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## A METHOD OF ETHNIC QUALIFICATION OF ARCHAEOLOGICAL SOURCES

The tendency of linking "silent" archaeological cultures with determined peoples has rather old tradition. Various trends and schools aiming at the theoretical elaboration of criteria of their ethnic qualification have recently developed.

A precursor of one of them, which has been rightly reminded by H. Jürgen Eggers,<sup>1</sup> was the outstanding Swedish archaeologist, Oscar Montelius.<sup>2</sup> Trying to answer the question: for how long time did Germanic peoples inhabit the Jutland peninsula and the Scandinavian countries, Montelius states among other things:

*"Wollen wir diese Frage, um welche Zeit unsere Väter hier eingewandert sind, aus der Überresten der Vorzeit zu beantworten versuchen, dann müssen wir von den ältesten Zeiten ausgehen, wo nach dem Zeugnis der Geschichte unsere Vorfahren tatsächlich hier gewohnt, und danach für jede (nach rückwärts) folgende Periode untersuchen, inwieweit die Altertumsdenkmäler uns einen Anhalt geben, auf die Einwanderung eines neuen Volkstammes zu schliessen. Würde nun ein solcher Anhalt sich nicht früher als z.B. während der Steinzeit finden lassen, da würde es alle Wahrscheinlichkeit für sich haben, dass der Stamm der Bevölkerung seit dieser Zeit unverändert geblieben und dass folglich unsere germanischen Vorfahren schon während der Steinzeit eingewandert sind."*

We should agree with H. J. Eggers<sup>3</sup> that just the above mentioned opinion of the known Swedish expert of antiquity, was a starting point for the settlement-archaeological method, formulated by G. Kosinna.<sup>4</sup> It was moreover based on H. J. Eggers's conviction that the "simultaneous appearance of a people, a race, a language, a culture and a state" are quite possible (*"Gleichsetzung von Volk, Rasse, Sprache, Kultur und Staat."*)<sup>5</sup> It has later found its reflection in the thoroughly false maxim of Hitler's three-unity. Moreover, as I have already point-

ed out in one of my previous works, it is beyond all doubt that G. Kossinna had found his inspirations also in Ratzel's geographic determinism.<sup>6</sup>

Gustaf Kossinna, later dishonourably noted to be one of the Hitler's spiritual leaders,<sup>7</sup> applied his method for the first time in 1895;<sup>8</sup> he exposed, however, more exactly its theoretical bases in his work: *Die Herkunft der Germanen. Zur Methode der Siedlungsarchäologie.*<sup>9</sup>

Before reminding the fundamental premises of Kossinna's settlement-archaeological method, independently of the higher mentioned H. J. Eggers opinion, it is also fit to quote Jacob-Friesen's view, speaking of the way that supposingly led Kossinna to that formulation. This germanophile stated among other things:<sup>10</sup>

*“Ich bin ausgegangen von der ältesten geschichtlichen Überlieferung, die mein ursprüngliches Arbeitsgebiet war, und kehre letzten Endes stets dorthin zurück. Ich legte für diesen letzten, weitest zurückliegenden geschichtlich erreichbaren Zeitabschnitt die beiden von der Geschichte und von der archäologischen Forschung gelieferten Bilder der Kulturverhältnisse aufeinander, und siehe da, es ergaben sich, wie das nicht anders möglich war, die schlagendsten Übereinstimmungen der Umriss und Züge dieser beiden Bilder, nur dass das von der Archäologie gelieferte Bild in den meisten Einzelheiten ein viel genaueres, klareres, bestimmteres ist. Nun bin ich von diesem Bild des ältesten geschichtlichen Zeitabschnittes aus weiter rückwärts gegangen auf Grund des tausendfach erprobten methodischen Grundsatzes, dass sich die Begriffe Volk und Kulturgruppe decken; für diese älteren Zeiten, die nur archäologisch erreichbar sind, bedarf ich daher nicht notwendig eines neuen Führers, sobald ich nur den lückenlosen Zusammenhang mit den Anfängen der Geschichte niemals aus den Händen verliere.”*<sup>11</sup>

Not thousand proofs, but quite different observations were G. Kossinna's starting points for his theories. He also stated:<sup>12</sup>

*“Schlagende Beispiele aber von ethnographisch streng umgrenzten Kulturen sind die germanische Kultur der Völkerwanderung, d.h. des 5. und 6. Jahrhunderts und die slawische Kultur des 7. und der folgenden Jahrhunderte. Ja die Hinterlassenschaften dieser beiden Kulturen sind das einzige Mittel, das uns helfen kann: einmal die durchaus verschwommenen Nachrichten über die Auswanderung der ostelbischen Germanen zu ergänzen und zu berichtigen, dann auch über die räumliche Ausdehnung der späteren slawischen Besiedlung in Ostdeutschland und Österreich ins Reine zu kommen.”*

It is just on this basis that G. Kossinna arrived to the following general judgement: *“Scharf umgrenzte archäologische Kulturprovinzen*

*decken sich zu allen Zeiten mit ganz bestimmten Völkern oder Völkerstämmen."*

In order to attenuate possible objections he is trying to support his theory by adding the following, very vague corroboration, which may be useful in every case, as it precised nothing indeed, yet it gave some appearance of objectivism:

*"Immerhin gehört zur Verbindung von Archäologie und Geschichte das höchste Mass kritischer Vorsicht. Denn nicht jeder Kulturwechsel, sondern nur der in allen Stücken ganz allmählich sich vollziehende zeigt Dauer der Bevölkerung an. Andererseits ist innerhalb einer geschlossenen Kulturperiode nur ein Wechsel kulturell völlig identischer Völker denkbar, d.h. nur ein Wechsel von Teilen eines grösseren ethnographischen Ganzen, von Stämmen eines und desselben Volkes".*

His pupils never failed to take profit of this sentence, following closely their master's intentions, as can be proved by the pronouncement of R. Stampfuss, referring to the Kossinna's speech in Kassel.<sup>13</sup>

Even before Kossinna had pronounced his above mentioned lecture, E. Mayer<sup>14</sup> stepped forward arguing against the overestimation of archaeological sources in the process of ethnogenesis investigation but — as was rightly noticed by E. Wahle<sup>15</sup> — his opinion was turned against another direction of studies than the one formulated later by G. Kossinna. After this, in connection with the second edition of E. Meyer's book, Kossinna himself and his followers treated the latter as an opponent turned exclusively against him. We should also agree with the opinion of certain G. Kossinna's apologists that M. Hoernes's opposition in respect to the rightness of Kossinna's assumptions contained only generalities of a negative character, without deeper studies on wider archaeological possibilities in the field of ethnogenetic research. He was, however, prophetic in a certain sense, which has been rightly noticed by E. Wahle.<sup>16</sup> Hoernes stated that supported by Kossinna's method, the Germanic scientists will aim at establishing the time of the Germanic tribes distinction at an incredibly remote epoch.

E. Wahle quoted examples supporting this opinion. He could, however, also have found them in dealings with different ethnic groups. It is what I have done myself in some of my works, reminding also the opinions of other writers, (reaching back to the beginning of the 20th century), opposed to Kossinna's theory.<sup>17</sup> But instead of repeating the opinions of these authors, referred to already in the literature, it will be better to mention here the theses of one of the chief defenders of G. Kossinna's conception i.e. of his pupil E. Blume.

This investigator, perceiving various feeble points hidden in the method itself and its premises — tried to give them his proper support. He aimed<sup>18</sup> therefore, at precisising some of the too general formulations. He wrote:<sup>19</sup>

*“Die Resultate der ethnographischen Methode sind aber vielfach auf Ablehnung, wenn nicht gar auf Widerspruch gestossen, ohne dass ein stichhaltiger Gegenbeweis geführt wurde. Es liegt das wohl zum Teil daran, dass die Wege der Methode selbst noch nicht für sich im Zusammenhange dargestellt wurden. Deshalb, hoffe ich, dass der folgende Versuch, den ich meinen mit dieser Methode arbeitenden Forschungen vorausschicke, dazu beitragen wird, das Verständnis ihrer Ergebnisse zu fördern”.*

It is interesting that E. Blume accepted, although very superficially, the importance of ethnographic analogies, even of extra-European provenience.

This support given to Kossinna's theory, demonstrated the weakness of its assumptions. E. Blume had drawn attention to diversities in the earlier development of various peoples, in comparison with the later periods, but his argumentation was quite static. It lacked all attempts a dynamic approach to the historical process. The most essential error in E. Blume's reasoning was that he has based it upon completely hypothetic assumptions: he first mentioned the written sources concerning Germanic tribes and after this he boldly stated that Germanic relics corresponded to them (he didn't even prove their Germanic origin, because he could not do it), coming finally to the conclusion that the culture zones had been in accordance with the limits of territories inhabited by determined tribes. He moreover wrote i. a.:<sup>20</sup>

*“Die bisherige Betrachtung hatte zum Gegenstand den Austausch und die Verbreitung materieller Kultur. Ziehen wir daraus das Ergebnis für die ethnographische Methode der Archäologie. Wir erkannten, dass die Grenzen einer Summe gleichverbreiteter Kulturformen, kurz gesagt Kulturgruppen, aus inneren wie äusseren Gründen sich decken mussten mit Stammesgrenzen”.*

Further considerations were devoted to the factors connected with the migration. They cannot convince the reader for they do not take account of all possible variants that had led to deserting or laying various cemeteries.

It will not be amiss to remind the manner of how E. Blume has been making use of the settlement-archaeological method he had ac-

cepted and substantiated by observations of his own, i.e. how he had applied G. Kossinna's opinions to those forwarded by himself:<sup>21</sup>

*“Unter den charakteristischen Gerättypen bestimmter Kulturbezirke spielt die Keramik eine Hauptrolle, da sie sich zum Handelsbetriebe weniger eignete, als die festeren Waffen, Schmucksachen und dergl. Man fertigte sich seine Töpfe meistens selbst, wie es sicherlich auch mit so und viel hölzernen Gerätschaften geschah, die nur selten erhalten sind”.*

At the end of E. Blume's book we also find this statement:<sup>22</sup>

*“In direkte Berührung sind die Slaven mit den ansässigen Ostgermanen in Ostdeutschland kaum gekommen, denn es fehlt jedes Kennzeichen einer Mischkultur, die wohl bei irgend einer Vermengung hätte entstehen müssen. Dagegen beweisen viele Formen des slavischen Kulturgebietes um 1000 n. Chr. die fast ohne Entwicklung auf germanische Vorbilder der jüngeren Kaiserzeit zurückgehen, dass die slavischen Stämme in jener Zeit einen bedeutenden germanischen Kultureinfluss zu erleiden hatten”.* Still more interesting in this respect is another opinion of this scientist:<sup>23</sup>

*„Die ersten Spuren nichtgermanischer Bevölkerung zeigen sich wieder in der jüngeren Kaiserzeit (3. und 4. Jahrh. n. Chr.). Es sind grosse Hügelgräber aus Steinen, etwa 10 m Durchmesser und 2 m Höhe, die über den Resten der Leichenverbrennung, einem auf der Erdoberfläche errichteten Scheiterhaufen, mit zerschmolzenen und überbrannten Beigaben errichtet sind. In den Kreisen Jarotchin, Bromberg und wohl auch Czarnikau und Kolmar sind derartige Gräber gesichert. Die Beigaben zeigen zwar vielfach germanischen Charakter, wie auch die reiche Ausstattung an importierten Bronze und Glassgefässen und Spielsteinen den gleichzeitigen germanischen Skelletgräbern des Westens entspricht, aber manche Züge sind wie die ganze Grabanlage barbarisiert [...]. Auch keramische Funde sind in der Zeit vom 5.—9. Jahr. nur selten mit Sicherheit festzustellen. Erst seit dem 10. Jahrh. wird die Keramik zeitlich bestimmbar [...]. Um diese Zeit geht die Töpferei zur rotierenden Drehscheibe über und es herrscht im 11. und 12. Jahrh. ein gut geprägter Gefässtypus von gerundeter doppelkonischer Form, mit ausladenden Rand, dessen Urbild in Gefässen der Jüngeren Kaiserzeit zu erkennen ist”.*

For in agreement with the principles of G. Kossinna's theory and taking account of the distinct contradictions between written sources and archaeological materials concerning the extent of Germanic tribes in the “Roman” period in Polish lands — not the “Roman” period

but the early Middle Ages ought to be here taken into consideration. I do not penetrate into the rightness of Blume's generalizations concerning different analogies between products of material culture (including pottery so important for Kossinna and Blume) of the "Roman" period and the early Middle Ages, but according to the premises of the settlement-archaeological method, Blume ought to have corroborated that the Polish territory had in the "Roman" period been inhabited by a Slav, not a Germanic population. This clear association could not have come into his mind, since he accepted that all the territory which he investigated had been inhabited in the "Roman" period by Eastern Germanic tribes. Neither did he, nor Kossinna care to conform themselves to scientific and exact methodical principles but under the cover of a many times verified theory they were pursuing determined political aims. Whether they had acted consciously, or had strayed under the pressure of an old tradition, that had made the use of the erroneous (as will further be shown) settlement — archaeological method impossible — it is an unsolved question. The consequences were exactly the same.

Looking at these problems from a distance of years, one must feel astonished by the circumstance that G. Kossinna's method found highest appreciation not only among German investigators, but also among Polish archaeologists, and was practically recognized by some of them up to quite recent years. It is, however, to be noticed at once that there existed substantial, tremendous differences in the conception of ethnic problems between idealistic and partly fascisizing German archaeologists and Polish scientists. This may probably be explained by the weakness of theoretical foundations observed not only in the works of Polish archaeologists, but generally in prehistory. Some of them, while solving various ethnic questions, according to G. Kossinna's theoretical assumptions, kept, however, a quite different attitude in making practical use of them in ethnic matters;<sup>24</sup> they would, therefore, by no means agree with the opinion (that had rightly followed them) about continuing in their work the settlement-archaeological method. In order to clear the matter it should be said that in the years between World Wars I and II all Polish archaeologists of prehistory's younger periods, among them W. Antoniewicz and J. Kostrzewski, and also younger ones: Z. Durczewski, R. Jamka, K. Jażdżewski,<sup>25</sup> S. Nosek and Z. Rajewski employed that method. It would not be fair to hold back that I too, as a quite young adept in prehistory, considered the application of the described method to be scientifically justified. I simply accepted the judgement of my teachers and older colleagues.

The method itself was not always consequently applied, especially by those who strived to apply the assumptions of the settlement-archaeological method also in the postwar years. There are known Joseph Kostrzewski's hesitations concerning joining the Culture of Wejherowo-Krotoszyn ("Pomeranian Culture") with Balts. This scientist, as well as K. Jazdzewski and some others continued to maintain that in the late Hallstatt and early La Tène period Preslavians were represented by at least two archaeological cultures, i.e. the "Lusatian" Culture and the Culture of Wejherowo-Krotoszyn. M. Jahn (which will be discussed further), when defending G. Kossinna's opinion against E. Wahle's objections stated that according to Kossinna's explicit declaration only "a sharply outlined culture areas" or "culture group," and not every culture corresponds to a single people. The idea of "sharply outlined culture" is known to be relative and can always be adapted to various cases in agreement with determined needs. The detailed analysis of G. Kossinna's and his adherents' works clearly confirms that these were frequently not applied even in more essential points; the theory itself was very often only an easily shifted screen.

In Polish prehistorical archaeology the critical attitude towards G. Kossinna's assumptions appeared only after World War II. A sign of anxiety concerning its validity was K. Jazdzewski's translation of M. Tallgren's dissertation, which will further be discussed. It did, however, not raise sufficient doubts in the ranks of adherents of G. Kossinna's theory. K. Jazdzewski himself used that method in his works.

In another place<sup>26</sup> I have already mentioned the works of some archaeologists indicating, in the years between World Wars I and II, the shortcomings of Kossinna's method, so I will not quote them again. I shall, however, point here to some very exact A. M. Tallgren's remarks.<sup>27</sup> He wrote i. a.:<sup>28</sup>

"To determine tribal belonging by the help of prehistory, ethnography, or material culture is to overestimate the possibilities of knowledge, and is indeed misleading." This point of view was supported by the following opinion:<sup>29</sup> "The economic structure itself in identical geographic environment and connected with the same employments shows that even in a living and well known material culture a nationality or a people (ethnos) do not always impress their stamp on the products of material culture and their traces in the spiritual one are nearly impossible to be distinguished. The culture then may be uniform, independently of ethnic frontiers." Indicating substantial examples of Kossinna's erroneous assumption, Tallgren said:<sup>30</sup>

"Scientists of all countries and particularly Germans, had often incorrectly identified the conception of a culture areas with the con-

ception of a tribal area, without examining their characteristics in every single case. For a tribal character is nearly never a primary feature in the products of culture."

A. M. Tallgren did not limit his comments only to negative remarks in the subject of the settlement-archaeological method, but pointed also to cases where ethnic qualification of archaeological relics can be applied; but his declaration is rather brief and not very clear. He further wrote:<sup>31</sup>

"There certainly existed ethnic territories in prehistoric Europe. The youngest early historic areas are best determined by the help of historic linguistic, toponimic facts and documents. Tribal Celtic and Germanic groups from the beginning of the "Roman" period have been examined this way. We see that the matter is not at all simple also in examples quoted by A. M. Tallgren. In spite of his critical attitude he remained to be fascinated by Kossina's and his adherents' arguments.

In the years between the two World Wars most important was the opinion of K. H. Jacob-Friesen, one of the few then theoreticians of prehistoric archaeology. In his work he drew attention to ethnographic data, indicating disaccord between the reach of ethnos and culture. He concludes by the following words:<sup>32</sup>

*"Aus alledem müssen wir zu dem Schluss kommen, dass es heute noch ausserordentlich schwer ist, Kulturkreise mit Völkerkreisen selbst dann zu identifizieren, wenn wir von den Völkern durch geschichtliche Nachrichten nicht viel mehr als ihre Namen kennen, dass aber derartige Gleichsetzung für Zeiten, die Jahrtausende vor der ersten geschichtlichen Erwähnung der Völker liegen, abzulehnen ist. Besonders deutlich tritt diese Forderung auf, wenn wir im folgenden die Versuche kennen gelernt haben, den Indo-Germanen, Germanen, Kelten und Slaven einzelne Formenkreise, als 'Leitsfossilien' zuzuschreiben."*

This pronouncement was widely extended in the scientific world. This is proved by announcements of outstanding adherents of Kossinna's method, which brought Jacob Friesen in the years of Hitler's rule, to some personal, probably forced declarations and obliged to present and to describe a collection in the Museum of Hannover according to Kossinna's premises.<sup>33</sup> I could not state whether this scientist had presented also a written selfcritic to the public. A distinct trace of the pressure exerted on Jacob Friesen were among others the declarations of a fervent Hitler's adherent B. Richtfofen<sup>34</sup> and of M. Jahn.<sup>35</sup> These circumstances allow to consider Jacob Friesen's above quoted opinion to have been his well grounded point of view in the matter of the settlement-archaeological method.

G. Kossinna's formulations had also been meeting a negative opinion among some outstanding ethnographers and — which is certainly significant — also in the circle of investigators representing an idealistic ideology. Similar point of view represented even such of them which had assumed a method having much in common with the settlement-archaeological one, and which — as for instance the culture-historical school-regarded F. Ratzel<sup>36</sup> as one of their precursors. Moreover, a rather not too enthusiastic opinion towards the settlement-archaeological method may be observed between the lines of the elaborations of W. Schmidt, one of the chief masters in ethnology. In his book published in 1937 he writes:<sup>37</sup> — “*Wertvolle Gedanken zur Methode der Prähistorie finden sich bei A. M. Tallgren, “Sur la méthode de l'archéologie préhistorique” (Eurasia, X, 1936, pp. 15—24)*”. Quite astonishing is in relation to living cultures the restriction of W. Schmidt, who was to some extent an adherent of the unity principle: culture circle — language circle; he stated among others:<sup>38</sup>

*“Ich lasse also keinen Zweifel darüber aufkommen, dass ich die hier aufgestellte Parallelisierung der Sprachenkreise und der Kulturkreise nur in ganz grossen Zügen für richtig halte, und dass ich der Meinung bin, dass selbst da die zukünftige Forschung noch Änderungen bringen mag”.*

The opinion of Kazimierz Moszyński, another outstanding ethnographer was decidedly opposed to that theory. He wrote i.a.:<sup>39</sup>

“Ethnographic data and those of the history of culture state evidently that even the close resemblance of cultures does not prove the similarity of their carriers, and often deep dissimilarities of culture do not decide of language differences in the respective peoples [...]. Ethnology provides a lot of examples from all parts of the world (including today's and ancient Europe). This can be generally presented in the following theoretical example. Let us suppose that in the area lying between the Elbe and the Dnieper, all written documents have been lost and the population has disappeared and that the future investigators will have to establish the history of these countries only on the base of excavations. Could they possibly disclose the fact that the population of today's Poznań region was, as far as the language is concerned, closer (*sic*) to the population of the Tatra mountains, the Lublin area, the Eastern Carpathians, Volhynia and even East Polesia than the population of neighbouring Brandenburg? Contemporary archaeologists should have known that more primitive countries, also exotic ones, could provide quite concrete and very similar examples.”

A quite different although not well grounded and generalized was in the discussed matter the attitude of another Polish ethnographer Jan Falkowski. He maintained<sup>40</sup> that "products of material culture mark the most important centres, corresponding to particular ethnic groups. This is clear, for it is in them (clothes, building, employments, etc.) that appear most palpable and essential differences even in smallest ethnic groups."

Historians also forwarded their restrictions against the settlement-archaeological method. Marcelli Handelsman wrote i.a.:<sup>41</sup>

"A separate place is that of prehistorical fiction. The prehistorical method valuable in the course of scientific procedure and final results, steps sometimes into the field of fancy, when in touch with historical events. For two illusions growing in its background, penetrating into history and accepted there without discussion, contribute to the forming of an erroneous picture of true development. The culture structures of prehistorical time are being linked into one evolutive construction with the system of historical times in the name of a hypothesis, impossible to be verified or grounded by sources, supposing that one has here to deal with quite the same ethnic material all through prehistorical and historical epoch."

A well-known German historian L. Schmidt was likewise not convinced about the rightness of Kossinna's assumptions. He wrote:<sup>42</sup>

*"Mit Recht haben Steinbach und Jacob-Friesen gegen ihre Allgemeingültigkeit Stellung genommen. Die Urgeschichte ist in allererster Linie Kulturgeschichte, sie bedarf der Stütze durch die schriftlichen Quellen, wenn sie für politische Geschichte nutzbar gemacht werden soll."*

Some of L. Schmidt's formulations strikingly resemble the above mentioned declaration of M. Handelsmann. Any mutual influence of the two writers on each other is here doubtful — it should rather be admitted that both these judgements had been inspired by a similar course of reasoning.

In the period of Hitler's biggest military successes and his simultaneous drifting down into ever deeper inhuman attitudes, that is, in the years when it seemed that no voice pointing to the deficiencies of G. Kossinna's methods would break the pervading silence — there appeared E. Wahle's book. The author, although representing an ideal way of thinking, was perhaps at the same time most consequent among Central-European prehistorians, aiming at a historical conception of prehistoric events. He therefore gave a multilaterally conceived and supported by many well grounded examples, critique of the settlement-

archaeological method.<sup>43</sup> His argumentation is not to be shaken by the once in my work mentioned possibility of seizing the appropriate ethnic content, on the basis of a penetrating analysis of various sources, also archaeological ones.<sup>44</sup> Such a procedure has nothing in common with Kossinna's theoretical assumptions, for making use of them should inevitably lead to erroneous conclusions. We have, therefore, in this case to do with the application of quite different theoretical assumptions than those which G. Kossinna considered to be inviolable; they certainly speak against his method.

The publication of E. Wahle's dissertation was rather unexpected for German archaeologists — Kossinna's adherents and faithful to Hitlerism. For criticism came indeed forth out from a scientist, proficient in theoretical problems and outstanding expert of the subject (moreover a former G. Kossinna's disciple), who had, together with some colleagues, taken share<sup>45</sup> in the publication of a special book, commemorating Kossinna's stepping out in Kassel in 1895. The pleaders of the settlement-archaeological method felt alarmed. It can be seen in the title and content of M. Jahn's replica, proclaimed still in 1941.<sup>46</sup>

M. Jahn strived, first of all, to point out to E. Wahle that he had skipped an opinion, much stressed by G. Kossinna, saying only:

*“streng umrissene, scharf sich heraushebende, geschlossene archäologische Kulturprovinzen unbedingt mit bestimmten Völker- und Stammesgebieten zusammenfallen.”*<sup>47</sup>

M. Jahn failed, however, to add that Kossinna himself had used formulas, in which he omitted the determination about sharply distinct culture frontiers. He wrote in one of his works: — *“Jede eigene, noch so kleine, Kulturprovinz bedeutet aber einen eigenen Stamm.”*<sup>48</sup> So M. Jahn's arguments were bound to remain unsuccessful, in spite of his additional stating that many examples gathered by E. Wahle to prove the weakness of Kossinna's theory were drawn from West Germany, archaeologically badly investigated. It is not necessary to remind today on what a frail material from the area beyond the Odra river had G. Kossinna built his theories. The objection of overdoing typology raised against Kossinna by E. Wahle, should be considered rightful.

We should, however, agree with M. Jahn<sup>49</sup> that the matter of authorship concerning the settlement-archaeological method is not clearly stated in E. Wahle's works; he is in my opinion not precise enough while differentiating the matter of making use of archaeological sources to ethnic conclusions from the manner in which G. Kossinna had wished to apply them. The reader may have the impression that

also this author has confounded different arguments together. There is no need to refuse to acknowledge G. Kossinna's authorship of the settlement-archaeological method, deficiencies of which have been rightly presented by E. Wahle and M. Jahn's arguments have not shaken his argumentation.<sup>50</sup>

The undersigned was the first to draw attention in Polish literature to the necessity of a modern approach to the settlement-archaeological method. I published a lecture which I had delivered in November 1947 at the meeting of the Polish Prehistorical Society in Poznań.<sup>51</sup> I have admitted above that it contained some sort of inconsequences. I, however, remind the conclusions which I had reached in my lecture, namely saying that: "the ethnological method may be used only when possessing linguistic and historical data, besides historical sources. In the case of failing to have them, we may only tell what prehistorical cultures have had their share in the shaping of the given people's culture."<sup>52</sup> It practically means rejecting the settlement-archaeological method and G. Kossinna's theoretical assumptions. My intervention had not been noticed in our archaeological literature, but if too little attention had been devoted to that question from a theoretical point of view — a quite different reaction was that of practice. In the postwar period there began to appear works, in which writers stopped ever more decidedly, although not always consequently, to establish the ethnic problems by basing them on G. Kossinna's theoretical assumptions. I contributed myself several times to that changes. The process had lasted during a long period, it is therefore difficult to agree with Z. Rajewski's point of view that only bourgeois scientists have made use of Kossinna's method at the time of writing his dissertation.<sup>53</sup> For long after the last war many prehistorical writers from People's Democratic Countries had applied it, rejecting, however, decidedly one of Hitlerism's basic canons: the unity of race and nation. In the same way the opinion of another investigator that the majority of Polish archaeologists have been using G. Kossinna's theory at that time — was also quite erroneous and untrue.<sup>54</sup>

In 1958 I clearly defined G. Kossinna's theory to be wrong, and not fit to be used in scientific work.<sup>55</sup> These were exactly my words: "[...] I draw special attention to three points of Kossinna's reasoning, sufficiently, however, indicating that the settlement-archaeological method, founded often on premises being in contradiction with historical reality, cannot be a theoretical basis to discovering historically conditioned language groups on the canvas of archaeological sources."

In Polish archaeological literature the following writers have expressed their opinion on the theory of connection between culture

and ethnos: A. Gardawski,<sup>56</sup> K. Godłowski, A. Abramowicz and K. Jądzewski. The first author mentioned, tried to find reliable bases connecting culture with ethnos, but his attempt stayed unsuccessful. K. Godłowski's critic was right in this case.<sup>57</sup> A. Abramowicz<sup>58</sup> was indeed intent on the problem of making use of hypotheses in scientific studies, so the theoretical aspect of the mentioned problem had found itself on the margin of studies. K. Godłowski's remarks have brought only few novelties to the literature of the subject, besides repeating a negative attitude to the settlement-archaeological method and multiplying examples, to prove the fact that the ranges of culture and language are not necessarily identical. He did not assume any attitude to my judgements referring to the criticism of Kossinna's theory, which is decidedly a defect in a work devoted to theoretical problems. The dissertation of K. Jądzewski<sup>59</sup> neither brought many new elements into the matter under discussion. W. Hołubowicz marked several times his negative attitude towards the settlement-archaeological method, although his opinion in this question was rather vague and too general.<sup>60</sup> It will finally be the right thing to remind here Z. Stieber,<sup>61</sup> who drew attention to the frequent discordance between the sphere of culture and language.

The Soviet ethnographer J. W. Bromley was firmly against such an identification of both sphere.<sup>62</sup> Quite different was the attitude of N. N. Čeboksarov,<sup>63</sup> who wrote: — "[...] the problem of ethnic communities in the epoch of primary unity seems to be most clearly presented. These communities, named mostly tribal ones, had formed similarly as other ethnic communities, in a defined territory between people whose relations were linked by concrete economic bonds, speaking one language and displaying specific culture particularities. But on the contrary to ethnic groups of later epoch, these tribal communities used to be formed on the basis of real or invented family bonds, i.e. they were composed of people united by common origin or considering to be connected by common origin. Another characteristic feature of tribal community was the lack of class division and certainly the lack of antagonistic classes. The culture of these communities resulted in a single body: ethnic and social specificity of culture were at that time homogenous." Quoting that statement we must, however, remember the circumstance, that Čeboksarov directs his thought to a living culture, and also the fact that the language is in his opinion a fragment of culture.<sup>64</sup>

Rather wide and taking different aspects of the problem, (also archaeological ones) into account, was the announcement of G. S. Knabe, published in the columns of "Sovetskaja Arheologija."<sup>65</sup> This

investigator understands the term "archaeology" similarly as do many Soviet scientists,<sup>66</sup> with which I do not agree. He collected a number of opinions concerning the subject in question, also of West European researchers, however, he did not confine himself to quoting only the opinions of archaeologists. He did not refer explicitly enough to G. Kossinna's assumptions; it can even be understood that he recognized part of Kossinna's reasoning to be appropriate. He is, however, strongly against its practical, non-scientific utilization in Hitler's Germany and stands decidedly far away from racism. He names, moreover, different methods of ethnic approach to archaeological sources; he rightly feels that a complex method is here the most convenient. Referring here to different authors he is inclined to adopt the opinion that the extent of a given ethnos is not to be mechanically identified with the limits of appearance of an archaeological culture. He emphasizes, similarly as have other scientists done, that an archaeological culture is a dead culture, presenting additionally only part of the general culture achievements of a given social group. He admits that archaeologists had made use of two ethnic methods to define the archaeological relics: — 1) a territorial one and 2) a method differentiating ethnic premises. The first method lively reminds G. Kossinna's assumption, although G. S. Knabe does not mention this coincidence. He determines the ethnic characteristics in burial rites, types of pottery, ornamentation, particularly on the vessels, rarely in types of building, tools, etc.

I shall remind here another Soviet scientist A. J. Brjussov,<sup>67</sup> who claimed that the extent of archaeological cultures of the Neolithic and Bronze Age, is not identical with the limits of the territory of all tribes belonging to one lingual community, but includes only part of them. This is certainly a too general remark giving scarce possibilities of utilizing it theoretically; it contains, however, a whit of truth.

A great number of critical remarks concerning G. Kossinna's assumptions appeared after the Second World War in the works of scientists, representing both German states. Among them the most important writers are: K. H. Otto,<sup>68</sup> H. J. Eggers<sup>69</sup> and R. Wenskus.<sup>70</sup>

In his work R. Wenskus states.

*"Vergleichen wir die Verbreitung beliebiger Züge der Volkskultur in Europa mit der Verbreitung der grossen Sprachfamilien, so zeigt sich, dass sie sich räumlich selten decken. Nun wird man mit Recht einwenden können, dass moderne Verhältnisse nicht ohne weiteres auf die Vorzeit übertragen werden dürfen. Besseren Aufschluss können wir jedoch von der amerikanischen Forschung erwarten, die von in mancher*

*Hinsicht bevorzugter Basis aus arbeitete. Einmal hatte sie Ethne, die in ihrer zivilisatorischen Ausrüstung mit vorgeschichtlicher vergleichbar sind, sozusagen vor der Haustür. Die Arbeit war überdies nicht so mit nationalistischen Forschungserwartungen wie in Europa. Schliesslich war dort, wo vorgeschichtlichen Kulturen ohne Bruch in rezente Indianerkulturen einmündeten, eine lebendigere und fruchtbarere Zusammenarbeit zwischen Prähistorikern und Ethnologen möglich”.*

R. Wenskus indicates that referring to Eskimoes we can evidently presume that the limit of their culture has been exactly the same as the extent of their ethnos. This is, however, a rare phenomenon. The majority of examples (e.g. Pueblo Culture) shows disparity between the culture and ethnos. He further writes:

*“Mit Recht weist H. Hencken auf die Warnung hin, die durch diese Tatsache für jede voreilige Identifizierung von Sprachgruppe und Kulturgruppe gegeben ist. Solche Fälle sind jedoch keineswegs so selten, wie er es noch annimmt. Im Gegenteil sie sind so häufig, dass sie — wenigstens in Amerika — fast als Regel gelten können während das oben angeführte Beispiel der Eskimo als Sonderfall betrachtet werden muss”.*

It can generally be said that Wenskus keeps a critical attitude towards the theory of young gramaticians and is himself against the settlement-archaeological method. At the same time, however, his arguments lack a convincing sound in the subject of identification of certain culture groups with linguistic observations. His work is certainly a step forward in the discussed matter, but it fails to give an entirely satisfying solution.

Argumentation used by the above mentioned investigators, although displaying differences in many details stays in general against the possibility of applying the settlement-archaeological method. But Egger for example erroneously identifies ethnic research with G. Kossinna's assumptions. Therefore the positive propositions he suggests are not able to save Kossinna's theoretical misleading reasoning, they can nevertheless be useful, bringing a number of adequate propositions, concerning bases of the method that might be helpful to carry out ethnic qualification of archaeological sources.

The opinions of different scientists on problems of ethnic qualification of archaeological sources, could further be multiplied. It would, however, bring no essential correction to the above presented theories.

It results obviously from the above that the settlement-archaeological method cannot be the starting point to solve the ethnic problems;

nevertheless, we shall recognize the necessity of finding ethnic solutions concerning the "silent" past.

Archaeological literature draws attention to some of the difficulties in ethnic studies. The first difficulty results from the fact that ethnos is an organism ranged among social categories, subject to defined but very complex rules of historical development. A certain, however also changeable role play here physical factors, moreover all its characteristics do not develop identically,<sup>71</sup> neither do they keep to the same time. New difficulties are consequently seen to appear, one of them is the impossibility to define the criteria that would allow to determine ethnos, independently of the time and place of its appearance. Estimation is less complicated in relation to living societies, although divergences may appear also here. But they essentially limit our possibilities in reference to dead societies who have left only very modest written or linguistic sources, not speaking of those periods, for which even such sources are not attainable. Then, at any rate in the light of actual criteria, the matter seems quite hopeless for an archaeologist.

The following criteria are considered to differentiate ethnos: 1) the consciousness of ethnic community,<sup>72</sup> 2) living in a determined territory, 3) speaking the same language, 4) the existence of common culture characteristics, or at least some of its sections; we can add here (although these ideas fit in the general conception of culture): 5) endogamic character of contracting marriage,<sup>73</sup> 6) common beliefs, 7) common premises of racial elements.<sup>74</sup> The mentioned characteristics do not exclude other variants. It is known that diverse peoples may speak the same language, for each epoch provides such examples. Various groups may display a similar culture, the same religion may be confessed by diverse groups. The same "ethnos" may live in the distant part of the Earth. Not all marriages in the same ethnic groups should have endogamic character, and the anthropological structure of the community can be very different.

Every one of these criteria (only the first excepted) when considered separately is, by no means deprived of chronology. As it has been mentioned above, it is neither deprived of the geographic situation nor can it be univocally determinant. In other words, if we have only one consistency, even well known, we cannot say much about the ethnic origin and composition of an investigated population group. The first criterion, being at least to a certain degree independent of time, slips out of the field of scientific observation of investigators, who deal only with a "lifeless" archaeological culture. It is impossible to apply diachronical rules, stating that in the frame of defined reality determined facts always provoke other phenomena.<sup>75</sup>

Special and ingenious must be the style of the investigation to disclose the mystery of the "ethnos" to which we owe the creation of archaeological sources we have discovered.

Our research procedure must first of all have a complex character. It should therefore fall into separate activities using methods — or perhaps (according to Kazimierz Moszyński) — a general superior method and a number of detailed ones, differentiated as directive and secondary methods.

The general superior method will be based on the assumptions of historical materialism. They are to constitute the guide line helping to elaborate a precise questionnaire of research, and simultaneously assist the task of summing up the results acquired, owing to the use of detailed methods. The questionnaire ought to be elaborated on the basis of a general analysis of the situation. This should be understood as the examination of the conditions favourable to the formation of a cultural complex in all the chronological periods that are to be the subject of our studies.

The secondary detailed methods are expected to facilitate the analysis of sources of these particular scientific disciplines, that are able to throw light on ethnogenetic problems, among other written, linguistic, archaeological, ethnographic and anthropological sources. Our procedure should be realized according to a structure: thesis — anti-thesis — synthesis, as had rightfully been reminded by H. J. Eggers,<sup>76</sup> though not all detailed conclusions of this investigator are acceptable. The realization of that task in the framework of detailed secondary methods require an analysis of the mentioned questions from the point of view of those groups of relics which are offering an opposition between material and spiritual culture. Only after having performed such a precise sources' analysis of mentioned disciplines, may we think of applying detailed superior methods. The results acquired on the basis of archaeological sources should be taken into account in the frame of the thesis, and in the frame of the antithesis — the results owed to the analysis of other categories of sources. A synthetic formulation of archaeological sources would be the final measure. The detailed superior methods should moreover not be conformed to archaeological activity, but into the range of prehistory.

Practically, various categories of sources of other disciplines ought to be similarly analysed. This would indeed be a method of complex ethnic qualification of all sources, among which a particular case would be the application of it to the analysis of archaeological sources.

Archaeological sources — as results from the above considerations and agrees with my previous statements — will be extremely im-

portant in ethnogenetic investigations only in close connection with the documents of other disciplines, among which I have not mentioned some natural ones (e.g. zoology and botany), while in the future perhaps also technical sciences.

To the investigation of the ethnogenesis concerning Slav peoples, most essential will be the correlation of linguistic, archaeological and ethnographic data, next to written documents. Attention has been already drawn in the literature of the subject to the fact that not all categories of archaeological sources are of equal value in ethnogenetic studies. Most essential shortcoming, which is the imperfection of some ethnogenetic works,<sup>77</sup> was failing to take account of the difference between the relics, of an interregional or interethnic character, and a regional one.<sup>78</sup> Only the distinction of local groups of relics and competent renouncement of the so called permanent correlation of the reach of some products — without the chance to consider geographic determinants — may give a chance of a better use of archaeological sources in ethnogenetic investigations.

This does not mean that I suggest to resign from relics of interregional type in this kind of research. First, stating connections in this group means a negative answer to the notion of a lack of continuity. Second, they can give an interesting picture of divergence between the reach of regional and interregional relics, which will allow a better penetration of different questions. So as to provoke no doubts I shall state here that the continuation in the extent of some archaeological culture and its products has not to be the proof of settlement continuity. For we know today examples of transmitting characteristics of their own culture by the local population also to Slavs (e.g. in the Balkans) and the simultaneous loss of linguistic features in their own ethnos. Then ethnic continuation may exist but not indispensably. This is the reason of my repeatedly insisting on the importance of complex investigations.

An essential matter, stressed already in the literature of the subject, although not conceived in the way I propose below — is carrying out an analysis on the basis of classification of archaeological sources, according to their characteristics closely related to concrete ethnos. Most reasonable will be dividing them into three classes. The first might include sources presenting greatest probability of connection with concrete ethnos (e.g. pottery with determined features, not known elsewhere, burial forms, ornamentation, etc.); the second, keeping to an interregional character (e.g. objects connected with building, and with other most basic activities); the third — sources of non-ethnic character.

Of the greatest importance will be here materials belonging to the first class.

The criterion of endogamic marriages was discussed above. I shall add that I have already earlier drawn attention to the chance of making use of the results of Ludwik Hirszfeld's method<sup>79</sup> in detecting groups of blood. The development of anthropological research in this direction would have tremendous meaning in the advancement of ethnogenetic research. Hungarian scientists are known to have acquired very interesting results in that matter. The statistical method would simultaneously be very helpful to making use of valuable J. W. Bromley's observation.

The contribution of ethnology should be considered particularly important. Different observations coming from diverse periods or gathered in diverse territories should not be mechanically transferred in time and space: but the advancement of ethnographic research would add considerably to the reconstruction of the reality and not only of the model situations — in which valuable results have been already achieved. Particularly rich are the attainments of Soviet ethnographers. More attention should be devoted to the defined elements changing the structure of ethnos. It would perhaps allow to construct some theoretical models for different lands and might contribute to the elucidation of various questions.

From the thread of my writings it has resulted clearly that defining the central territories of a given ethnos is especially important in ethnographic research. The borderlands of ethnic territories may raise doubts in this respect, offering data of mixed origin.

The main system of proceeding in ethnogenetic research must be a retrogressive method which should start only out of a directly determined ethnic unit. Then after having analyzed its diverse components may we reach back to periods weakly enlightened in historical sources. The further we shall step back and the less chance we shall have of a complex analysis of sources at our disposal — the more will our conclusions be hypothetical and the more precaution we should apply to their formulation. The method that should prevent us from too hasty hypothesis will indisputably be the above outlined method of complex ethnic qualification.

Many useful indications of methodological nature which are useful also in solving ethnogenetic problems, we find in J. Topolski's theoretical deliberations on history.<sup>80</sup> I agree with the mentioned writer on the importance of knowledge obtained outside sources in historical research. I also confirm that the results acquired by particular investigators are dependent on the degree of their general knowledge and

their skill of putting questions; nevertheless I should see the matter somewhat differently in its details. It may be more sharply marked in archaeology and prehistory than in history *sensu stricto*. The scheme proposed by J. Topolski<sup>81</sup> obliterates the differences between the source-analysis and the synthetic part. In my opinion his proposal refers only to synthetic proceeding. In our case it concerns prehistory and cannot be applied to archaeology. Practically there occurs also in this field the case of stating source-facts supported by knowledge lying beyond sources, my opinion is, however, to avoid that type of proceeding, as it may lead to a disobjectifying of the source itself. In synthetic activities I should not speak about stating archaeological facts, but about the selection of archaeological facts supported by knowledge lying beyond sources. In archaeology, on the contrary, there would exist relation between the accuracy of the description and the analysis of sources — and the knowledge of the investigator working on the problem and his abilities to apply the rules of scientific proceeding which are defined by him and intended to limit the subjectivity of elaboration — though it probably can never be eliminated. Even computers are not expected to be helpful here. The degree of subjectivism may diminish only, but badly applied criteria can also cause its increase.

#### N O T E S

<sup>1</sup> H. J. Eggers, *Einführung in die Vorgeschichte*, München 1959, p. 209. The author failed indeed to take account of the fact that Montelius had first shaped his hypothesis in an article printed in 1884 in Sweden. But K. Jażdżewski mentions him in: *O możliwościach poznawczych archeologii w kwestiach etnicznych* [On the Cognitive Chances of Archaeology in Ethnic Questions], "Prace i Materiały Muzeum Archeologicznego i Etnograficznego w Łodzi," Seria Archeologiczna, No. 16, Łódź 1969, p. 7, not quoting, however, some bibliographic notes. This author is mistaken throwing every ethnic interpretation of archaeological relics into one bag. I have sinned myself in this matter in my early articles on the application of ethnic criteria. I do not think e.g. S. Müller or R. Virchow to have been precursors of the theory formulated later by G. Kossinna. I shall return to the question.

<sup>2</sup> O. Montelius, *Über die Einwanderung unserer Vorfäter in den Norden*, "Archiv für Anthropologie," Vol. XVII, Berlin 1888. I quote after H. J. Eggers, adding that the work published by O. Montelius in 1888 on the typological method is understood to have been the date of prehistory starting to be an independent science. This refers indeed to prehistorical archaeology.

<sup>3</sup> Eggers, *op. cit.*, p. 210.

<sup>4</sup> In the subject literature and also in Polish writings, it was incorrectly called — the ethnic, ethnographic or ethnologic method (this was due to some declarations of Kossina himself and especially from the moment of E. Blume's stepping forward in: *Die germanischen Stämme und die Kulturen zwischen Oder und Passarge*, Vol. I, Leipzig 1912, p. 1) This often led to a false conviction that every ethnic approach to archaeological facts was either precursory in respect to Kossina's theory, or else was proof of using its assumptions. Such a *iunctim* treatment of the matter is found partly in K. Jażdżewski's study *O możliwościach poznawczych...* [On Cognitive Chances...], p. 7; this may result from failing to dot the i's and cross the t's, therefore due to the lack of presenting different trends in the introductory part of the article. This learned man, having highly appreciated achievements on his credit, tries to defend some of Kossinna's assumptions. In the mentioned paper he quotes among scientists treating different archaeological relics from an ethnic point of view, only foreign investigators, although such attempts had also been noted in Poland. See be it only: J. Kostrzewski, *Dzieje polskich badań przedhistorycznych* [A Record of Polish Prehistorical Research], Poznań 1949, pp. 12 and 14. An interesting light is thrown on these matters by a letter of J. Lelewel (22nd July, 1850) published by A. Abramowicz, which runs:

"The earth is a library full of books, still unknown, waiting for people with exceptional insight [...]. While writing this roughly outlined sketch I see my room getting gradually wider and full of all sorts of vessels shaped by humans, beginning with the day when man's hand began to knead clay — in these vessels do I see the branching out of human races, their bonds, their displacements, the joining of branches distinct in new forms, I see colour, glaze and the finishing touch." Cf. A. Abramowicz, *List archeologiczny Joachima Lelewela do Beniamina Fillona* [Archaeological letter of Joachim Lelewel to Benjamin Fillon], "Prace i Materiały Muzeum Archeologicznego w Łodzi," *Seria Archeologiczna*, No. 15, Łódź 1968, p. 216 and ff. with remarks of A. Abramowicz. I have formerly been writing about the need of differentiating trends in the field of ethnogenetic research, making use of archaeological sources, see W. Hensel, *O tzw. baltosłowiańskiej, archeologicznej wspólnocie kulturowej* [On the so-called Balto-Slav Culture Community], in: *Z polskich studiów slawistycznych. Prace językoznawcze i etnogenetyczne na IV Międzynarodowy Kongres Sławistów w Moskwie 1958*, Warszawa 1958, p. 151 ff. The content of E. Wahle's work: *Zur ethnischen Deutung frühgeschichtlicher Kulturprovinzen — Grenzen der frühgeschichtlichen Erkenntnis*, Vol. I, Heidelberg 1941, p. 74, shows that this investigator also distinguished various directions in this field of research procedure. I shall add by the way that E. Wahle's name is placed among the authors who had honoured the date of 1895 as an extremely important turn in the development of prehistory. See: H. Hahne (ed.), *25 Jahre Siedlungsarchäologie*, Leipzig 1922, p. 149.

<sup>5</sup> Eggers, *op. cit.*, p. 257.

<sup>6</sup> e.g. Hensel, *O tzw. baltosłowiańskiej...* [On the So-called Balto-Slav Culture Community], p. 151.

<sup>7</sup> German archaeologists faithful to Hitler's system have stressed the thing many times. Here is the opinion of Werner Hülle: — "Wenn wir heute rückschauend das gewaltige Lebenswerk Gustaf Kossinnas überblicken, darf über den reichen wissenschaftlichen Ergebnissen nicht vergessen werden, dass er zu den Meistern am Bau eines neuen Weltbildes gehört, das in der nationalsozialistischen

*Weltanschauung seinen klaren Ausdruck gefunden hat.*" In a further part of the same article the mentioned investigator comes to the following conclusion: — "*Es ist deshalb auch kein Zufall, dass in den Büchern derjenigen Führer, die sich um die geistige Grundlegung des Nationalsozialismus wesentlich verdient gemacht haben, Kossinnas Kampf um die gerechte Beurteilung der Kultur unserer Vorfahren kräftige Unterstützung gefunden hat.*" Books of the following authors have further been quoted: A. Hitler and A. Rosenberg, see: W. Hülle, in: G. Kossinna, *Die deutsche Vorgeschichte, eine hervorragende nationale Wissenschaft*, 7th ed., Leipzig 1936, p. 269.

<sup>8</sup> At a meeting of the Anthropological Society on 9 August, 1895 Kossinna stepped forward with a lecture (published in "Zeitschrift des Vereins für Volkskunde," Berlin 1896): *Die vorgeschichtliche Ausbreitung der Germanen in Deutschland* (see also the opinion of H. Hahne (ed.), in: *25 Jahre Siedlungsarchäologie...*, p. III ff.). G. Kossinna began his lecture with the words: — "*Wenn ich den Versuch wage die vaterländische Archäologie mit der Geschichte in Verbindung zu bringen und den durch die Arbeit unseres Jahrhunderts aufgesammelten reichen Funden aus heimischem Boden gleichsam ihre Subjektlosigkeit zu nehmen [...]*" I have quoted that sentence in order to remind the urgent necessity of undertaking the problem of marking out the periodization of prehistory treated as historical science. Declarations concerning its historical existence cannot be a basis here. G. Kossinna rightly observes that "*die Wachstafel und das Pergament des Altertums mindestens ebenso geduldig waren wie heutzutage das Papier [...]*", but the conclusion drawn out from the latter stating at least the same value of archaeological sources also for those distant times, must arouse some reservations against this very general conception. See also what G. Kossinna had been writing on settlement-archaeological method in his other works and in the book: *Ursprung und Verbreitung der Germanen in vor- und frugeschichtlicher Zeit*, 3rd unchanged edition Leipzig 1936, p. 3 ff. Although we esteem negatively G. Kossinna's theoretical achievements and his conclusions built on the same basis, as will further be shown, we must, however agree that G. Kossinna's point of view aroused a wave of discussion on ethnogenetic subjects and was therefore a step forward in placing prehistory among historical sciences. I, therefore, see the importance of G. Kossinna's views in a quite different light than they were seen by his adherents from nationalistic groups. See e.g. R. Stampfuss, *Gustaf Kossinna, ein Leben für die deutsche Vorgeschichte*, Leipzig 1935, p. 19. I do not either agree with the opinion of this fascist archaeologist, saying the year 1895 was a landmark in the development of prehistorical research. The scientific unfitness of Kossinna's method is confirmed by the fact that it had clearly been put to use for purposes — as we know today — having little in common with the desire of knowing historical truth.

<sup>9</sup> See G. Kossinna, Würzburg 1911.

<sup>10</sup> I quote here an excerpt out of K. H. Jacob-Friesen, *Grundfragen der Urgeschichtsforschung*. Hannover 1928, p. 141 ff.

<sup>11</sup> I add here that the further quoted Kossinna's words, reminded already by Jacob-Friesen, are proof that Kossinna did not take account in his writings of data incompatible with his conception. He also failed in this case to make use of such basic sources as are certainly ethnographic sources. His speaking of "a thousand proofs" was simply untrue. He stated: — "*Ich kann vor einer stärkeren Heranziehung der Völkerkunde nur warnen, europäische Kultur und Aussereuropa, das sind stets zwei ganz verschiedene Welten gewesen.*" This was

by no means the statement of a learned man, but that of a politician totally given up to racism. See Jacob-Friesen, "Die Grundfragen...", p. 139.

<sup>12</sup> This sentence has been also quoted by Stampfuss, *Gustaf Kossinna...*, p. 21.

<sup>13</sup> Stampfuss, *Gustaf Kossinna...*, p. 22.

<sup>14</sup> E. Meyer, *Geschichte des Altertums*, Vol. II, Stuttgart 1893, p. 52.

<sup>15</sup> Wahle, *Zur ethnischen Deutung...*, p. 75.

<sup>16</sup> Wahle, *op. cit.*, p. 71.

<sup>17</sup> See e.g. Hensel, *O tzw. balto-słowiańskiej...* [On the so called Balto-Slav...], p. 150.

<sup>18</sup> Blume, *Die germanischen Stämme...*

<sup>19</sup> Blume, *op. cit.*, p. 2.

<sup>20</sup> Blume, *op. cit.*, p. 7.

<sup>21</sup> Blume, *op. cit.*, p. 5.

<sup>22</sup> Blume, *op. cit.*, p. 212.

<sup>23</sup> *Kaiser Friedrich-Museum in Posen — Amtlicher Führer*, 3rd ed., Poznań 1911, pp. 44—45. The book is truly not signed by Blume, but its style and the fact that at the time this guide was published, Blume was working in that museum, undoubtedly indicate his authorship of the prehistorical part of the mentioned publication. See e.g. H. Gummel's details on Erich Blume (1884—1912) in: *Forschungsgeschichte in Deutschland*, Berlin 1938, p. 402; the book contains further bibliographic data.

<sup>24</sup> A characteristic thing to be quoted here is one of the pronouncements of J. Kostrzewski of 1939, where he sharply disproves ascribing different archaeological cultures to Germanic tribes, not supported by scientific sources, but simultaneously does not put the settlement-archaeological method to question. See J. Kostrzewski, *Badania prehistoryczne w dzisiejszych Niemczech* [Prehistorical Research in Germany of Today], "Kwartalnik Historyczny," Vol. LIII, 1939, p. 50.

<sup>25</sup> He expressed his opinion in a popular-scientific work devoted to the methodics of archaeological research. See K. Jażdżewski, *Co to jest prehistoria i w jaki sposób bada przeszłość* [What Is Prehistory Indeed and How Does It Study the Past], Warszawa 1934, p. 30 ff. He wrote there — "Scientific research aimed at getting known the culture of various primitive peoples such as Polynesians, African negroes, Indians, Lapps, etc., and the steady advancement of ethnological investigations in civilized countries have revealed, beyond any doubt, that every people — had it only kept its primitive characteristics and did not yield to the acting of contemporary civilization that equals everything to one level — has its own material and spiritual culture typical of it only, which makes it different from other peoples. Even today, in spite of the fact that towns and rural educated people have been making their impact for ages, it is still possible to specify in Poland territories inhabited by Mountaineers, Peasants of the Łowicz district, Kashubians or inhabitants of the Kampinowska Forests. They all have features distinguishing their individuality, observed in different local clothing, building of houses, furniture, rites, songs and language. On this ground, historians have come to the conviction that those sets of prehistorical relics, appearing at a certain time in different limited territories, are the material culture of prehistorical peoples; they concluded otherwise that every people covered a territory corresponding to the range of a single set of relics." In the above words J. Jażdżewski does not mention the creator of the

method he himself named "ethnological." This conception reminds G. Kossinna's ideas, where only the words "zu allen Zeiten" were omitted and some other examples are given instead. The statement that there can exist cases when the borders of culture and ethnos are not exactly the same, was only admitting that there is no rule without exceptions. For it is difficult to imagine that our investigators of the years between the two World Wars, particularly older people did not notice cultural divisions that had occurred in Polish lands due to their dismemberment; in other words, that political and not ethnic factors had contributed to the formation of differences in the material culture of the population in the mentioned partitions. No one was much impressed by the sentence formulated already in the 19th century, saying that: "no law of nature can deceive, there are no true exceptions from true rights. If there exist contradictions, it should be sought only in the investigator's mind. Either the rules are fictitious, or fictitious are the events, being in collision with them." (See W. S. Jevons, *Zasady nauki. Traktat o logice i metodzie naukowej* [The Principles of Science. Dissertation on Logic and the Method of Learning], vol. II, Warszawa 1960, p. 423). It refers to the laws of nature, but what can be the value of a sentence concerned with the interpretation of cultural events, if one must mention numerous conditions, in which they can be applied, indicating contradictions existing among them in the history of different peoples in various periods of their development. This saying itself should lead to discarding such concepts in the methodical dekalogue of a given discipline. Here also Francis Bacon's sentence can advantageously be applied which says that our minds do not need wings, but rather leaden weights to keep down their flights (see Jevons, *op. cit.*, vol. II, pp. 355—356). For in what other conditions indeed sound statements, quoting examples from different continents, could substitute, several scores years later, the opinion of the same investigator, that those are proofs of incompatibility of the range of archaeological culture and ethnos? (see Jażdżewski, *O możliwościach poznawczych...* [On Cognitive Chances...] p. 16 ff.). It can here be added that still in 1962 W. Antoniewicz, although judging critically Kossinna's method, did not bring himself to determine it scientifically unfit (See *Wielka Encyclopaedia Powszechna*, PWN, vol. I, p. 341). In the same *Encyclopaedia*, Vol. VI, in the entry "Kossinna Gustaf" (p. 86), nobody dared the statement that the ethnic method proved to be false.

<sup>26</sup> W. H e n s e l, *Stosowanie metody etnologicznej w prehistorii* [The Application of an Ethnological Method in Prehistory], "Roczniki Humanistyczne," Vol. I, 1949, p. 60 ff.

<sup>27</sup> A. M. T a l l g r e n, *O metodzie archeologii przedhistorycznej* [On the Method of Prehistorical Archaeology], "Wiadomości Archeologiczne," Vol. XIV, 1936, p. 15 ff. This investigator failed to understand, just as did many archaeologists of western countries, the tendencies of Soviet archaeologists in the years between World Wars I and II; this resulted in an erroneous attitude to sound ideas expressed in the criticized text of one of Soviet periodicals. See *ibid.* p. 17.

<sup>28</sup> T a l l g r e n, *op. cit.*, p. 19.

<sup>29</sup> T a l l g r e n, *op. cit.*, p. 19.

<sup>30</sup> T a l l g r e n, *op. cit.*, p. 20.

<sup>31</sup> T a l l g r e n, *op. cit.*, p. 20. Similar ideas do we read in K. Jażdżewski *O możliwościach poznawczych...* [On Cognitive Chances...], p. 16, ff.

<sup>32</sup> J a c o b - F r i e s e n, *Grundfragen...*, p. 144 ff.

<sup>33</sup> Cf. W. Hülle, (excerpt from:) *5000 Jahre Niedersächsische Stammeskunde...*, Leipzig 1936, in: *Mannus*, Vol. XXVIII, 1936, p. 291 ff.

<sup>34</sup> B. von Richthofen, *Die Vor- und Frühgeschichtsforschung im neuen Deutschland*, Berlin 1937, p. 17. On the above, many times quoted, work of Jacob-Friesen he writes: — “das leider den entscheidenden Verdiensten Kossinnas und seiner Arbeitsrichtung nicht gerecht wird.” And about the author himself he adds: — “Der Verfasser ist sonst aber durch von ihm geleitete ausgezeichnete Landesmuseum in Hannover ebenfalls mit besonderen Erfolg für die Germanenkunde und ihre volkstümliche Auswertung tätig. Dabei wird auch die Arbeitsart Kossinnas in der richtigen Weise mit berücksichtigt. Die neuesten Veröffentlichungen Jacob-Friesens und seiner Mitarbeiter zeigen das ebenfalls deutlich.”

<sup>35</sup> M. Jahn (rev.), Ludwig Schmidt, *Geschichte der deutschen Stämme bis zum Ausgang der Völkerwanderung. Die Ostgermanen*, 2. Aufl., München (Beck) 1934, in: *Mannus*, Vol. XXVIII, 1936, p. 284 ff. We find among other things such a statement on p. 285: — “Besonders wegen dieser schädlichen Folgen wäre es sehr zu begrüßen, wenn Jacob-Friesen seine jetzige Stellungnahme zur Methode Gustaf Kossinnas öffentlich bekannt geben würde.” The principal objection was that Jacob-Friesen’s opinion weighed on the negative attitude towards Kossinna’s theory of many representatives of different disciplines related to prehistory. It is perhaps worth while reminding that starting from similar premises, although keeping to quite different intentions, did K. Jazdzewski state unwillingness to making use of archaeological sources by some representatives of other disciplines. See Jazdzewski, *O możliwościach poznawczych...* [On Cognitive Chances...], p. 12.

<sup>36</sup> See e.g. W. Schmidt, *Handbuch der Methode der kulturhistorischen Ethnologie*, Münster 1937, p. 25 ff. The school of culture circles (Culture-Historical School) had often already been criticized. Attention was rightly drawn to its a-historical manner of conceiving various cultures, quite opposed to the name it had assumed. See e.g. I. Sellnov, *Grundprinzipien einer Periodisierung der Urgeschichte*, Berlin 1961, p. 57. W. Schmidt represented the school’s fideistic direction.

<sup>37</sup> Schmidt, *op. cit.*, p. 311.

<sup>38</sup> Schmidt, *op. cit.*, p. 268.

<sup>39</sup> K. Moszyński, *Kultura ludowa Słowian* [The People’s Culture of Slavs] vol. II, Part II, Kraków 1939, p. 1573 ff. Cf. further works on the subject of K. Moszyński’s declaration: Hensel, *Stosowanie metody...*, *passim*, [The Application of an Ethnological Method...], *passim*, and K. Jazdzewski, *O możliwościach poznawczych...* [On Cognitive Chances...], *passim*.

<sup>40</sup> J. Falkowski, *Ze wschodniego pogranicza huculskiego* [From the Eastern Border of the Hucul Peoples’ Land], “Lud.” series II, Vol. XIV, 1936, p. 153.

<sup>41</sup> M. Handelsman, *Na marginesie naszej najnowszej literatury o najdawniejszych dziejach Polskich* [On the Periphery of our Newest Literature Concerning the Most Ancient History of Poland], in: *Studia Staropolskie. Księga ku czci Aleksandra Brücknera* [The Book in Honour of Alexander Brückner], Kraków 1928, p. 68, Cf. also my remark on p. 26 of the book.

<sup>42</sup> Noted after Jahn (rev.), Ludwig Schmidt, *Geschichte...*, p. 284.

<sup>43</sup> Wahle, *Zur ethnischen Deutung...*

<sup>44</sup> Cf. Hensel, *Stosowanie metody...* [The Application...], p. 68.

<sup>45</sup> Hahne (rev.), *25 Jahre Siedlungsarchäologie...*, p. 149 ff.

<sup>46</sup> M. Jahn, *Die deutsche Vorgeschichtsforschung in einer Sackgasse?*, "Nachrichtenblatt für deutsche Vorzeit," Vol. XVII, 1941, p. 72. Jahn was by the way obliged to admit (p. 74): — "Bei der Tragweite der Wahleschen Ausführungen halte ich es für notwendig, schon um dem Aufkommen einer, bei Fernstehenden verständlichen Verwirrung und Unruhe zuvorzukommen, sofort im Nachrichtenblatt zu den grundsätzlichen Erwägungen Wahles Stellung zu nehmen. Viele von Wahle berührte Einzelfragen und angeführte Beispiele lohnten gleichfalls eine Besprechung, zumal eine ganze Reihe meist kritischer Bemerkungen Wahles recht treffend sind, doch fordert die an dieser Stelle gebotene Kürze eine Beschränkung auf das Wesentliche."

<sup>47</sup> Jahn, *Die deutsche Vorgeschichtsforschung...*, p. 75 ff.

<sup>48</sup> See e.g. Kossinna, *Ursprung und Verbreitung...*, p. 4. It is only in the content of this very much condensed chapter that we find the titles of secondary chapters, announcing far more than what G. Kossinna had presented. There are e.g. such titles (p. VII): — "Meine Methode, die Grenzen des Germanengebietes auf archäologischem Wege von frühgeschichtlichen Zeit an so weit wie möglich rückwärts zu verfolgen. Jede scharf ausgeprägte archäologische Kulturgruppe bedeutet ein Volk oder einen Volkstamm." It is easy to observe that such a formulation is inaccurate, for a culture group is certainly widely different from an archaeological culture, similarly as the concept of "Volkstamm" is subordinate to the concept of "Volk." There is no need to be expert in theoretical problems to know that the same key cannot be used to open several types of locks, unless it is a picklock, but then the implement itself can neither successfully work; other aptitudes are moreover necessary.

<sup>49</sup> Jahn, *Die deutsche Vorgeschichtsforschung...*, p. 75.

<sup>50</sup> I shall add here, so as to avoid returning to M. Jahn's opinion that the latter has not contributed to substantiating the rightness of Kossinna's theoretical argumentations, neither has the study of this investigator published in 1953. See M. Jahn, *Die Abgrenzung von Kulturgruppen und Völkern in der Vorgeschichte*, Berlin 1953. On the contrary, emphasized and well estimated should be the fact that M. Jahn devotes much attention in this study to the problem of mixed zones lying along the frontiers of culture and ethnic groups. This has been stressed by F. Petri, *Zum Stand der Diskussion über die fränkische Landnahme und die Entstehung der germanisch-romanischen Sprachgrenze*, Darmstadt 1954, p. 19 ff.

<sup>51</sup> Cf. "Z otchłani wieków," Vol. XVII, 1948, p. 63; W. Hensel, *O przydatności metody etnologicznej w prehistorii* [On the Utility of the Ethnological Method in Prehistory], "Z otchłani wieków," Vol. XVII, 1948, p. 2; idem, *Stosowanie metody...* [The Application of an Ethnological Method...]. This was to a certain degree referring to an earlier A. Brückner's step. He had somewhat exaggerated, but indeed, although archaeology has very much advanced since then, he rightly determined its place in ethnogenetic investigations. These are A. Brückner's words: — "Archaeology is able to determine the state of a very remote culture owing to excavations, but only language can decide whose culture it had been." See A. Brückner, *Cywilizacja i język* [Civilization and Language], in: *Szkice z dziejów obyczajowości polskiej*, Warszawa 1901, p. 10. He further writes that language may disappoint in ethnic research, however by means of the language one may define the origin of a people, with far more certainty than by means of archaeological sources only.

<sup>52</sup> Hensel, *Stosowanie metody..* [The Application...], p. 68.

<sup>53</sup> See A. Gardawski, J. Gąssowski, Z. Rajewski, *Archeologowie i pradzieje Polski* [Archaeologists and Polands Prehistory], Warszawa 1957, p. 38.

<sup>54</sup> This is to be traced in K. Godłowski's article: *Uwagi o niektórych zagadnieniach interpretacji źródeł archeologicznych* [Remarks on Some Problems Interpreting Archaeological Sources], (Referring to A. Gardawski's work: *Plemiona kultury trzcinięckiej w Polsce* [The Tribes of Trzciniec Culture in Poland]), "Prace i Materiały Muzeum Archeologicznego i Etnograficznego w Łodzi," *Seria Archeologiczna*, No. 8, Łódź 1962, p. 79 ff. I have more generally spoken earlier on the shortcomings of this article, See W. Hensel, *Archeologia o polskości ziem zachodnich* [Archaeology Has Something to Say on the Polish Origin of our Western Provinces], in: *Munera archeologica Iosepho Kostrzewski quinquagesimum annum optimarum artium studiis deditum paragenti ab amicis, discipulis oblata*, edition under the direction of W. Kočko, Poznań 1963, p. 34.

<sup>55</sup> Cf. Hensel, *O tzw. balto-słowiańskiej..* [On the So-called Balto-Slav...], p. 150 ff. Similar conceptions have been later published in French, in Italy and in Poland.

<sup>56</sup> A. Gardawski, *Plemiona kultury trzcinięckiej w Polsce* [Tribes of Trzciniec Culture in Poland], "Materiały Starożytne," Vol. V, 1959, p. 9 ff. Gardawski spoke not very clearly in his very interesting lecture, held in 1965. See *idem*: *Zagadnienie ciągłości osadniczej, kulturowej i etnicznej w międzyrzeczu Odry i Dniepru od II okresu brązu do VI/VII w. n.e.* [The Problem of Settlement, Culture and Ethnic Continuity between the Odra and the Dnieper from the II Period of the Bronze Age to the 6th-7th Cent. A. C.], in: *I Międzynarodowy Kongres Archeologii Słowiańskiej, Warszawa 14—18, IX 1965*, edited by W. Hensel, Vol. I, Wrocław 1968, p. 217.

<sup>57</sup> Cf. note 54.

<sup>58</sup> A. Abramowicz, *Uwagi o roli hipotez w archeologii (na marginesie dyskusji K. Godłowskiego z A. Gardawskim)*. [Remarks on the Role of Hypotheses in Archaeology (Referring to a discussion between K. Godłowski and A. Gardawski)], "Prace i Materiały Muzeum Archeologicznego i Etnograficznego w Łodzi," *Seria Archeologiczna*, No. 8, Łódź 1962, p. 103 ff.

<sup>59</sup> Quoted in note 1.

<sup>60</sup> See e.g. W. Hołubowicz, *O kształtowaniu i rozwoju społeczeństw słowiańskich na ziemiach polskich* [On the Formation and Development of Slav Societies in Polish Lands], in: *Pierwsza sesja archeologiczna Instytutu Historii Kultury Materialnej Polskiej Akademii Nauk, Warszawa 4.V—8.V 1955*, Warszawa 1957, p. 84 ff.

<sup>61</sup> Cf. e.g. Z. Stieber, *Grupa etniczna a kulturowa* [Ethnic and Culture Group], in: *Pierwsza sesja archeologiczna...*, p. 105.

<sup>62</sup> U. W. Bromlej, *Ėtnos i endogamija*, "Sovetskaja Ėtnografija" 1969," No. 5, p. 84 ff. An interesting example of losing the traits of an individual Ethnos are the Khazars, who despite having lost their language continued to exist as an individual ethnic group for some time before being completely absorbed by the new ethnical environment. They are supposed to be the "Brodniks" known from 12th cent. written sources. They spoke Russian and belonged to the Orthodox Church, but in those written sources they never were identified with the "Russ." The 16th century's Brodniks' descendants were to be called with

a Turkish name "Kozaks." We don't know neither the etymology nor the Turkish meaning of this name. See A. Brückner, *Słownik etymologiczny języka polskiego* [Etymological Dictionary of the Polish Language], Warszawa (no date of edition), p. 263. In the recently edited dictionary of the oldest Turkish dialects there does not exist the term "Kozak" though it is known e.g. from mediaeval Chinese written sources. (See collective study: "Drevnetureckij slovar," Leningrad 1969). See L. M. Gumiljev, *Otkrytie Hazarii*, Moskva 1966 p. 173 ff.

<sup>63</sup> N. N. Čeboksarov, *Problemy tipologii Ėtničeskijch obščnostej v trudach sovetskijch učenyh*, "Sovetskaja Ėtnografija," 1967, No. 4, p. 100 ff; see also L. V. Chomič, *O sodržanii poniatija Ėtničeskije procesy*, "Sovetskaja Ėtnografija," 1969, No. 5, p. 81, and D. E. Jeremijev, *Jazyk kak Ėtnogenetičeskij istočnik*, "Sovetskaja Ėtnografija," 1967, No. 4, p. 62.

<sup>64</sup> Čeboksarov, *Problemy...*, p. 99.

<sup>65</sup> "Sovetskaja Arheologija," 1959, No. 3. On the separateness of notions of "Culture" and "Ethnos" see still more expressive opinion of P. N. Tretjakov, *Fino-Ugry, Baltij i Stavjane na Dnepre i Volge*, Moskva-Leningrad 1966, p. 3 ff.

<sup>66</sup> See for example A. S. Amalrik, A. L. Mongajt, *V poiskah isčeznuvšijch civilizacij*, Moskva 1959, p. 185 ff.

<sup>67</sup> A. J. Brjussow, *Geschichte der neolitischen Stämme im europäischen Teil der UdSSR*, Berlin 1957, p. 25.

<sup>68</sup> K. H. Otto, *Über den Standort der Archäologie in der deutschen Ur- und Frühgeschichtsforschung*, "Forschungen und Fortschritte," Vol. XXVIII, 1954, p. 339 ff.

<sup>69</sup> For instance Eggers, *Einführung...*, *passim*.

<sup>70</sup> R. Wenskus, *Stammesbildung und Verfassung — Das Werden der frühmittelalterlichen Gentes*, Köln-Graz 1961. Cf. e.g. p. 126 ff. Some ideas referring to the ethnic situation in Polish lands remind H. Łowmiański's formulation (in the question of early European population). He is not inclined to ascribe the Wejherowo-Krotoszyn Culture to Germanic peoples (see p. 177 and 377). The following sentence discloses his line of thought (p. 374): — "Wir haben oben auseinandergesetzt, dass das Germanentum aus mehreren Gruppen verschiedener Herkunft zusammengewachsen ist [...]"

<sup>71</sup> A. Leroi-Gourhan also drew attention to the latter in his essay *L'histoire sans textes*, in: *L'histoire et ses méthodes*, edited by Ch. Samaran, Paris 1961, p. 245, ff.

<sup>72</sup> So is the problem seen among others by W. I. Kozlov, who rightly wrote at the same time, that the feeling of community is not an inborn but a social fact, (see e.g. Chomič, *O sodržanii...*, p. 80 and Leroi-Gourhan, *L'histoire sans textes*, p. 246). Wenskus had also drawn attention to that feature in *Stammesbildung...*, *passim*.

<sup>73</sup> Such is the proposition of Bromley, who indicates simultaneously, that he understands endogamy in a far wider meaning, which would be in the compass of a whole tribe. See note 62.

<sup>74</sup> They should not be understood as seeking for the accordance of race and ethnos or the attempt at finding some pure race, but as a tendency to fix the structure of a race, therefore the mutual relation of various race components and their prevalence in a given ethnic group.

<sup>75</sup> Compare J. Topolski, *Metodologia historii* [The Methodology of History], Warszawa 1968, p. 197. The feedback occurring here results in various systems. We may therefore apply only synchronic-diachronic rules.

<sup>76</sup> Eggers, *Einführung...*, p. 265 ff.

<sup>77</sup> See e.g. T. Malinowski, *Obrządek pogrzebowy ludności kultury pomorskiej* [Funeral Rite of the Population of Pomeranian Culture] Wrocław 1969, p. 168.

<sup>78</sup> Neither should those products be forgotten, whose very simple manufacture during many periods, does not allow to link them with one ethnos only; the discovering of their production could have very often been the consequence of convergence (e.g. bone needles, bone skates, etc.). Every such case requires separate examination.

<sup>79</sup> Compare e.g. W. Hensel, *Perspectives de la recherche archéologique sur le milieu rural en Europe occidentale du Haut Moyen Age*, in: *Settimano di studio dell Centro italiano sull'alto medioevo*, Vol. XIII, Spoleto 1966, p. 717. See also H. Ullrich, *Interpretation morphologisch-metrischer Ähnlichkeiten in ur-und frühgeschichtlichen Skeletten in verwandschaftlicher Hinsicht*, "Zeitschrift für Archäologie," Vol. III; 1969, p. 48 ff.

<sup>80</sup> Topolski, *Metodologia...* [The Methodology...], p. 284 ff.

<sup>81</sup> Topolski, *op. cit.*, p. 287.