageot is not satisfied with

<sup>1</sup>This *kochen* is one of the misfortunate translations from Russian into German, due to the difference in the semantic complexes,—in German *kochen* and in Russian *kipet*.

W. Grube’s translation (W. Grube, *op. cit.*, p. 116) and he gives his own interpretation here reproduced *in extenso*—*muju* (*m- < \*p*), *ujuren*—“faire bouillir de l’eau.” So *mu* (water) is transformed into *puju*, whence only one step to make of it *pšd*. It is remarkable that even a new “law” is formulated on this good occasion,—“Paasonen et K. Donner posent \*-š- en ouralien. On aurait donc oural. \*-š- ~ tong. i (ma. -dž-)” (*id.*, p. 24).

The loan-words also occur in their quality of connexion between Tungus and other languages, two instances of which I shall give.

Case 162. The Goldi word *atū* (“schuh”) and Tungus *unta* (“winterstiefel, bas”) is compared with Dahur *uáda* (“bas”) and Tchouv. *aDā* (“stiefel”) and it is supposed that *-nt* of *unta* corresponds to *t* of Ural-Altaic. Of course, the words are different. Dahur *uáda* is Chinese *wazē*—“the stockings,” in the form of *vase* known in Manchu, whence *vasa* (Bir.), *wasā* (Khin.)—“the stockings,” of Chinese type (cf. Mongol, *Rudnev*).

Case 159. The Goldi word *xuse* is translated *mann, männchen*, and connected with *kisi* (Turk, Osmanli)—“person,” “*mensch*.” However, *xuse* is not “man” and is not Goldi, even not Altaic, but “beard” in Chinese *-xuzē*. This Chinese word is used in Manchu, in a slightly joking sense, in reference to the adult men who have a beard *-xuse* (*xuseta* -plur.) as *xuzē* may be used in Chinese intimate family language.

<sup>1</sup>The original source (A. O. Ivanovskii, *op. cit.*, p. 20) gives “(*múju*) *ujuren*. S<sup>9</sup>. *Kipatit vodu*. Sr. Cast. *hujuu kochen*.” Indeed, *ujuren* cannot be translated as *infinitivus* (of a trans. verb), for the suffixes *re* and *n* definitely indicate “is boiling” without presuming its transitive meaning. The latter may be given either by addition of the suffix of the *accusativus* to the object (*mu+ju* where *ju ~ wV ~ vV*) or its insertion into the verb (e.g., *ujuv* (*V*) *ren ~ ujuren*) if there is no object to be increased with this suffix. Yet when the phonetic character of the verbal stem permits, some Tungus would prefer to give, along with the suffixes, both the object and the verb. In this particular case it is likely that *uju* in its transitive function will appear without the suffix if the latter may be agglutinated to the object. Anyhow, *ujuren*, taken alone, is understood by the Tungus as intransitive “is boiling.” A. O. Ivanovskii’s sentence presumes that there was a person who was boiling water, for *mu* is supplied with the suffix indicative of the function of *mu* as an object. W. Grube has suppressed brackets and separated words with a full stop. A. Sauvageot has changed it into a comma, whence his equation. This case shows how dangerous it is to “correct” the original material and that the operation with the Tungus published material requires great caution and a certain knowledge of language. The problem is still complicated by the fact that there are, in the dialects, two series of words with seemingly different stems; namely, *ui* and *uju*, the origin of which would require too long a discussion.



The class of instances with the defects as to the morphological operations, etymologies, and loan-words is rather large, but it is impossible to quote all of them,—the corrections of errors take much more time than in the doing of them, for in every case one must bring evidences and reasons where and how the error might have crept into A. Sauvageot's text. Most of these errors and mistakes have been made because of a great need of linguistic evidences, for what has been called "la cause commune"—the Ural-Altaic hypothesis. It may be supposed that A. Sauvageot did not have any intention of adapting unreliable and doubtful material, but he is affected by a kind of blindness when the material may help his great cause of proving the existence of the Ural-Altaic reality.

#### 48. Summary as to the Methods Used

Owing to the violation of elementary principles of practical application of phonetic parallelisms, owing to the abuse of generalizations in reconstruction of stems and their reverse application, owing to the extension of semantic complexes and their reverse application, and lastly owing to the liberty in dealing with the material (Section 47), the chance of finding common words is very great. It is now evident how A. Sauvageot could find as many as 214 cases in which over 170 cases contain Tungus parallels.<sup>1</sup>

Many particular cases of A. Sauvageot's methodology can be seen from the cases analysed. We may now summarize what has been observed. In the Tungus parallels A. Sauvageot did not use all available material and sources, as, for example, I. Zaxarov's and E. Titov's dictionaries and several minor collections of lexical material. In using old published material, he is not critical enough and accepts translations which are sometimes not close to the actual meaning of Tungus words. From a series of known meanings, he often makes a selection of meanings suitable for his purpose, and even violates the meaning of words given by the authors. He extends semantic complexes into the Tungus language where they are unknown. He postulates that the Tungus initial glottal and dental are preserved from the hypothetic pra-language. He takes for granted that the Tungus language has lost the initial *p*. He

It ought to be pointed out that under these conditions it would be possible to bring still more—this is a question of leisure—but they would be useless as a new support of A. Sauvageot's position.

admits alternations of sounds in Tungus which cannot be justified by the facts. In some cases he does not pay attention to the presence of consonants in the Tungus stems. In some cases he arbitrarily cuts words into supposed stems and suffixes without being familiar with the Tungus etymology and morphology. In most of his cases he does not trouble himself with the task of finding whether the words found in the Tungus vocabularies are Tungus words or recent "loan-words" from other languages. He does not pay attention to the fact of the spreading of Mongol influence over Manchu and Northern Tungus dialects.<sup>1</sup> He takes no notice as to the position of the Goldi in the group of Tungus ethnical units.<sup>2</sup> He considers words found in dialects as belonging to the language where they have been recorded. He does not question himself as to the cultural complexes in which the linguistic complexes might or might not exist.

The enumeration of peculiar conditions of work which have been created by A. Sauvageot may be still extended over that above mentioned, but it will be perhaps too much for what I want to show, namely, why the finding of parallels is not satisfactory, and why after the analysis there are only two words left which are undoubtedly common to all languages here discussed. The "ensemble" of facts about which A. Sauvageot is speaking in reference to the Tungus language is a product of an artificial selection of badly chosen facts.<sup>3</sup>

Now another question may be asked,—What was the reason of carrying out such a work, which, after a slight touch from the critics, goes to pieces and even compromises parallels which are

<sup>1</sup>He frequently quotes a dialect (Ur., *Castr.*) which soon after having been recorded lost its Tungus lexicon complex and was substituted by a Mongol dialect. Indeed, in the time of M. Castrén, a great part of the words was already substituted by the Mongol elements without speaking of the ethnographical complex, which naturally, together with the terms, was already borrowed from the Buriats.

<sup>2</sup>E.g., in 1926 already represented by me (cf. "Northern Tungus Migrations. Goldi and Their Ethnical Affinities," *op. cit.*).

<sup>3</sup>As to other facts forming the "ensemble" of facts, namely, vocalic harmony, morphological similarities, etc., with the present advanced study into these phenomena, they cannot be considered as a proof of common origin of the Ural-Altaic languages which is also felt by A. Sauvageot.



not perhaps wrong at all? Yet how does it happen that such experienced linguists, as Z. Gombocz and J. Németh,<sup>1</sup> have omitted the most evident blunders and A. Meillet has omitted to notice the crying contradiction between the theories maintained by himself and the theoretical deficiency of A. Sauvageot's work? A. Meillet shows a great cautiousness in the matter of etymologies, semasiology, and phonetics when the question is about the Indo-European languages, but in the case of Tungus parallels A. Sauvageot allows himself such liberty that the dangerous effects may be foreseen by any general linguist who even might know no Tungus.

These questions may be answered by the supposition as to the existence of a general cause for such an attitude on the part of the persons responsible for the final form of the work published by A. Sauvageot,—they wanted to prove a certain proposition, and this desire was so great that it put secondary the problem of choice of technical ways for reaching the goal.

#### 49. The Origin of Methodological Peculiarities Met with in the Discussion of the Ural-Altaic Hypothesis

A. Sauvageot's methods of analysis of the linguistic material and particularly etymologies as they appear after the critical analysis are such that one naturally turns one's eyes to the problem of how he came to such a result. So, for instance, some etymologies and parallels in terms of J. Piaget should be defined as "spontaneous etymology, i.e., the imaginative interpretation of imperfectly understood words,"<sup>2</sup> characteristic, according to this author, of children's practice. The bringing of uncertain evidences seemingly supporting the main idea (as we have seen with numberless hypotheses) is also characteristic of the child and the maniacal state.<sup>3</sup> Yet the scaffolding of hypotheses supposed to support the fundamental hypothesis which we have observed throughout the whole of A. Sauvageot's work is characteristic of the same mental

<sup>1</sup>I have already had occasion of dealing with the etymology of *saman* proposed by J. Németh.

<sup>2</sup>Cf. J. Piaget, "The Language and Thought of the Child," *op. cit.*, p. 149 (I use the English translation here).

<sup>3</sup>Cf. G. Dromard's definition of *délire d'interprétation*, quoted by J. Piaget.

state. The imaginary reasoning "in which every possibility becomes a probability or a certainty"<sup>1</sup> has been pointed out on several occasions. The diagnosis of a psychiatrist would be that we have a case of "interpretative mania," the diagnosis of a psychologist would be that we have a case of child's behaviour, and the diagnosis of a fervent student of primitive mentality would be that we have a case of the survival of primitive mentality. None of these is good, although formally such diagnoses cannot be rejected. The etiology of the case is different. In fact, first of all the limits between "normal" and "abnormal" thinking and "childish" and "adult" thinking as well cannot be formally established. Second, the case here treated is not characteristic of one person, but in various degrees it is typical of a large group of persons to the work of which in a greater or lesser degree the above-given characteristics may be applied. Third, no one would agree that the survival of a primitive mentality may affect the leading group of linguists. And lastly, if the persons in question are not children, then perhaps is the conception of child's mentality defective? If this is so, then the whole phenomenon cannot be interpreted in one of the above-indicated ways and the problem is more complex than it first appears. What comes to mind is that such a behaviour is not particularly characteristic of childish, abnormal, and "primitive" man, but in certain conditions it is characteristic of all human adult beings. I will permit myself to be detained on this side of the problem in order to show how far I am from the idea of giving insulting labels and diagnoses to the work with the methods of which I disagree, and how one must be careful with the similar allusions in critical references to others' works.

A "wrong reference" or a "wrong conclusion" may be conditioned by various causes, but the first place belongs to "lack of experience" in the matter referred to. In fact, the child acquires the ability of doing correct references and conclusions only after certain experience, which results in the stabilization of "meanings" and corresponding "starters" for personal and social use. The larger the field of milieu, the longer the process of adaptation, and thus the

<sup>1</sup>J. Piaget, *op. cit.*, p. 148.



longer the period of occurrence of "wrong references and inferences." Indeed, this process is closely correlated with the duration of the process of growth which in different human and animal groups is variable. However, the tempo of acquirement of the experience is conditioned, not only by the quantity of the elements to be known, but also by their relative value in reference to the security of the growing and self-reproducing organism. Some of the elements of milieu are not indispensable for the survival, while some other elements may threaten the very existence of the growing organism which must know them as soon as it is left without constant help from the society, be it represented even by the mother alone. The amount of the experience needed for survival and self-reproduction is subject to great variations, which depend upon a number of conditions; e.g., the size of the animal, its position in the inter-species milieu, the degree of secondary adaptation (particularly the secondary milieu in man), the degree of ethnical and interethnical cohesion, etc. The quantity of elements of a milieu of the complexly adapted groups is much larger than that of groups simply adapted. Amongst the elements constituting the milieu, there are some which are transmitted from the experience of previous generations through the mechanism of tradition and there are some other elements which are only scarcely known. Yet since there is a constant increase of the elements due to the adaptation of the secondary milieu to the ethnical and interethnical milieus, there are always some new elements of the preceding generations unknown and which must be perceived and investigated by the living generation. Amongst these elements there are some of vital importance and some other elements of secondary importance from the point of view of security of individuals and the unit to which they belong. By which way the process of perception of new elements is going on is not essential for the moment, but it is essential that it should be correctly done, for in many a case it becomes *condicio sine qua non* of survival.

Indeed, a child may have a "childish" method of thinking and communicating with the outer world, so long as it is protected by society. This period is shorter in animals and probably it is shorter in simply organized ethnical units. In reference to the elements which do not seem to be indispensable for the survival of the unit and individuals, the childish and "primitive" attitude may persist throughout life, and in reference to various elements and complexes

of the milieu. The difference between the "childish" and "adult" method of thinking is that which is seen when one refers to the unknown and the known. The same is true of the "primitive" mind. The difference is that of the quantity of elements and complexes of the milieu to be "meant." Let us suppose for a moment that the individual throughout his life preserves the "childish" or "primitive" mind and is unable to make correct references and inferences to and from the elements and complexes indispensable for life. It is evident that such a theoretical individual would perish very soon. We may go further and suppose that the other animals, in so far as their central and peripheral nervous systems are involved into the process of adaptation, must have the ability of forming the correct "meaning" of milieu in so far as it conditions the survival. The difference in all cases is that of the quantity to be meant, the quality of method of thinking being in all cases the same.

Now if we remember the history of different sciences (branches of knowledge), we may see that at a certain moment all of them were built upon the principle of a "childish" and "primitive" method of thinking. The child and the "savage" will never do "wrong" in reference to the elements indispensable for their life, but they will do it in reference to the elements unknown to them. Although in the eighteenth century the Western mind was not "primitive" nor "childish" in reference to mathematics, yet the science of society and language of that time very often showed all typical characters of "primitive" and "childish" production. From this point of view, there is no difference between the mentality of children, adult, "primitive," and "civilized" men.

It may be here noted that the extension of the field of observation and approach greatly helps in avoiding methodological errors at the early stages of science. It is so, particularly speaking, in the field of language, where the individual conditions, as seen from psychology, and social conditions, as seen from ethnology, yet the physical conditions of the formation of sounds as seen from anatomy, physiology, and the theory of sound, are helpful in avoiding elementary cases of spontaneous etymologies, imaginary reasoning, etc. On the other hand, the inoffensive character of the "wrong references and inferences" concerning linguistical phenomena which are not yet threatening (in the given conditions) individuals and which are not



yet very harmful for the ethnical units,<sup>1</sup> are favourable conditions for uncontrolled application of the above-described methods.

I have already shown how and in which conditions the idea of the evolution of languages has made its appearance. In some ways it remains beyond control, for it forms one of the essential elements of the present psycho-mental complex of the European cultural cycle. In the particular case of the Ural-Altaic languages, the idea of an organically evolving phenomenon has been developed in a manner too artlessly carried out and presented in a too naïve form. This was greatly due to the fact that A. Sauvageot was not alone in such an approach to the problem and he was preceded by a brilliant group of old linguists and a not less brilliant group of living linguists who joined their efforts for a "cause commune"—to prove the existence of the Ural-Altaic family and the parenthood of a certain pra-language. Under these conditions, it is very difficult for a new author to keep his own path of investigation. However, at the present time the choice of a new path is rather easy, for the neighbouring sciences have already solved many a problem which could not have been solved through the linguistical ground. The theory of complexes and their variations is fairly well advanced. The theory of evolution has already given place to other methods in ethnography and ethnology. The facts, linguistical facts, are now much more numerous than they were some fifty years ago. The only difficulty, which is not the least one, is the ethnographical complex in which the linguists are living and which they must leave if they want to keep their pace with the neighbouring sciences.

There are many things to be thought over before the coming generation of linguists may successfully choose their path.

<sup>1</sup>The inoffensive character of linguistics (without speaking of ethnography, ethnology, and also anthropology) will very soon be a thing of the past. During the last war and during the Peace Conference, the linguistical problems had a certain importance in finding a correct solution to the problems created by the collapse of the former interethnical equilibrium. This will be shown still better in the near future.

## 50. The Problem of Common Words

Since A. Sauvageot has requested his critics to give their explanation of "coincidence," and since I am responsible for the dismissal of some of these coincidences, I will touch once more on this side of the problem. As a matter of fact, I am not competent to give an answer in reference to all languages discussed; but as to the Tungus language, the coincidences are not numerous enough to occupy the attention of A. Sauvageot. Practically two "coincidences" are not a serious evidence for showing the common origin of the Tungus language with the Ural-Altaic languages. However, this question can be discussed even without establishing Tungus parallels.

There is a great number of words in Tungus met with also more than in two different languages, such as Mongol, Turk, Ugrian, Indo-European, and Chinese, without speaking of the Palæasiatic languages. The number of common words is so great, that it will constitute an ungrateful task for the one who will attempt to establish the original linguistical complexes for different Asiatic languages, and particularly the Tungus language. But what is interesting for us is the linguistic meaning of the common words and the relation between the common words and the common origin of languages. In this way, A. Sauvageot's warning to the critics in charging them with the duty of explaining to him the fact of the existence of common words otherwise than through the common origin of languages will lose its seeming roughness and gravity for the future of comparative linguistics.

In one of the previous sections, I have already pointed out that there are different sources of origin of common words, so that one has to find out with reference to each word discussed by which way the word has happened to be similar in two or more languages. The number of common words due to statistical convergence is very great in case one deals with monosyllabic stems. It is much less in



the case of disyllabic stems. A coincidence of similar words in two languages is greater than in three; in three languages, greater than in four; and so on. However, it is possible to calculate, as I have shown, the percentage of possible coincidences in two, three, and more languages if the stems are grouped according to the types of consonants; e.g., labial, dental, glottal, and others. The point of difficulty in this case will be the establishment of the fact whether it is a statistical phenomenon or not. In some cases it is absolutely impossible to establish it, indeed; for the source of borrowing may be lost altogether. The only way is mathematically, to establish the probable frequency of occurrences and to compare the number of cases showing similarity and at the same time not bearing any traces or documents of their alien origin. In case the number of such words greatly exceeds the number expected by calculation of probability, then two things may be suggested: (1) the words were received from a source which is now unknown; and (2) the words were preserved from the time of the existence of a common language from which other languages originated by differentiation. The last suggestion is one which can be done only after a minute analysis of word origin. If one postulates the existence of such a pra-language and explains the common words through their common origin,—the transmission from a pra-language,—one may easily be misled by the statistical convergence. Although A. Sauvageot or his colleagues have never calculated these probabilities, for they do not want to face the problem and reject this possibility, owing to their conviction that the languages have originated from common ancestors; yet the issue of investigation is already known, for the answer already resides in the question.

The difficulty of understanding the origin of the similarity of languages otherwise than through the direct origin from the parent language is not that residing in the nature of language, but in the fact of accepting the original axioms of origin from the ancestor language, just as the difficulty of non-Euclidean geometry for a person unfamiliar with some other systems resides not in the difficulty of other systems, but in the fact that the original Euclidean axioms are not postulated.

Other sources of common words, as cultural phenomena, local phenomena, and imitation, are productive of a great number of common words. Unfortunately, only a minor part can be traced back to their sources, for the spreading of words might have taken place in such a remote time that there may be no question as to the finding of sources. There has been a great inclination amongst some authors for finding common words originating from certain centers, so that even a slight phonetic similarity was considered to be sufficient evidence for establishing the source of origin. In this case a great source of the mistakes made resides in the method too. First of all, it is presumed that such and such a class of phenomena are "cultural phenomena" and as such they ought to come from a certain definite source. Let us take instances. Of course, it is evident that the Greek word *κῦβερνάω* [originally "to stir," whence *κῦβερνήτης*], as P. P. Schmidt shows, is responsible for the existence of various European terms, and it may be established when this word made its appearance amongst different groups, and in what particular "meaning." It has even reached the Yakuts in the form of *krimidatal* (and *kübärnätär*) adapted from Russian *gubernátor* to the Yakut phonetic complex, in which form it could not be connected with the Greek original word without the competence of E. K. Pekarskii, who knew how it could happen to change in the Yakut soil. But a word such as "war" may be accepted as cultural phenomenon connected with Chinese only on the acceptance of several hypotheses, the principal of which is that "enemy," "war," etc., are cultural phenomena of Chinese origin, supported by linguistical hypotheses.<sup>1</sup> Indeed, it is not improbable that this word is of Chinese origin, but there is a methodological danger, namely, a reasoning by analogy and reference to the cultural character of "war," which is a universal ethnographical phenomenon, the term for which may also be borrowed. The question is thus confined to the phonetic evidences only. We have seen that *xuse* and *vase* are now used by the Manchus

<sup>1</sup>The Chinese etymology of the Manchu word *dain* was originally proposed by I. Zaxarov. P. P. Schmidt has maintained the Chinese etymology by the help of the hypothetical restoration of old Chinese (cf. the discussion between P. Pelliot, *op. cit.*, p. 256, and P. P. Schmidt, "The Language of the Samagirs," p. 4) and supported with reference to an analogy with other "cultural phenomena."



in forms very close to their Chinese equivalents, besides other Manchu words, so the supposition of borrowing is confirmed by the facts, while in the case of *dain* it is supported by the hypotheses. It is true that the terms for "war" in Tungus are borrowed; e.g., *bulon* (Ner. Barg.), *bulen* (Bir., war amongst the shamans), *bulän* (Ur., *Castr.*), which can be compared with *bulija* (Mong., *Rud.*)—"to rob," "to take by force," etc., also *carik* (Mank.) *čirik* (Khin.) which are connected with *čerig* (Mong., *Rud.*)—"the troops," "soldier," "military"; also *siri* (Tum.) connected with *säri* (Yakut, *Pek.*)—"the war." From the stem discussed above there has originated only *dajan* (Bir.) in the sense of war (cf. the previously particularized meaning of an older term *bulen*), recently borrowed. However, all Tungus dialects know the verb *va*—"to kill," and *valdi*—"to kill one another," "to fight a war," *valdinki*—"the battlefield," etc. The danger resides in the fact that this method may become of common practice and simple statistical convergences may be mistaken for borrowings. In my "Social Organization of the Northern Tungus," I have made an attempt to show how the borrowing of such an elementary term as "son" might have happened and what was the reason of such a borrowing. It has required, not only phonetic parallels, but also a thorough analysis of social institutions and their possible variations. These instances show us that the grouping of phenomena into "cultural" and "non-cultural" phenomena may become a source of mistakes. It is easier to operate with the complexes. For instance, in the complex of local institutions covering the marriage and wedding, we have seen that most of the terms are Mongol, Manchu, and Yakut. This may be understood from the fact of the change of these institutions at a certain historic moment. On the other hand, there are cases which are more difficult than this one. Here I have in view, for instance, the complex of the reindeer breeding unknown amongst the present Mongols and Manchus. The term for reindeer in Tungus is *oro~oron*, but in Chukchi it is *horana*, and in Finn *poro* (cf. P. P. Schmidt, "Etymologische Beiträge"; although P. P. Schmidt supposes it to be Western Palæasiatic *poro* it may also be Tungus *oro*, aspirated and bilabialized), the origin of which is unknown. But the Tungus terms connected with breeding are very

often borrowed from the Mongols; e.g., *kure*, etc., for the reindeer fences. So the complex of reindeer breeding, although one of the most characteristic elements of the Northern Tungus complex, is built up of phenomena borrowed with the terms from various sources. It is simple with the complex of horse and cattle breeding which is recently borrowed by the Tungus from Mongol- (Buriat-) speaking people together with the terms, so no doubt may be raised up as to the origin of words. It is thus evident that in every particular case of borrowing or hypotheses as to borrowing, the complexes and the possibility of their total or partial borrowing must be shown. Yet if the borrowing of the most "elementary" complexes is a fact which has been established, we cannot reject such a possibility for any phenomenon whether it appears to the European mind to be a "primitive" or a "cultural" one.

On different occasions we have already seen that there are some local phenomena, e.g., *buran*, *purga*, which are characteristic of the regions with snowstorms and these stems are met with in various languages with nearly the same sense. It is quite natural that every new comer meeting with a new phenomenon, when possible, borrows the name for it from the antecedent experience of the local people, so that if we do not postulate that the people were always living in the same region (the history of human migration does not allow us for a moment to soothe our mind with this idea), the borrowing of these terms is very likely. However, it is difficult in some cases to show documentary evidences for it. The names of local animals, plants, meteorological and other local phenomena ought to be included in this class.

It is evident that the class of words of onomatopoeic origin is also a source of common words, but they are not very numerous. It may be noted that the onomatopoeic words may become subject to borrowing as well. This is perhaps the case of the word "cuckoo," which might have been invented through the imitation of the bird's cry only once or twice, and afterwards it might receive a greater geographical distribution. Some languages, as pointed out (for example, the Manchu), show a quite definite tendency to form words from these sources, while others may borrow these



stems as stems and not as onomatopoetic complexes. In this way a local reference to an onomatopoetic origin of some words is neither convincing nor historically correct. The Tungus words for some birds, as, for example, *gaki* (Bir.) (Neg. Sch.) (Gold., Oroči Sch.), *gaxa* (Manchu Writ.)—"the crow," is explained by the Birarčēn as an imitation of *gak! gak!* supposed to be produced by this bird; *saksaxa* (Manchu Writ.) *sažiga*, *sažia* (Bir.)—"the magpie," is explained by the Birarčēn as an imitation(?) of *sak! sak!* etc. However, there are also other words in other dialects for the same birds.

It is evident that to use common words as an evidence of the common origin of languages requires the most careful analysis, which is well known from the previous experience of comparatists. In spite of it all, possible considerations are rejected and the only one is adopted: the common origin of words due to the common origin of language. There is thus something in the idea of language which implies this attitude. This is the idea that the language is an organical entity existing in time and transmitted as such.

A. Sauvageot charges his critics who would not accept his idea that common words quoted by him are of Ural-Altaic origin to show a great number of coincidences between Uralian and Keshua or Algonquian, and he says that they "veillent bien se donner la peine d'en déterminer un aussi grand nombre, et de la même qualité," as that presented by himself. Nobody can accept this challenge, for if the comparative material is of the same quality as his Tungus material, the work will not be convincing—it must be better done. Second, before we proceed to such an experiment, the probability of meeting common words must be calculated, which has not been naturally done by A. Sauvageot, who does not know how great the chance of meeting common words is. Third, the experiment must be carried out with two languages, the phonetic systems of which do not differ very much; so, for instance, the presence of some rare consonants may greatly reduce the chance of finding coincidences. I do not know Keshua and Algonquian and I have no dictionaries at hand, but if the phonetic difference is not very great and if the experimentator allows himself to have the same liberty of comparing phonetic and semantic groups, it may be

supposed that the number of common words will be also great. In the course of our analysis of cases taken from A. Sauvageot's work we have seen many instances of coincidence with various Indo-European languages. Such a work, at least in reference to the Finno-Ugrian languages, has already been undertaken by several authors and the attempts at comparing Chinese and Indo-European is still fresh, and we have some other recent instances of this kind.

#### 51. The Tungus Language and the Uralo-Altaic Hypothesis

In order to understand the "relationship" between the Tungus and other languages we have to accept the idea that this language, as any other, is a cultural complex which consists of elements of various origin. We do not need the hypothesis of the Ural-Altaic language for understanding the process of formation of the Tungus language and the Tungus as a group of ethnical units.

In our analysis of Tungus complexes, as, for instance, the terms of social organization and breeding of some domesticated animals, we find that some terms are borrowed, together with the cultural elements, from the Buriats, the Mongols, and the Yakuts. Yet some elements are borrowed from the Russians, and yet other elements are borrowed from the Chinese. We know that the Russian terms could not have been borrowed before the seventeenth century, for prior to this time the Tungus did not meet with the Russians, and there is no evidence for showing that Russian terms might have been received from other sources. It is more complex with the Chinese elements. Some of them have been recently received from the Chinese whom the Tungus groups met with in Manchuria and Mongolia and these terms are unknown amongst the Tungus groups living beyond contact with the Chinese. But there are other Chinese elements which have been received through the intermediary of the Manchus and the Mongols. However, there are some Tungus words which sound like Chinese ones, but the fact of their borrowing from the Chinese cannot be shown. Are they common owing to the fact that the ancestors of the Tungus and Chinese did speak the same language? Or are they borrowed at such a remote time that the intermediary groups have disappeared



altogether or have forgotten these words? Both hypotheses are good. With the Yakut words it may be a relatively simple position, just as with the Russian words, for we know more or less exactly at which moment the Yakuts made their appearance in the Tungus territory; but some of the Yakut words themselves may be traced back to either Mongol or Turk languages, while a certain part of them will remain of unknown origin. If we now consider the Manchu language, it will be found that the amount of Mongol words is still greater than that in the Northern Tungus dialects spoken by the groups living at a certain distance from the Mongol-speaking groups, yet the amount of Chinese words is also much greater. Still there will be a certain amount of words which can be connected neither with Chinese nor with Mongol words, but which are found in most of the Tungus dialects. The latter are sometimes also found in various Palæasiatic languages. Are they "pra-Tungus" or "Palæasiatic"? The same situation is found in Mongol—a certain amount of words is easily traced back to the Chinese sources, where they are found together with the ethnographical elements of Chinese ethnographical complex. There is also a small amount of Manchu (not "Tungus") and even Russian elements easily seen together with the ethnographic specific elements. Yet there is also a large amount of terms which are of Turk "origin," which is shown by the connexion with definite ethnographical elements and moments of appearance of terms in Mongol. The Turk elements in their turn may be in some cases connected with Iranian, Ugrian, etc., sources. Still there will remain a certain amount of words which will be common in Mongol and Tungus, in Mongol and Turk, but which will not be common for Tungus, Mongol, and Turk. And finally there will be a certain group of common words for Tungus, Mongol, and Turk, which are not met with in Chinese, Palæasiatic, Iranian, or Ugrian. Will they be considered Altaic? On which ground then? If they are not met with in other languages, it does not mean that they have originated from a common language—they may happen to be so for several reasons; e.g., a word might be borrowed by the Turks, say, at the beginning of the present era, transmitted through borrowing to the Mongols and from the Mongols to the Tungus and the original source might disappear altogether. Or it might start its travelling from the Tungus or the Mongols. More than this, there must be a certain amount of words, the phonetic similarity of which is nearly the

same, for the number of combinations is limited and there *must* be some coincidences. Which of these common words can be proved to be Altaic, due to the migration and due to the chance of coincidence? These cases cannot be scientifically distinguished, so that their adoption as "Altaic" is a hypothesis which cannot prove the existence of an Altaic pra-language. Indeed, other corroborative evidences are needed. These "evidences" are produced too, but they are not facts, new hypotheses, and shall I say hypotheses of no scientific value. First of all, it is postulated that the Altaic ancestors must have lived under certain definite conditions, e.g., in the steppe region, so that the common words concerning cattle-breeding are supposed to be of Altaic origin, and the terms relating to steppe conditions are explained in the same way. Methodologically, this idea is erroneous, for the terms designating the cattle complex and the steppe complex might actually have originated in the midst of the people living by cattle breeding in the steppes, but such a people might speak, not the common Altaic language but some other language which disappeared at a certain moment, transmitting the local terminology to the new-comers of the region, who might be ancestors of non-Turk- and non-Mongol-speaking peoples. Yet these terms might have been invented by a small group of people starting cattle-breeding and interested in having a "steppe terminology." These people might later on have been included into the body of the Mongols and Turks. In all these cases the explanation of origin of these terms does not, indeed, require an Altaic language hypothesis.

The second idea is that the common words must designate "elementary" conceptions and simple phenomena. We have already seen that this hypothesis is a mere extension of the hypothesis of the "evolution of languages" which cannot be supported by scientifically checked facts. The most "primitive" languages possess complex ideas as well. The ideas are of different type, as compared with the European complexes, but they are not elementary at all. Yet in a greater degree it is true of the phonetics and structure of languages.

Attempts have been made in published form for connecting populations, "races," and hypothetic Altaic languages. Of course, all are doomed to fail, for we have no means for localizing either the populations or the hypothetic Altaic language. In this respect, the



theoreticians of the Altaic hypothesis must have before their eyes the sad history of the "Aryan problem." The most "Germanic" race, as the Nordics are often pictured, is supposed by some anthropologists to have issued from either depigmented Negro stock, or from European troglodytes of Quaternary, while the Germanic language is now sometimes regarded as a result of the blending of Finn or Japhetic with Celtic or Slavonic, and the Indo-European pra-language is sometimes now treated as a kind of business language spoken by the Western Asiatic traders (G. Slater).<sup>1</sup> Of course, all the above-indicated theories are very suggestive hypotheses, but they are only hypotheses. Their most characteristic features consist in the fact that after over a century of work of thousands of searchers for pra-languages, pra-people, and the original birth-place of Indo-Europeans, the hypotheses come into conflict between themselves and with the facts. This blue-bird perhaps has never existed, being a mere product of methodological fallacy of conception of language as organic phenomenon and not as a function of adaptation.

Attempts at the reconstruction of the Altaic, and even the Ural-Altaic, languages<sup>2</sup> made by a group of Orientalists has not been successful and as it is shown before the public by A. Sauvageot it becomes ridiculous. Of the two hundred and fourteen cases analysed we have found that only two Tungus stems are met with in all the languages of the four groups. We have analysed how one

<sup>1</sup>Cf. review of some recent hypotheses in Pizzagalli, A. M., "La Question dell'origine," etc., in *Scientia*, Vol. XLVIII, 1930.

<sup>2</sup>There is even a definite tendency to include into this group other languages as well. Such is the case of the Japanese and Korean languages. As I have pointed out, there are two sides to this question; namely, the classification of these languages without a presumption of their "genetic" relationship, and their classification as a method of establishing their "genetic" connexions. As a pure and simple classification, such an including of these two languages is not needed by practical considerations, for these languages are well known; as a "genetic" classification, it is absolutely undesirable, for it will lead investigators along the wrong path in their research. As a matter of fact, the attempts at a "genetic" classification of these languages have already resulted in rather extensive treatises and owing to the fact that the issue of these investigations in many an instance had been anticipated, the facts brought forth have been selected, and as such they have only relative value. The hypothesis of connexion between Japanese, Korean, and the so-called Ural-Altaic languages was proposed by several authors beginning from Fr. von Siebold, and further developed by H. Winkler, and recently supported,

could arrive at such a thin result and we have found that several, we have supposed unintentional, mistakes and misuse of scientific methods were committed. We did not stop our analysis and we proceeded to find out how such an idea might appear. We found that the idea of such an investigation is conditioned by the desire of finding proofs of the common origin of certain groups of languages and when we pushed forward our inquiry we found that this desire is a logical consequence of the idea of the evolution of language, which in its turn is one of the essential elements of the European ethnographical complex.

We may now take another step and say that generally, in so far as the function of ethnographical complexes is known and the European complex is investigated, the reaction on the present critical attitude as to the inflated desire of finding an Altaic and Ural-Altaic pra-language, and on the critical attitude towards the European ethnographical complex, will meet with opposition, the reasons for which will be varied, but the essential element will be the same. Yet we may also add that this opposition will later diminish down to giving place to the modern conceptions of ethnographical phenomena as functions in a certain system of

e.g., by W. Pröhle, G. Ramstedt, and E. D. Polivanov. The latter supposes that the Korean language may be connected with the Altaic, while the Japanese language ought to be regarded as a complex "amalgama" comprising Austro-Asiatic and Altaic elements. The hypothesis of "genetic" relationship has not been adopted by all linguists, for a part have remained sceptical. Amongst the Japanese linguists this methodological confusion has also produced its retarding effect, being in addition intensified by the conditions peculiar to the linguistic problem in its political aspect. Some cases reflecting these attitudes may be quoted. K. Shiratori, in his earlier period, maintained the idea of affinity between Japanese and the Ural-Altaic group, R. Torii connected it with Mongol ("Populations préhistoriques de la Mandchourie meridionale," p. 38, footnote); S. Yoshitake ("Etymology of the Japanese Word *jude*") recently supported the Altaic origin of Japanese, admitting, however, a possibility of Austro-Asiatic elements. On the other hand, in the eyes of Kanazawa (paper published in 1910) Korean is a Japanese dialect, while A. Matsumoto (*op. cit.*) brings forth a long list of parallels from Austro-Asiatic languages which leads him to the idea of "parenté." However, the whole discussion as to the affinities and genetic relations between these languages is not needed for establishing common elements in the languages compared. It is remarkable that the attention of linguists has for so long been diverted from another source of Japanese lexic complex; namely, the Chinese language (cf. S. Yoshitake, *op. cit.*). [In connexion with this problem cf. W. Schmidt's paper just published: "Die Beziehungen der australischen Sprachen zum Japanischen."]



equilibrium. Yet it must give place, for it is already in conflict with the results obtained from ethnography and anthropology, both of which point to the great antiquity of the human species, human culture, and naturally, languages.

Indeed, my point of view on this question may meet with the criticism on the part of those who continue to keep the old path. This criticism may easily be foreseen. A. Sauvageot has already put forward a protective wall—a long series of hypotheses and propositions which can be neither proved nor disproved, since they are conditioned by a greater belief in evolution. If A. Sauvageot is not isolated with his work, assistance may be expected in the form of new hypotheses illustrated with mosaic work, and so the discussion will continue till the last epigones of the creators of old theories give up the old conception of language. Such discussions were frequent during the brilliant period when the school of evolutionists (in cultural phenomena) was gaining ground over their predecessors. Of course, at that time the science of language was not so advanced as it now is, so the discussion was not so artful as it must be nowadays, but the ethnographical significance of the discussion will be the same, indeed.

The positive result of our critical point of view in regard to the work of theoreticians of Ural-Altaic and other hypotheses is that one's attention is drawn to problems the work on which may result in an accumulation of new facts instead of spending one's energy on the problems which are hopeless. In fact, referring to the Tungus languages, it may be much more useful to find out the Mongol and Turk elements in Manchu<sup>1</sup> and some Tungus dialects (I have published material,<sup>2</sup> in view) to find Chinese elements in Manchu (a

<sup>1</sup>In so far as I know from the title of G. D. Sanžeev's paper, the latter is dealing with the Mongol-Manchu parallels.

<sup>2</sup>During the printing of the present work a new important publication has reached me. W. L. Kotwicz ("Contributions aux études altaïques," *op. cit.*) gives the analysis of the Altaic numerals and names of principal colours. Owing to lack of space, I shall now confine myself to the quotation of the conclusion that W. L. Kotwicz has come to; namely, "Les résultats de mes recherches—du moins en tant qu'ils concernent le tongous—témoignent plutôt en faveur des adversaires de la théorie de parenté. Cela n'a rien qui doive surprendre. Je n'ai fait que glisser sur la surface de deux sections d'un champ immense. Je n'ai presque pas touché à la glèbe, et c'est elle qui recèle la solution du problème" (*op. cit.*, p. 234, p. 105 of the reprint).

work which I think is ready for publication by P. P. Schmidt), to find out whether there are Tungus elements in Yakut, to do a similar work with the languages spoken in adjacent territories of Palaeasiatics, and finally to collect more material and to publish it. Even small contributions along these lines may result in great achievements, while the work like that of A. Sauvageot is a mere wasting of time and energy. A list of Mongol words with the initial *h* in Mongol is immeasurably more valuable than G. Ramstedt's theories regarding the initial *φ*, because *h* in Mongol is a fact and the initial Altaic *φ* is imaginary, involving other investigators into further fallacious researches. First, the phonetic laws must be settled upon in every group of language, and the dictionary, as far as possible, ought to be brought to etymological clearness. Then the problem of the existence of an Altaic pra-language will perhaps never come out. If one wishes to travel, one must not put the cart before the horse,—an old but good rule for successful travelling.



## LIST OF WORKS REFERRED TO

- Adam, L.: "Grammaire de la langue tongouse," in *Revue de linguistique et de Philologie comparée*, Vol. VI, Fasc. 2 and 3, 1873
- Amyot, M. A.: "Dictionnaire Tartare-Mantchou françois, etc.," Paris, 1789-1790
- Boas, F.: "Anthropology," in "Encyclopædia of Social Sciences," New York, 1930
- Bobrovnikov, A.: "Grammar of the Mongol-Kalmuk Language," Kazan, 1849 (in Russian)
- Boule, M.: "Les Hommes fossils," Paris, 1921
- Castrén, M. A.: "Grundzüge einer tungusischen Sprachlehre, herausgegeben von A. Schiefner," St. Petersburg, 1856
- Czekanowski, A.: *vide* A. Schiefner
- Ellwood, H. A.: "Cultural Evolution. A Study of Social Origins and Development," New York and London, 1927
- Gabelentz, H. G. von der: "Mandschu-deutsches Wörterbuch," Leipzig, 1864
- Gennep, A. van: "Les Rites de passage," Paris, 1909
- "Traité comparatif des nationalités," Vol. I, Paris, 1922
- Grube, W.: "Giljakisches Wörterverzeichnis . . . mit grammatischen Bemerkungen versehen von . . ." in the series L. von Schrenck's "Reisen und Forschungen im Amur-Lande," Anhang zum III Bande, St. Petersburg, 1892
- "Goldisch-Deutsches Wörterverzeichnis mit vergleichender Berücksichtigung der übrigen tungusischen Dialekte," *ibid.*, St. Petersburg, 1900
- "Die Sprache und Schrift der Jučen," Leipzig, 1896
- Ivanovskii, A. O.: "Mandjurica. I. Specimens of the Solon and Dahur Languages," St. Petersburg, 1894 (in Russian)
- Jespersen, O.: "Language; Its Nature, Development and Origin," London, 1922
- "Mankind, Nation, and Individual from a Linguistic Point of View," Oslo, 1925
- "The Philosophy of Grammar," London, 1924

- Karlgren, B.: "Le Proto-chinois, langue flexionnelle," in *Journal Asiatique*, 1920 (avril-juin, pp. 205-232)
- Keith, Sir Arthur: "Ethnos, or the Problem of Race Considered from a New Point of View," London, 1931
- Kotwicz, W. L.: "Contributions aux études altaïques," in *Rocznik Orientalistyczny*, Vol. VII, pp. 130-234, Lwów, 1930
- "Material for the Study of the Tungus Dialects," in *Živaja Starina*, St. Petersburg, 1909 (in Russian)
- "Quelques données nouvelles sur les relations entre les Mongols et les Ouigours," in *Rocznik Orientalistyczny*, Vol. II, Lwów, 1925
- Kroeber, A. L.: "Anthropology," New York, 1923
- Laufer, B.: "Tobacco and Its Use in Asia," Chicago, 1924
- Leroy, B.: "Le Langage," Paris, 1905
- Lotka, A.: "Elements of Physical Biology," Baltimore, 1925
- Lowie, R. H.: "Primitive Society," New York, 1923
- Marr, N.: "Linguistically Marked Stages of Development of Mankind and Their Correlation with the History of Material Culture," in *Bulletin of the Academy of Material Culture*, Vol. I, pp. 37-70, Leningrad (St. Petersburg), 1926 (in Russian)
- Matsumoto, N.: "Le Japonais et les langues austroasiatiques," Paris, 1928
- Meillet, A.: "Les Langues dans l'Europe nouvelle" (first edition, Paris, 1918), Paris, 1928
- "Linguistique historique et linguistique générale," Paris, 1926
- "La Méthode comparative en linguistique historique," Oslo, 1925
- Meillet, A., et Cohen, M.: "Les Langues du monde par un groupe de linguistes sous la direction de . . .," Paris, 1924
- Mironov, N. D., and Shirokogoroff, S. M.: "Šramana-Shaman, Etymology of the Word 'Shaman,'" in *Journal of the North China Branch of the Royal Asiatic Society*, Vol. LV, Shanghai, 1924
- Mostaert, A.: "Le Dialecte des Mongols Urdus (Sud)," in *Anthropos*, Vol. XXII, Wien, 1927
- Mostaert, A., et Smedt, A. de.: "Le Dialecte Monguor parlé par les Mongols du Kansu occidental," in *Anthropos*, Vol. XXV, Wien, 1930
- Ogburn, W. F.: "Social Change with Respect to Culture and Original Nature," New York, 1922
- Ogden, C. K., and Richards, I. A.: "The Meaning of Meaning. A Study of Influence of Language upon Thought and of the Science of Symbolism," London, 1923



- Patkanov, S. K.: "Essay on the Geographical and Statistical Distribution of the Tungus," St. Petersburg, 1911 (in Russian)
- Pavlov, I. P.: "Twenty-Five Years' Experience, etc. Conditioned Reflexes," Leningrad-Moscow, 1925 (in Russian)
- Pearl, R.: "Studies in Human Biology," Baltimore, 1924
- Pearson, K.: "Grammar of Science," London, 1900
- Pekarskii, E. K.: "Dictionary of the Yakut Language" (twelve fasc.; not yet completed), St. Petersburg, 1907-1929 (in Russian)
- "The Russo-Yakut Dictionary," St. Petersburg, 1916
- Pelliot, P.: "Les Mots à *h* initiale aujourd'hui amués dans le mongol des XIII<sup>e</sup> et XIV<sup>e</sup> siècles," in *Journal Asiatique*, avril-juin, 1925
- Note in *T'oung Pao*, Vol. XXVI, pp. 411, 412, 1929
- Piaget, J.: "The Language and Thought of the Child," London, 1926
- Pizzagalli, A. M.: "La Questione dell'origine degli Indo-Europei e le recenti scoperte della linguistica," in *Scientia*, Vol. XLVIII, No. 222, -10, 1930
- Podgornunskii, I. A.: "Russo-Mongol-Buriat Dictionary," Irkutsk, 1909
- Polivanov, E. D.: "Sur les affinités de la langue coréenne avec les langues 'altaïques' (in Russian), in *Bulletin de l'Académie des Sciences de U. R. S. S.*, Leningrad (St. Petersburg), Nos. 15-17, 1927
- Poppe, N. N.: Critical review of P. Pelliot's publications, in *Zapiski of the Orientalists*, Vol. III, pp. 564-580, Leningrad (St. Petersburg), 1928 (in Russian)
- "Dahur Dialect," published by the Academy of Sciences, Leningrad (St. Petersburg), 1930 (in Russian)
- "Material for the Study of the Tungus Language. The Dialect of the Barguzin Tungus," in "Material for Japhetism," Leningrad (St. Petersburg), 1927 (in Russian)
- "Mongol Names of the Animals in X. Kasvini's Work," in *Zapiski of the Orientalists*, Vol. I, Leningrad (St. Petersburg), 1925 (in Russian)
- Pozdneev, A.: "Mongol-Buriat Translator" (quoted by G.-J. Ramstedt in "Comparative Phonetics"), St. Petersburg, 1891
- Ramstedt, G.-J.: "Ein anlautender stimmloser Labial in der mongolisch-türkischen Ursprache," in *Journal de la Société Finno-Ougrienne*, Vol. XXXII, Helsingfors, 1916-1920
- "Comparative Phonetics of the Mongol Written and Xalxa-Urga Dialect" (translated and revised, from "Das Schriftmongolische und die Urgamundart," 1902), St. Petersburg, 1908 (in Russian)
- "A Comparison of the Altaic Languages with Japanese," in *Transactions of the Asiatic Society of Japan*, Vol. I, Tokyo, 1924
- Regnault, F.: "La Question des races devant l'anatomie et la

- linguistique," in *Troisième Session, Amsterdam, Institut International d'Anthropologie*, pp. 193, 197, Paris, 1928
- Richards, I. A.: *vide* Ogden, C. K., and
- Rivet, P.: "L'Anthropologie," in *Scientia*, Vol. XLVIII, pp. 87-102, 153-166, 1930
- Rudnev, A. D.: "Material for the Dialects of Eastern Mongolia," St. Petersburg, 1911 (in Russian)
- "Xori-Buriat Dialect," St. Petersburg, 1913-1914 (in Russian)
- Sapir, E.: "Language," New York, 1921
- Saussure, F. de: "Cours de linguistique générale," Paris, 1922
- Saussure, L. de: "Les Origines de l'astronomie chinoise," Paris, 1930
- Sauvageot, A.: "Recherches sur le vocabulaire des langues ouralo-altaïques," Paris, 1930
- Schiefner, A.: Material gathered by Baron Maydell, A. Czekanowski, and others, analysed and published in *Mélanges Asiatiques*, Vols. III, VII, and VIII, St. Petersburg, 1859, 1874, 1877
- Schmidt, P. P.: "Etymologische Beiträge" (Reprint), pp. 1-5, 1929
- "The Language of the Negidals," in *Acta Universitatis Latviensis*, Vol. V, Riga, 1923
- "The Language of the Olchas," in *Acta Universitatis Latviensis*, Vol. VIII, Riga, 1923
- "The Language of the Oroches," in *Acta Universitatis Latviensis*, Vol. XVII, Riga, 1928
- "The Language of the Samagirs," in *Acta Universitatis Latviensis*, Vol. XIX, Riga, 1928
- "Der Lautwandel im Mandschu und Mongolischen," in *Journal of the Peking Oriental Society*, Vol. IV, Peking, 1898
- Schmidt, W.: "Die Beziehungen der austrischen Sprachen zum Japanischen," in *Wiener Beiträge zur Kulturgeschichte und Linguistik*, Vol. I, pp. 239-252, Wien, 1930
- Shirokogoroff, S. M.: "Ethnical Unit and Milieu," Shanghai, 1924
- "Ethnos. Fundamental Principles of Variations of Ethnical and Ethnographical Phenomena," Shanghai, 1923 (in Russian)
- "Northern Tungus Migrations. Goldi and Their Ethnical Affinities," in *Journal of the North China Branch of the Royal Asiatic Society*, Vol. LVII, Shanghai, 1926
- "Northern Tungus Terms of Orientation," in *Rocznik Orjentalistyczny*, Vol. IV, Lwów, 1928
- "Notes on the Bilabialization and Aspiration in the Tungus Languages," in *Rocznik Orjentalistyczny*, Vol. VII, Lwów, 1930
- "Phonetic Notes on a Lolo Dialect and Consonant L," in *Bulletin of the Institute of History and Philology, Academia Sinica*, Vol. II, pp. 183-227, Peiping, 1930
- "Place of Ethnography Amongst the Sciences and Classification of Ethnoses," Vladivostok, 1922 (in Russian)



- Shirokogoroff, "Social Organization of the Manchus. A Study of the Manchu Clan Organization," Extra Vol. III of the *Journal of the North China Branch of the Royal Asiatic Society*, Shanghai, 1924
- "Social Organization of the Northern Tungus, with Introductory Chapters Concerning Geographical Distribution and History of These Groups," Shanghai, 1929
- "Study of the Tungus Language," in *Journal of the North China Branch of the Royal Asiatic Society*, Vol. LV, Shanghai, 1924
- "Šramana-Shaman," etc.; *vide* Mironov, N. D., and Smedt, A. de: *vide* Mostaert, A., and Smith, Elliot: "The Evolution of Man," Oxford, 1927
- Titov, E. I.: "The Tungus-Russian Dictionary," Irkutsk, 1926 (in Russian)
- Torii, R.: "Populations préhistoriques de la Mandchourie méridionale," in *Journal of the College of Science* (Imper. Univ. of Tokyo), Vol. XXXVI, Art. 8, Tokyo, 1915
- Vendryes, J.: "Le Langage. Introduction linguistique à l'histoire," Paris, 1921
- Wylie, A.: "Translation of the Ts'ing Wan K'e Mung, Chinese Grammar of the Manchu Tartar Language, with Introductory Notes on Manchu Literature," Shanghai, 1855
- Yoshitake, S.: "Etymology of the Japanese Word *fude*," in *Bulletin of the School of Oriental Studies*, London Institute, Vol. VI, pp. 45-54, 1931
- Zaxarov, I.: "Grammar of the Manchu Language," St. Petersburg, 1879 (in Russian)
- "The Manchu-Russian Dictionary," St. Petersburg, 1875 (in Russian)