The Alpine Races in Europe: Discussion

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Malays or Vikings, or to the present proceedings of the Japanese or ourselves; the phenomenon of a group of coastal islands influencing, from without, the adjacent mainlands, but not penetrating them or occupying them permanently, except under the occasional leadership of an Agamemnon or an Alexander, whose activity is from beyond the hills, and whose instincts are still pastoral mainly. How these "Mediterraneans" reached the *Ægean* is a further question. My own impression is that they are, as they themselves believed, indigenous, even if that has eventually to mean *pre-glacial*; analogous to the population of Cyprus or of Sicily, but brought thither possibly by sea; and, if by land, then by ways which postulate either a land-bridge over the Adriatic, or a prolongation westward of the basal plateau of Cyprus.

In any case, the actual distribution of the *Ægean* long-heads, apart from that of their culture, is no more obstacle to the view here stated of the relationship of the European and Anatolian broad-heads, than is the episode of the Thraco-Phrygian blondes.

The CHAIRMAN (Mr. D. G. HOGARTH): Mr. Myres has presented us with a problem of very great range and very great complexity. He himself represents a rare combination of the geologist, the palæogeographer, and ethnologist. He deprecates, as those who have read his paper will see, the introduction of the physiological question; but at the same time, if there is any one present who would wish to speak upon that, I am sure he would not object. I always feel that in all these craniological questions there lurks a certain physiological factor of very great importance, but of quite unascertained value—that is to say, the problem as to what conditions bring about change of skull-form, and whether the skulls of a race can alter and the race remain the same. The question has arisen with regard to the present peoples who claim to be Greeks. I think it will probably be better if, to some extent, we keep the different sides of this question apart, and I should therefore propose, first of all, to call on those who can speak to it more particularly upon the geological side, and deal with the question whether the glacial and pre-glacial conditions really render the northern route as impossible as Mr. Myres holds. I should like to know how far they agree with the view of Suess as regards the configuration of the east Mediterranean at a rather later age.

Mr. ARTHUR EVANS: I was not fortunate enough to be present during the first part of Mr. Myres' interesting communication. In any case, it would be difficult to deal with the whole of the questions raised by him. I understand that he wishes to make out that the round-headed "Alpine" race came into Europe by way of Asia Minor, and not, as has been hitherto generally supposed, from Central Asia. In support of this contention he has raised a great water-barrier on the Central Asian side, and thrown a land bridge over the *Ægean*. It seems to me—not speaking as a geologist, but as one to a certain extent familiar with the subject of Quaternary man—that a good deal of this argument would have been pertinent to the case of the remains of the older Palæolithic population in the European area, and might indeed be applicable in a comparatively late Quaternary period. But we can hardly suppose that, within the limits of any reasonable number of millennia—within the limits, at any rate, of the Neolithic Age—the *Ægean* was not much as it is at present, and I imagine, in the same way, that the shrinkage of the water-barrier on the side of Russia was of extremely gradual consummation.

We do know, of course, that the remains of human settlements in the Ægean country under its present geographical aspect go back to early Neolithic times, and so far as the evidence goes—so far, at least, as I have been able to follow it—there were at a very early period two main lines of connection with the Ægean area, one of them following the Russian coast of the Black sea, the other going very far north. Particular forms of “idols,” which on the Ægean shores belong to the close of the Neolithic period and the earliest age of metal, can be traced by means of intermediate types to the Danubian countries, and again, through the remains in Polish caves, to the Neolithic settlements of the Baltic Amber coast, and even as far north as the interesting settlement on the shore of Lake Ladoga, to which Mr. Myres has referred. And on the other hand, the Neolithic remains of Southern Russia, recently described by Dr. Stern, of Odessa, have brought out very interesting connections between the Neolithic settlements in that region and those of the Danubian basin, and have shown that these extended through the Balkan countries as far as Thessaly. One important point, as it seems to me, is, when do you find this round-headed race making its first appearance in the Alpine regions? So far as I know, the earliest Neolithic remains on that side are all of the dolichocephalic type, and in the Balkan countries, at any rate, the round-headed element begins to appear at a period when metal culture was fairly advanced. I do not deny the possibility of the round-headed Alpine race coming from Asia Minor, but their first appearance in the Alpine regions—which hardly goes back even to the earliest Neolithic stage—certainly seems to date from a time when the Ægean was already in existence. This, indeed, in itself is no bar to Mr. Myres’ theory, because there are such obvious facilities of access across the Dardanelles and the Bosphorus, as well as by the island stepping-stones of the Ægean. There is, however, a physiological question that has not been touched.

The theory that has been brought forward by Prof. Arthur Thompson, and experimentally illustrated by him, makes the development of the round-headed type dependent on cranial expansion, due either to the muscular action of the jaw or to increase in cerebral volume. According to this view, it is quite conceivable that there may have been centres of brachycephalic development, radiating from more than one point within a dolichocephalic area, and we need no longer look for distant avenues of immigration to explain the existence of a round-headed race in a given region.

Dr. HADDON: I am very sorry no geographer has spoken, as it is mainly a geological and geographical problem that Mr. Myres has raised. It seems to me that the anthropologists will have to consider very carefully the data supplied to them by geologists and geographers. For a long time I have felt the need of reconstructions of past geographical conditions, and when ethnologists have these they will probably be able to understand the differentiation and migration of races and peoples far better than they could from the material supplied only by their own science. In Asia there seem to be two well-marked groups of brachycephals—the beardless yellow stock, or *Homo mongolicus*, and the bearded white stock, or *Homo alpinus*. I hope Mr. Myres does not propose to introduce into literature, as he did in his discourse, the term “Anatolian” man for the latter group, since the older name is sufficiently explicit, and at any time it can be divided into varieties, such as Cevenole, Dinaric, and Anatolian. It has long been my view that this race originated somewhere on the western plateaus of Asia, and immigrants crossed into Europe, worked their way up the Danube, and entered France, perhaps by the pass of Belfort, in the Neolithic age. I do not at all see how the “diet of masticable fruits” could have materially assisted in inducing brachycephaly, as the people could not have continuously eaten soft food for a long space of time; fruit is ripe

for so short a period, and there must of necessity have been other kinds of food of harder or tougher quality, and why should not these, according to this method of explanation, have elongated their heads? Also I do not quite follow the connection, indicated on one or two of the lantern slides, between certain trees and races of men. I have been greatly interested in this paper of Mr. Myres, and it appears to me that his main conclusions are likely to stand.

MR. ARTHUR EVANS: I was speaking of the Balkan peninsula. The early appearance of dolichocephaly in the West might possibly be explained by Thompson's theory. My point was in relation to Mr. Myres' theory of trans-Ægean immigration—namely, that, in that case, you ought to find this Alpine type at a very early period in the Balkan regions. Of course, the exploration of the prehistoric remains through a large part of that peninsula is still very imperfect.

DR. SHRUBSALL congratulated Mr. Myres on a brilliant and interesting interpretation of the present-day distribution of the Alpine races of mankind in the light of palæography. The chief difficulty seemed to him to be to prove the existence of the broad-headed type at the period under consideration. No remains of the broad-headed type have been found which could be ascribed to an earlier period than the Neolithic, and not to the earlier part even, of that cultural stage; whereas an enormous period of time must have elapsed since the various glacial and interglacial epochs. In the earlier of these epochs there was, even, no quite satisfactory evidence of the very presence of man in the western section of the Eur-Asian continent, so that the development of the special features of *Homo alpinus* at this time in an elevated and isolated Anatolia could be but an ingenious speculation. Another difficulty, on Mr. Myres' hypothesis, which had not been quite cleared up, was to explain the presence of broad-headed man in the Danish islands, the Canaries, and on the Tunisian coast. Lastly, Dr. Shruballs desired to emphasize the insufficiency of the present data for anything more than an outline hypothesis such as Mr. Myres had propounded; until more was known, any such suggestion was as difficult to controvert as to formulate, but this paper served a most valuable purpose in summarizing the present knowledge of the subject.

THE CHAIRMAN: Perhaps Mr. Hudleston will say a few words now.

MR. HUDLESTON: Of course, it is open to any geologist to make any sort of map he likes, and I am bound to say a great many geologists make maps just in order to suit their own particular views. On the whole, I'd rather not attempt to criticize anything I saw on the screen; although those plans shown by Mr. Myres were most beautiful, yet at the same time I fail to see their connection with the subject, and that being so, I certainly am not competent to discuss it.

MR. GRAY: For the most part, I should be inclined to agree with Mr. Myres. I think in one point, however, he does not take quite the right view: I mean on the question of the influence of environment in changing the physique of a race. I think that anthropologists hitherto have made far too little allowance for the comparatively rapid changes that can be made upon the physique of a race by change of environment. Quite recently there has been a great deal of evidence cropping up which shows that these changes may be very rapid indeed. For instance, some skulls, measured by Dr. Macdonell, which were found in Whitechapel, and pretty certainly date from the time of the plague, belong to a type quite different from any found in the British Isles at the present time. They belong to a race which we can only parallel by going back to Neolithic times. At the present day we cannot find in any region of Great Britain a population corresponding to this Whitechapel population of two to three hundred years ago, and the conclusion that appears to be forced upon us is that the great change of environment produced by

modern industrial conditions has actually changed the type of the race. It has also been found in recent investigations that in large industrial towns the pigmentation of the population is so completely changed that, judging the race from this particular feature, we can only say it is a race entirely different from the average of the whole country, the probability against its being the same race being very high. So that here we have an industrial environment changing a population in a hundred years so completely that anthropologists would be compelled to say that the resulting population was a different race. Now, I think that, in view of these facts, we would have to very seriously consider the possibility of these round-headed types being the result of their environment, and it appears to me to be much more probable that the Cevenole type was really evolved from the surrounding long-headed races than that it was an offshoot of the Dinaric race. It corresponds much more nearly to these long-headed races of the Neolithic type in stature. It seems highly probable, from their present geographical distribution, that broad-headed races are better adapted to mountainous countries than are long-headed races; and if that is so, a Neolithic race inhabiting the European Alps would have plenty of time to develop into a broad-headed race. Mr. Myres seemed to think that it was difficult to change the shape of the skull of a race. But it is quite conceivable that such a change may take place in the course of many generations under the influence of a moderate selective action always acting in the same direction. The longer-headed members of a group would be eliminated by a suitable environment, and in the course of time only the broader heads would be left. It is quite possible that by selection the Cevenole group was produced from the previous long-headed Neolithic races. I agree entirely with Mr. Myres that the Dinaric type came from Asia Minor, but I am inclined to think that it is a different type from the Cevenole type, and of quite different origin.

Dr. WRIGHT: I should like, first of all, to congratulate Mr. Myres upon his very interesting paper. I think that by very great ingenuity he has harmonized the facts which we have; my only quarrel would be that the facts are rather too few, and many of them are probably not very correct. I think, too, that the division of the skulls into round-headed skulls and long-headed skulls is altogether too crude. I think it is quite likely many round-headed skulls might be easily developed in a long-headed community. I found, in some skulls I examined in the East Riding of Yorkshire, that one got all intermediate types between extreme dolichocephalic and extreme brachycephalic skulls; but a brachycephalic skull with an index of 82, and a brachycephalic skull from South Germany with an index of 82 might be totally different skulls. I always regard the South German skulls, with that characteristic occipital decline which they have, as very sharply distinguished thereby from other brachycephalic races, and as furnishing really the strongest argument that can be brought forward, that certain round-headed races are racially round-headed, and not produced from long-headed races merely by environment. I was extremely interested, too, in the diagrams which Mr. Myres showed of the vegetation in certain parts of Europe. I think it is very important to bear in mind the vegetation accompanying the different races, and I think one should also consider more the funeral rites which are practised. And then there is another point, and I think Dr. Sturer of Berne is the only man who has taken it into consideration, and that is the domestic animals. I wish Mr. Myres had considered these associations more fully. As regards the origin of this peculiar round-headed people, I imagine them to be of the type of the South Germans of to-day, and not of the type which was found in France and England in Neolithic times, and I think they came comparatively late into Europe, because they are still extremely pure, and I think they came almost certainly from the

East. I think there is no evidence of their coming from Africa. Whether they came north of the Caspian, or whether they came south of the Caspian, I do not think I could offer any opinion of the slightest value whatever. I think Mr. Myres' paper is a very useful one, in that it summarizes the facts and gives us a useful theory upon which to work. I believe, with Prof. Haddon and Mr. Shrubbsall, that the geographers and the geologists should come in and give us more facts, and then we might take up the line from them.

The CHAIRMAN: If no one else wishes to say anything, I shall in a moment ask Mr. Myres to reply to the various criticisms brought forward. I can only repeat my regret, and it is evidently the regret of the committee, that we have not been addressed upon the conditions which seem to underlie all these problems—that is to say, particularly, the geological and palæogeographical conditions. The paper deals so exclusively with palæogeography that one feels that one's own personal knowledge of the geography of some parts of this area is of no value whatever. We are really asked to consider this part of the world under conditions totally different from those which obtain at present. But, so far as I know the geography of Asia Minor and of the Balkan lands, Mr. Myres has stated the modern conditions where he brings them in, with very great accuracy. There are one or two minor points in his paper which I should like a little more information about. One of those is with regard to a remark he makes at the end of the paper about the prevalence of the long-headed *Homo Mediterraneus* in the Ægean islands up to quite the end of the Bronze Age. He mentions it as being absolutely exclusive. Of course he would admit that the data at our disposal are very few. There has been a certain amount of anthropometric investigation in Crete, but so far as I know not more than about 70 skulls have been examined, and those, as it happens, belong to one period of the Ægean civilization, and that an early one. We have practically no information about the latter periods, and it is just in one of those that a rather curious contradiction seems to be suggested by the type which is represented upon one of the Knossian frescoes which Mr. Arthur Evans first discovered, the fresco since known as the "cupbearer," which reminded him of the Albanian type; that is to say, we have a broad-headed type represented. That possibly may imply the entrance of a new race such as there is some other evidence to show may have come in about that period. At any rate, it seems to me to impair the general statement which Mr. Myres has made in his paper, that the long-headed race prevailed exclusively in the Ægean lands down to the end of the Bronze Age. Then again, though perhaps it might take us too far afield, I always feel I want my doubts about some palæoliths removed; that is to say, how far the presence of man can be assumed simply from these so-called stone weapons. I noticed, in one sketch-map which Mr. Myres put upon the screen, that he indicated these palæolithic remains extending in a kind of crescent round as far as Egypt. I believe there is more doubt about Egyptian palæoliths than perhaps about any others. There you have a country where peculiarly dry conditions have prevailed for many centuries, conditions in which sand can exercise its greatest power, and these supposed flint implements could have been produced by natural action, and not human action. However, Mr. Myres did not rest any part of his argument upon the distribution of palæolithic remains. With regard to what Dr. Haddon said about the introduction of the name "Anatolian man," I would join with him in deprecating the bringing in of such a name. I think it would be peculiarly inappropriate, because "Anatolian" is not only a modern name, but also describes purely modern conditions; that is to say, it is applied to the eastern part of an area which, according to the theory of Suess, had no distinct existence in geological time; therefore we had better not have a peculiar type of man introduced

as "Anatolian man." I am rather glad to see that those authorities who have spoken with peculiar knowledge of craniology all seem to echo my doubts about the physical factor in craniological questions. They all seem to imply that there is little reason to suppose broad-heads need always have come from a common origin, and that they may have developed quite independently. And I must say that I feel a little doubt about the great superiority of this enlarged south-eastern Balkan area over the Balkan area as it is at present, for the development of a special type of man, and I feel still greater doubt about the conditions of isolation which Mr. Myres seems to postulate as necessary for the evolution of a distinct type. Then, again, there is the difficulty which has been brought forward about the presence of small patches of broad-headed men at a considerable distance from any centre of broad-heads, and cut off from them at present by very considerable geographical boundaries. But the time is getting late, and I think there are a great many things about which Mr. Myres has been asked, and if no one else wishes to say anything I will ask Mr. Myres to reply.

MR. MYRES: No one could be more sorry than I am that we have not been fortunate enough to draw the geographers and the geologists. It is precisely because it seemed to me worth while to do even very imperfectly what Dr. Haddon insisted on the importance of—re-construct the palæogeography of this region—that I ventured to submit this paper to the committee at all. One or two points of geology have, however, been raised. Mr. Shrubbsall, in particular, raised points which I admit were very natural interpretations of the maps which I put on the screen. I used a map labelled "interglacial conditions" to illustrate also a point in post-glacial times. I ought to have explained—and if I had not been cutting things down I should have explained—that upheaval at one period of comparatively mild climate does not, of course, mean that upheaval occurred in a similar way at the next subsequent period of mild climate. I ought really to have produced a map which would show the retreat of the ice-sheet when it was retreating finally, and not have economized by showing over again a map which showed the retreat of the ice-sheet under what seem to be regarded, at all events by some geographers, as having been conditions of upheaval in the north-west.

There is a further point. If I understand the geological evidence, upheaval in the north-west does not necessarily mean upheaval in the south-east also. One of the things, upon which apparently geological speculators seem to be fairly agreed, is that one gets evidence of differential upheaval. And, in particular, in regard to the Ice Age, one of the things that seems to have happened is that the whole of the sub-structures of what is now Europe swung to a certain extent upon an axis: sometimes that axis is sufficiently central to cause an upheaval of the south-east when there is a depression of the north-west; sometimes the whole thing moved more like the lid of a box, and then you have only differential upheavals. One's own experiences in the Ægean teach one sufficiently well the extent to which, in a small area like that, very marked swings occur. Take the island of Crete. In the west, you have an upheaval of 80 feet since classical times, and in the east you have a depression of 30 feet.

In regard to the question of environment and physique, I do not think Mr. Gray took account, in his criticism, of quite all the cases that one has to deal with. When you have got ancient and modern stages of the population of an area, in some cases you find it is perfectly true that an absolute change of type has taken place in time without any obvious change of population, historically recorded or evidenced by changes of culture and so forth. But it seems to me that that is only one of three groups of cases. For instance, in the Ægean you can trace definitely the intrusion of fresh people out of an adjacent area, and *pari passu* with that a

gradual change of the skull form. That is to say, it is roughly in proportion to the exposure to incursions which the Ægean had undergone, that you find the Greek physical type altering. There you seem to have a case of what one was taught in one's youth was "concomitant variation." Then take the case of Sicily and Italy; there you have a population which has undergone all sorts of conditions; you have periods of great prosperity, and periods of starvation; the country has such an enormous seaboard, it is liable to have all sorts of things happen to it. But in spite of that, there is practically no difference at all in the population of South Italy and Sicily since Neolithic times onward. Here you have all sorts of conditions apparently changing, and the physical type simply sticking where it was. So that I am not sure that the case is by any means made out at present for anything like habitual variation of type. No doubt in this, as in other respects, some types of mankind are much more stable than others; and probably where a type has been once mixed, a certain swing of the pendulum goes on while it adjusts itself.

With regard to the particular piece of evidence upon which Mr. Gray laid most stress, one has to remember modern conditions are modern, and it will be a very dangerous thing to argue, because in the east of London artificial stress has produced certain effects in a couple of centuries, that even in two thousand years comparatively small results would be effected when you are dealing with mankind in the state of nature. That meets also a point Dr. Haddon raised, as to the hardness and softness of skulls. I doubt whether Neolithic skulls were much softer than modern skulls; I think they would be harder; but the period of time is infinitely longer.

There was another point with regard to food and trees. I am bound to say, in passing, that the evidence in regard to the animals is to my mind so very difficult that I left them out. I have been into the case of animals as far as I have had the opportunity, but the illustrations of "spread" in a particular direction—I confess I am looking for "spread"—the particular illustrations all happened to come from the plant world. Besides which, the animal is much more migratory than the plant, especially in association with man. Man can take his animals with him; the plants by no means follow so well; they are, therefore, better test cases of the lines of least resistance, climatic and geographical. I, perhaps, ought to explain more clearly than I did before, that the reason why I laid stress upon the chestnut in particular is because it still remains, over the area where it will grow, a very considerable article of food, especially among the least civilized classes. One sees this very markedly in Crete, and all round the fringe of the Ægean; one sees it again in north Italy, and pretty well wherever the chestnut has gone. The case of the chestnut is the clearest, but in the case of stone-fruit particularly there does seem to have been concomitant intrusion of certain edible fruits along with the arrival of broad-headed men. Of course, our evidence with regard to that is from the things found in settlements. We cannot tell that they had them for food or simply for toys, but it looks as though the way they got at the peach-stone was that they ate the peach.

One point more Mr. Shrubbsall raised with regard to the route of influence from north-west into Russia. One has to draw distinctions between a route of intercourse and a route of migration, and I am not at present satisfied that we have any evidence of a route of migration—of bodily transference of masses of people—from the Ægean north-westward so as to complete the circle, which he mentions, of broad-headed man. Where one can trace broad-headed man in the area between (say) the mouth of the Danube and the Ægean islands, he has the appearance of being intrusive and comparatively recent, and not earlier than Neolithic, at either end, so far as one can interpret the evidence at present.

I am afraid I did not make myself quite clear about the question of the long-headed people in the Balkans. I had to mention them, and the thing would have borne considerable expansion; but the difficulty which Dr. Evans feels, and we all feel, is this: How is it that you have a well-marked and very persistent patch—we do not know that it is more than a patch—of long-headed people among the Bosnian tombs at a period so very much later than that at which we have well-marked broad-headed people so much further west? Either the broad-headed people came by way of the north route out of Asia, or they came by way of the south route. I hope that I have satisfied others besides myself that the north route was closed. If they came by the south route, how is it that they have left this patch, or that this patch of broad-headed peoples in Balkan lands has survived? My suggestion for dealing with that difficulty is to suppose that the tomb-type is of significance; and that, thanks to their tomb-type, one is justified provisionally in regarding these Bosnian long-heads as an intrusion (of a kind which has frequently happened since) of pastoral people out of the steppes into the Balkan peninsula who have pushed considerably far afield into an area where broad-headed man did not follow, and where possibly he never had been. I suggest that the reason is, that (so far as we know) the geology and geography of that area are such as to make it neither forest nor lake-land.

Dr. Shrubsall's point about the Guanches is explained, I think, if one interprets Deniker's "Atlantic-Littoral" type as the result of the intrusion of "Dinaric" tall short-headed folk among a "Mediterranean" population, long-headed and short. Thus interpreted, the distribution of the "Atlantic-Littoral" type reveals itself as a series of stepping-stones between the south-western fringe of "Dinaric" man and the Guanche and other short-headed outliers in north-west Africa. That is why I have included the "Atlantic-Littoral" people in my map-analysis of Deniker's conclusions: it is the south-western counterpart of his "Eastern" race in central Russia, and his Russian patches of "Cevenoles" are the analogue of the Guanches.

Dr. Wright's suggestion as to the comparative study of funeral rites leads to very instructive results already; but one of the points still in suspense is, how far funeral rites can be regarded as really deep-seated characters of human groups. One can learn to bury one's dead a fresh way; one cannot learn a fresh shape for one's head. In the case of the southern analogues of the *Kurgans*, even, I only venture to adduce them in this argument because I think there is sufficient evidence on the physical side to justify the assumption of early long-headed intrusion southwards from the *Kurgan* area proper.

A FIFTH JOURNEY IN PERSIA.*

By Major P. MOLESWORTH SYKES, C.M.G.

AFTER leaving Rizáb we rose steadily to the watershed, which was almost level for about 2 miles. We then descended close under a red and yellow hill, where the steep ravine was almost too much for the labouring team; but at last that obstacle too was left behind, and, finally, after a march of 37 miles, Sághand was reached. Late at night

* Read at the Royal Geographical Society, June 18, 1906. Map, p. 429. Continued from p. 453.