WILEY

The Burial of Olympia: Discussion

Author(s): Mr. Hogarth, Dr. Strahan, Prof. Haverfield, Dr. Stein, J. L. Myres, Ernest A. Gardner, W. A. Cannon, M. M. Allorge, W. N. Shaw and J. W. Gregory

Source: *The Geographical Journal*, Vol. 36, No. 6 (Dec., 1910), pp. 675-686

Published by: geographicalj

Stable URL: <http://www.jstor.org/stable/1776839>

Accessed: 31-05-2016 06:48 UTC

Your use of the JSTOR archive indicates your acceptance of the Terms & Conditions of Use, available at

<http://about.jstor.org/terms>

JSTOR is a not-for-profit service that helps scholars, researchers, and students discover, use, and build upon a wide range of content in a trusted digital archive. We use information technology and tools to increase productivity and facilitate new forms of scholarship. For more information about JSTOR, please contact support@jstor.org.

Wiley, The Royal Geographical Society (with the Institute of British Geographers) are collaborating with JSTOR to digitize, preserve and extend access to *The Geographical Journal*

would help the mosquitoes, for under such circumstances streams spread in many channels. Then, when they diminish in the summer, numerous stagnant pools are left in the beds of the many branches. It is worth noting that at Olympia special mention is made of the great prevalence of malaria in the later days, after the decay of Greece had become pronounced.

A theory may easily be carried too far. Important factors are almost inevitably overlooked. The writer realizes that he is dealing with great matters about which little is known. His purpose is not to advocate the theories here presented, but to set them forth that others may criticize and improve them. Geography is a root from which spring innumerable and, as yet, unsuspected branches in the form of economic and pathological results. These in turn bear leaves and fruit in the form of political and psychological phenomena of varied sorts. Other similar fruits may spring from other roots. With them the geographer is not concerned. It is his province to study faithfully his own subject and learn the nature of the connection between the parts from root to fruit. Only by knowing the whole of a plant can the fruit be improved.

MR. HOGARTH: On the whole I do not feel convinced that so large a cause is by any means necessary to account for the particular phenomena with which Mr. Huntington is dealing. I will not attempt to go into the wider question of general desiccation over the whole, or certain parts, of the world. There is no doubt that the most important questions that this paper raises are the meteorological question, and the semi-geological question about the deposit of alluvia in terraces. About these I have nothing which would be of any importance or any value to say. Having seen something of the country, and having studied Greek history, I will only advance a few questions about the arguments which Mr. Huntington derives from history. It is obvious that the period with which he is dealing is rather short. As geological periods go, it is nothing at all. He supposes that the dry period in Greece began about 300 B.C. But so far as I know, such scanty evidence as we have in Greek authors for the climate of Greece before that period tends to show that it was a dried-up country even in the fifth century, at any rate upon the eastern side. There is a well-known allusion by Aristophanes to the extreme clearness of the air of Attica, and another by Thucydides to the dryness and poverty of the soil. Mr. Huntington, in his paper, has made a great point of the fact that the eastern side is much drier than the western, and has stated his belief that the former could not have been so dry when it was the most populous and most historically important, as it was in the ancient times. Well, I should like to point out that the reason why the eastern side was the most important and the most populous in the ancient times had apparently no reference to its climate, but to the fact that the only land route up the peninsula goes up the east side. Those eastern cities lay in relation to a great trade route, which was of course extremely important when Greece was important, but which ceased to be important when Greece collapsed from various causes into a little provincial place. The transference of population, therefore, was not necessarily due to any climatic change, but to the fact that the routes by land and sea ceased to be important up the east coast, and population took the natural course of going over to the west

side, where the rain-laden winds of the Adriatic deposit most of their moisture without reaching the east side. That brings me to another point to which I think Mr. Huntington has paid too little attention—that is to say, to the difference in the conditions which will be produced by an organized society and by a dis-organized society in respect of forests and in respect generally of the water-supply of a country. We have a great deal of evidence in the Near East to show that in Roman times, at any rate, water was much more carefully stored than at present. Some of you, no doubt, have seen that book called ‘The Thousand and One Churches,’ which deals with a certain site in the central part of Asia Minor. You will see there a great deal of evidence for very large constructions for storing water, the decay of which has allowed that region to decay into absolute desert. It was only the action of man, impelled by other causes to interfere with nature, to modify nature, which ever made that region fertile at all. That consideration is, I think, particularly worth notice when we come to the question of forests. Mr. Huntington has used in his paper an argument which I repeated in the summary, to the effect that the barbarian invaders were fewer in number and used less wood than the settled inhabitants, and therefore cannot have been responsible for the more rapid decay of the forests. That leaves out of account the economic difference between a civilized and a barbarian society. The latter is, of course, wasteful in the extreme. When the barbarians came into Greece they could quite well have caused a destruction of forests which was out of proportion to their own number. We must also remember that the barbarians who came in that way were a nomadic society, and that they brought with them the most destructive agent in the Near East, namely, the goat. On the whole, I do not feel convinced (though I am quite ready to be convinced) that we have to do in this particular case of Olympia, with greater phenomena than might be produced by such sudden floods as occur at this day very freely in all parts of the Near East, and even in parts of Arabia. I myself have been witness of such a flood. It lasted about four hours, and the result was to lay silt 5 feet deep over the whole plain on which I was living, and completely to alter the physical geography of the valley. Since this can happen at the present day in a very short space of time, I do not see that it is necessary to assume any great climatic change to account for the deposit of silt, even to the depth of 15 feet. We know that similar silting used to happen in earlier periods. There is, *e.g.*, Ephesus, where the silt was laid to about the same depth by two little tributaries of the Cayster. We also know that this silting became so serious in the early imperial time the Romans were impelled to take certain measures to keep the mouth of the river clear for shipping. In the event, the dam which was made was the immediate cause of the complete ruin of Ephesus. At any rate, this case shows that the problem of silt was very serious 500 years before the period at which Mr. Huntington thinks his great causes were operative. I should also question one or two of those historical facts of Greek history of which he speaks. I am sceptical of this great decline of the Greek character, supposed to have begun in the fourth century. Greece itself declined in population simply because the Greek civilization touched the point at which it must have extension and transcend a little country unsuited for an expanding population. The population left Greece to evangelize the world.

At any rate, one must take with caution the statement, to which the writers upon malaria have subscribed, that there was a great and marked decline in Greek physique and Greek character, beginning somewhere about 350 and continuing with increasing rapidity up to the Roman time. That may be due to reasons independent of climate—to merely racial reasons—to a race which had begun to live early and live hard, wearing itself out. It is probably true that in a period

of political disintegration, malaria has a much better chance of increase than it had before ; but at the same time, it is pretty clear that malaria existed in the classical period in Greece. If it did not, it is almost impossible to explain the Boeotian character. That is all I think I have to say. On the whole, it seems to me that what happened at Olympia was not more than can be accounted for by alternating wet periods in such a normal state of dryness as prevails over all the Nearer East to this day, and which, so far as I can judge from the little that we can tell from ancient authorities, prevailed in Greece in very ancient times. These particular floods of the Alpheios which happened between 500 and 600 A.D. may have been simply the result of certain wet periods occurring during a normally dry period, when man was making no effort to restrain floods.

Dr. STRAHAN : We are indebted to Mr. Hogarth, not only for an admirable *précis* of Mr. Huntington's paper, but for making, by means of his interesting personal reminiscences and critical remarks, what almost amounts to a communication of his own to the Department.

Prof. HAVERFIELD : In discussing changes such as those now before us, we particularly need to keep our chronology in mind. So far as I can judge, the decay of Greece began *after* 350 B.C. ; and, so far as it was due to any one cause, it may be connected with the increasing emigration of the best Greeks to the East in the early Macedonian period. In Italy the case is plainer. The decay of Italy did not begin about B.C. 200, when the mosquito is said to have first brought in malaria or the climate to have changed, but 450 years later, and while malaria then intensified the evil, as weeds and vermin will everywhere, it was not a preliminary cause. I may also say that malaria seems to me to have reached Italy long before B.C. 200. The early Italian towns show that, planted as they were on high hills away from water and fertile soil, yet often in positions of no great tactical strength. Later, cultivation came in and malaria went back ; towns arose in the valleys, and parts even of the Roman Campagna became thickly peopled.

Dr. STEIN : It has been a source of great satisfaction to me to read Mr. Huntington's highly interesting paper, and to recognize in it the same remarkable powers of close observation and lucid analysis which have made his earlier investigations into the physical changes affecting the economic conditions and civilization of Central Asia so instructive to me as a student of the geography and archæology of that region. Having worked myself over most of the ground which is dealt with in his fascinating book on 'The Pulse of Asia,' I feel I ought at the outset to express my conviction that the case for the general desiccation of the Tarim Basin within historical times, which Mr. Huntington has set forth there in so illuminating a fashion, appears to me established beyond all doubt by the changes which systematic archæological exploration has disclosed about the oases ancient and modern along the Taklamakan and Lop-nor deserts. Evidence almost as conclusive is known to me from those parts of the extreme north-west of India and the adjoining border lands with which I am familiar. Nevertheless five years ago, when I had the pleasure of meeting Mr. Huntington before his start for Eastern Turkestan, I thought it necessary to suggest to him the advisability of extending his investigations to classical ground, Syria, Asia Minor, etc. I am hence particularly glad to welcome in his paper the firstfruit of this new campaign.

The reasons are, briefly, that questions bearing on the rate of progress in desiccation or on its pulsatory nature cannot be properly studied without an adequate range of exact chronological data ; and, further, that in order to discriminate within an area once cultivated between results produced by desiccation and others which resemble them, but may in reality be due also to a variety of complex

factors connected with human activity, it is very important to have the means of checking our observations by consecutive historical records. Now, it must be clear to every critical student that our knowledge of the early history of the Tarim basin and the adjoining regions is only just beginning to emerge out of a dawn which, compared with what we know of ancient Greece or Italy or Asia Minor, might well be called utter darkness. It is true that our archaeological explorations during the last decade have allowed us to fix approximately the period when certain ancient sites were abandoned to the desert, and to realize the general conditions of life, cultivation, etc., prevailing before their abandonment. But in no case has the actual proof been furnished that this abandonment was a direct result of climatic change, *i.e.* desiccation. All that can be asserted with certainty is that, as regards a number of such ancient oases, desiccation has since progressed so far that irrigation, the indispensable condition of cultivation, would now be quite impossible.

On ground where the inadequacy or fitfulness of historical light makes it still difficult to interpret correctly our archaeological observations, gathered from widely scattered sites, special caution seems needed before we accept as definite chronological evidence for pulsatory changes of climate, conclusions which cannot be checked from independent contemporary records, and which may often prove inconsistent with facts brought to light subsequently. This warning as to the cogency of certain archaeological conclusions seems to me necessary, because in the course of my second Central-Asian journey I have had occasion more than once to note plain archaeological facts which cannot be fitted into Mr. Huntington's Fluvial and inter-Fluvial Epochs however attractive this theory may appear otherwise. I may mention one instance. The wall of the ancient *limes* which the Chinese built right through the desert west of Tun-huang or Sha-chou and south of the dying course of the Su-lo-ho river, supplies an almost ideal gauge for any physical changes which that part of Central Asia may have witnessed, the influences of the complex factors connected with cultivation being there altogether excluded. Documentary evidence proves this wall to have been built about 100 B.C., and abandoned, as far as the section here referred to is concerned, early in the first century A.D. Salt lakes and marshes, such as are ranged along the terminal course of the Su-lo-ho river, are particularly liable to changes of level in accordance with the climatic factors which affect the area of their drainage. Yet wherever this wall abuts on any of the salt lakes and marshes which were included by the old Chinese engineers in their defensive line, it can clearly be traced down to within a few feet of the actual water-level observed by me in the spring of 1907.

The period of construction and occupation of this *limes* fell into Mr. Huntington's "First Historic Fluvial Epoch," while at present we are supposed to be in a dry "Inter-Fluvial Epoch."

Chronologically precise evidence as to climatic conditions such as this is unfortunately very rarely at our disposal in Central Asia. But in the regions around the Mediterranean, where the remains of well-known civilizations go back to much earlier periods, and can be dated far more accurately with the help of historical records, a systematic search is likely to produce ample materials. The problem of climatic change affects intimately many lines of research bearing upon the history of human activity and development. It is hence a matter of congratulation for geographers and historical students alike that a scientific observer of such rare penetration and experience as Mr. Huntington has now commenced to collect those essential materials on the ground which seems the most favourable. I earnestly hope that he will find the needful leisure and assistance to extend his investigations there over a sufficiently wide area, and by examining an adequate

number of exactly datable sites, to secure broad and secure foundations for his theory.

Prof. J. L. MYRES: We are dealing with a very complicated problem; with evidence from different directions; with the interaction of a number of quite large causes operating over wide areas; and, at the same time, with smaller and individually less important factors, operating locally in varying degrees, and producing different results in different places. We have heard, for example, from Mr. Hogarth how a disorganized condition of society lets loose the pastoral nomad, and leads to destruction of forest; and then we hear from Dr. Stein of an instance, that has come under his notice, of a precisely opposite effect. That indicates how complicated the problem is. This is particularly so in a region like the Mediterranean, where the general uniformity is itself the result of generally uniform interaction of large different conditions and factors.

Mr. Huntington has indicated how in his belief the Mediterranean climate has been affected by pulsatory extension of Asiatic conditions westward, and at other times affected by the extension of Atlantic conditions eastward. But it seems highly probable that there has also been similar fluctuation between the north and south. That north-and-south fluctuation I hope Mr. Huntington may some day examine for himself, because it seems to me that it is on that side that he is likely to get confirmatory evidence of pulsatory desiccation.

This aspect of the matter has occupied my own mind for some time, and this is how the problem presents itself to me. Any large causes of desiccation which have operated as far west as the Mediterranean region, ought to have been producing the same effects in the large grass-lands and deserts of North Africa, as in Arabia and East Central Asia. Consequently, it seems worth while, where we have so long a series of historical records as we have in Egypt, to plot out those occasions when, simultaneously with troubles of the east frontier due to aggression of nomad peoples from the Arabian desert, Egypt was troubled on its other frontier by incursions from the Libyan grass-land. Here, when I first went into the matter, I found a pleasantly large proportion of approximate coincidences. Unfortunately, that little inquiry may have to be done over again, when the historians have satisfied themselves as to the chronology; for as long as you have a period of 1460 years lost or mislaid in the middle of Egyptian history, it is difficult to be sure of the length of pulsations, if pulsations there be. In the only other bits of the North African desert area with which we have any historical acquaintance, the evidence on the whole seems to me to be favourable to Mr. Huntington's general theory; for in Tripoli you have very marked evidence, last, of desiccation since Roman time; then, of a highly-organized water administration in Roman time itself, supervening upon a period in which there was magnificent prosperity, but, so far as I know, no remains of waterworks.

Here, on the face of it, we are dealing with a period of desiccation, in which it became more and more urgently necessary as time went on to provide for water, which hitherto had fallen naturally. That has to be taken, too, in connection with the historical evidence for forests—as late as the fifth century B.C.—in that section of the North African coast, a region which is practically waterless now, and seems to have been for the most part underwatered in Roman time.

In Greece itself, we are dealing with a strongly featured region, of abrupt ridges and small plateaux, where minor local forces have been acting in an intensified degree. In such a region it is extremely difficult to detect the larger forces at work in the way which Mr. Huntington suggests; exact definition is less possible, and also less urgently necessary, than it would be otherwise, for we are dealing in any case with an area which is marginal, and we must expect that

whatever pulsations there were would be somewhat less regular, somewhat more rapid, and perhaps locally somewhat more accentuated, than in larger and less irregular regions. I should not be at all surprised to find that in Greece we had ups and downs of climate, superposed upon the larger pulsations, lasting only a comparatively small time, and effecting only ill-marked variations in the forest *régime*, or the human *régime* either.

Prof. ERNEST A. GARDNER : I should like to keep, as far as I can, to the details with which I have a certain amount of familiarity. As regards Greece, I think the question of change of climate is a very difficult one. It is a question which troubled the Greeks just as much as it has troubled us. You will find a passage in Plato's 'Critias' which states a theory that Greece was once covered by forests, and that owing to the disappearance of forests the mountains have become denuded of soil. If that statement was made in the fourth century, it is rather difficult to know exactly how much was true then and how much is true to-day. Of course some of that passage is simply geographical theorizing, of which it is one of the most early and interesting examples. I think this deforestation has been exaggerated a good deal. It is to some extent true ; the causes which have been mentioned certainly operate at the present time. The goatherd thinks nothing of burning down a forest in order to get fodder for his goat, and there is very little care taken of the forests in Greece. A fact that tends for the preservation of the forests is the use of resin in wine ; in some pine forests every single tree has been tapped. In the first place, I should like to protest against the description of the bareness of the country with which Mr. Huntington began. It is, after all, its character ; it is due a great deal to that bareness that you have the extraordinary clearness of outline that is the great beauty of Greece. A well-wooded country would not have that extraordinary clearness of outline, and it is these clear-cut outlines which lead to a great deal of what has often been noted as characteristic of the Greeks, their love for clear and precise and exact form. As regards another question which Mr. Huntington raises, I should like to speak a little more in detail as to the question of the burial of Olympia. I suppose that everybody who has ever conducted an excavation has wondered how exactly the ruins with which he is dealing came to be buried as deep as they are. In some cases, as at Delos, there is no soil to cover them, and the houses are simply buried in masses of loose stone, which apparently are the upper parts of the houses which have collapsed and buried the lower parts. And elsewhere you have the walls of houses built of mud-brick and washed down. In the case of Olympia there are the causes which have been mentioned ; but I am not quite sure about this question of Lake Pheneos ; the question is an extremely interesting one geographically. The lake is a mystery ; we have many records of its flooding the valley of the Ladon. I believe at present there is no water at all in it ; at least there was not two or three years ago. When I saw it some twelve years ago it was a very considerable lake, and the most extraordinary feature about it is the high water-mark which exists about 100 feet or so above the level of the plain, running right along the side of the mountain. Dr. Philippson, who is the most competent authority who has examined the lake, has made it quite clear that it is a high water-mark. The best explanation seems to be that the lake was repeatedly filled up to this height and then emptied itself ; there must have been some curiously arranged channel which led to the water rising a certain height and then emptying itself with great violence and causing a flood. It is recorded that in the year 1834 there was a violent flood down the valley caused by this emptying of the waters of the lake. Such a violent flood can in quite a short time deposit a large amount of silt, and that may be partly the explanation why Olympia i

buried to such a great depth. Of course the earth from the hills does get washed down into the valley, and the silt does extend up beyond the junction of the Alpheios and Ladon. At the same time, I think the great depth of soil at Olympia is partly caused by these inundations from Pheneos. That, of course, is a purely local matter, but Mr. Huntington discusses it at considerable length. As regards the very interesting question of desiccation and the result of it in connection with the mosquito, I should like to say that I was immensely interested by Mr. Jones's theory. Of course it is quite true that the history of Greece does not end at the time of Alexander. But at the same time what is most characteristic of the Greeks in arts and literature does come to an end at his time, or even earlier; and for this reason it is very tempting to accept some such explanation as has been suggested for this decadence.

MR. HOGARTH: I will only answer what Prof. Gardner has said about literature—that its decline was a sign of the general decline of the Greek people. Is our literature at this moment quite what it was? Yet are we on the decline? I am afraid no one here has felt himself competent to say how far this question which Mr. Huntington has raised about the formation of terraces in Central Asia is really a sound theory. I am inclined to believe that Prof. Gardner is right, and that the old theory that the Ladon had a good deal to do with the filling up of the Alpheios valley is not yet refuted.

MR. W. A. CANNON: I have been much interested in the discussion, but as my work has been on other lines than those of Mr. Huntington, I cannot, I am sorry to say, contribute anything to it. It may be of some interest to your members, however, to learn that at this moment Mr. Huntington is conducting his researches, with which you are familiar, in the south-west portion of the United States. He is studying evidences of prehistoric peopling of the Tucson region, Arizona. Of Mr. Huntington's results I am not prepared to speak, as the investigation was only begun at the time I left Tucson. That much of interest and of scientific value will come from his work in this semi-arid region can hardly be doubted, as there are numerous abandoned villages throughout the region and many miles of unused irrigating ditches—evidences of an earlier inhabitation which seem to indicate more favourable climatic conditions than at present.

MR. M. M. ALLORGE: I should like to direct the attention of the members of the Department to a paper read last month by M. Cayeux before the Geographical Society in Paris. M. Cayeux is Professor at the French School of Mines; he spent two summers at Delos, and in this paper concerning the archæology and geology of Delos he comes to the conclusion that uniform geographical conditions have prevailed there since classical times. There is practically no change of the sea-level: a very slight change of meteorological conditions. A large torrent was diverted and utilized for water-supply, and on the low alluvial plain left free by this torrent the city extended. I believe that M. Cayeux corrected several assumptions made by the archæologists over there, and I understand that in the future a geologist will be appointed as adviser at the French archæological school in Athens.

DR. STRAHAN: I hope it will be conveyed to Mr. Huntington that by his work he has stimulated in this room, all across the Atlantic, one of the most interesting discussions we have had for a long time. It only remains for me now to ask you to pass a cordial vote of thanks to him for his paper.

MR. HOGARTH: I was responsible for this paper being brought before this Department. It would not have been quite fair for the paper to be brought, in its author's absence, before a general meeting of the Society, but I thought the paper would be the better for being discussed, because it raises the important

question of the modification of Geography by man. I agree with the Chairman that the paper is extremely able and likely to stimulate a great deal of interest in the subject, and I hope Mr. Huntington will understand that we have discussed it from a purely scientific point of view without taking any particular side in the matter. We shall be only too glad to hear his further views upon the subject.

The following communication was received from Dr. W. N. SHAW: I read Mr. Huntington's paper through. It is time something was done about the suggestions of variation of climate; suggestions of a somewhat similar kind occur in so many different places and in such a variety of ways. The phenomena to which Mr. Huntington calls attention are apparently a combination of physical geography and geology as well as meteorology. I do not know whether anybody can give an answer to the question as to what would happen supposing that a country of the geological structure of Greece were subjected to the constant recurrence of variations of moderate intensity in the way of rainfall, etc., which constitute the climate as we know it. In other words, supposing that the meteorology remains the same, what happens to the physical geography of the district? Geology being a continuous process, changes will go on continuously, the economic results of which might be those actually experienced. Unless there is some evidence of definite meteorological character, or the economic effect of persistence of climate is worked out, the introduction of a hypothetical change of climate must necessarily be a little arbitrary. The fact is, if we are to have no direct meteorological evidence—and I am afraid that is impossible—we must depend upon comparing the phases of the different suggestions of climatic change in different parts of the Earth which are thrown out as speculations, and find out whether the phases agree, so that some day it could be somebody's duty to compare the chronology of the different papers suggesting climatic changes. Perhaps Brückner would like to write a fresh edition of his book.

The following was received from Prof. J. W. GREGORY: Mr. Ellsworth Huntington's interesting paper raises issues that concern many important questions, and as I cannot be at the meeting I venture to refer to a few facts bearing on the problem.

Mr. Huntington's conclusion that about 200 or 400 B.C., Greece and Italy underwent a diminution in rainfall in consequence of a climatic change due to the recession of glacial conditions in northern Europe may be regarded as a variant of the oft-suggested theory that the world is now undergoing slow desiccation. Mr. Huntington's modification of that hypothesis is a great improvement on the original. Nevertheless certain facts tell against it. Most geographers will probably agree with Mr. Huntington that the disappearance of the ice from northern Europe had an important climatic effect along the Mediterranean countries. When the northern parts of the British Isles were covered by snow and ice the present cyclone track must have lain further to the south, probably giving southern Europe and northern Africa a better rainfall than they have at present. It may be doubted, however, whether that period was so recent as Mr. Huntington's theory requires. The glacial conditions must have passed from the British Isles, except in northern Scotland, at the date of palæolithic man, and even if we do not accept the exaggerated antiquity that has been attributed to him, he must be referred to a date to be reckoned in thousands of years B.C. The evidence of Roman works such as the catch-water drains around the Fens and the Roman bridge at Chollerford, the classical records as to the freezing of the Rhine, etc., all appear to indicate that England and Germany had in Roman times a similar

rainfall and climate to that of the present time. The change of climate in Europe produced by the recession of the glacial conditions was probably complete long before the Christian era.

Two years ago I had occasion to consider this question carefully in regard to Cyrenaica, on the shores of the Mediterranean opposite Greece. There may be evidence in that country of a heavier rainfall in prehistoric times, and probably synchronous with the British glaciation. I did not, however, see any evidence of any considerable climatic change or decrease in rainfall since the date of the stone age in Cyrenaica. The classical records and the evidence of the ruins both suggest that the climate at the date of the foundation of the Greek colonies six centuries B.C. was not appreciably different from the present. I went to the country expecting to find that at the date of the Roman and Greek occupation of Cyrenaica the country was densely forested, with luxuriant gardens in the clearings, and that it then had a much heavier rainfall and better rivers than at present. I found no such evidence, however. Cyrenaica appears in classical times to have been an open country, liable to periodic droughts, so that the utmost care had to be taken of every available source of water. The general conditions seem to have been similar to the present. The people, for example, suffered occasional periods of severe famine, and they had to guard against plagues of locusts, which are pests characteristic of dry districts. As there is no evidence of a marked climatic change in historic times in Cyrenaica, it seems doubtful whether a sufficient climatic change can have affected Greece to explain its political decay.

Mr. Huntington adopts the view that the decay of the ancient Greeks and Romans was due to malaria, while earlier (p. 9) he attributes the comparative inactivity of tropical people solely to climate. Is not the latter view a tradition that has survived from a time before the recognition of the effects of disease? Why attribute degeneration to climate in the tropics, and to disease in the temperate zones?

The following communication was received from Mr. G. G. CHISHOLM: The subject of Mr. Huntington's present paper is one of absorbing interest, and that interest is delightfully kept up by the sustained ingenuity of his presentation of his case. His account of the valley of the Alpheios seems to me to make a material addition to the evidence bearing on changes of climate within historical times; still his interpretation of that evidence, plausible as it is, does not seem to me absolutely convincing, and I should therefore like to pay homage to the scientific spirit which he has shown throughout his paper by responding to his concluding invitation to express reasons for doubt. It seems to me that, as accounting for the terraces of the Alpheios, he has passed over too lightly "the theory of deforestation" as expounded by him. He says that a report recently submitted to the Congress of the United States by Prof. Moore, Chief of the United States Weather Bureau, seems to indicate that the cutting off of forests so as to leave mountains naked does not lead to great quantities of soil being washed down. I have not seen this report, but if the meaning is merely that once the hill-sides are naked little or no soil is washed down, I should think that that hardly needed the demonstration of "carefully kept records." But this supposition cannot be involved in any theory of deforestation. All that that theory presupposes is the rapid washing away of soil as the roots die off, such as Mr. Huntington points out must take place when forests die naturally.

Mr. Huntington says that in failing to give a satisfactory explanation of the history of a people he does not mean to criticize the historians, but the economists, pathologists, and geographers. But if geographers have failed in

this regard Mr. Huntington implicitly admits that they have at least the excuse that the influence of geographical conditions is extremely complex and often very indirect. Sometimes his argument, however, seems to imply that the influence is simpler and more direct than it is. It must be borne in mind that where diminution of population is the direct result of diminished local production that is no proof of any diminution in natural productiveness. Witness the observations of Sir William Ramsay and other speakers on the nomadization of Asia Minor in this *Journal* (vol. 20, pp. 263, etc.). Further, it has to be borne in mind that a diminution in population does not necessarily imply any diminution in local agricultural production. With reference to this point we have only to consider the present position of Great Britain in relation to its food-supply. Commerce, when developed under peculiarly favourable conditions, has always been the means of attracting the means of support from a distance. As to Greece, Prof. Philippson finds the explanation of the changes in the conditions of its civilization partly in the mischief wrought by man, partly in the shifting of the world-situation (*Weltlage*), partly in variations in the demands made on the land by the civilizations of different periods. In ancient times, and more particularly from the sixth to the fourth century B.C., Greece was so situated as to be an intermediary through which a great part of the trade in the valuable products of eastern Asia with the newly opened lands of the rapidly developing western Mediterranean was carried on.* This, of course, cannot explain the marvellous development of Greek civilization, but it formed one of the necessary conditions of that development, and no other explanation, I will not say of the decay of Greece, but of decay in Greece, than the loss of that situation, seems to be needed.

That, however, does not of course preclude the possibility of a deterioration in climate as a factor contributing to that decay. Of the spread of malaria there can be no doubt, and Mr. Huntington's observations on the connection between that and a possible change of climate are, to say the least, extremely interesting. But the evidence with regard to such a change within historical times is so conflicting that I still feel compelled to keep an open mind on the subject, although the cumulative weight of evidence seems to tell in favour of a certain degree of desiccation. One argument sometimes adduced with confidence as a proof that there can have been no diminution in the rainfall of the Mediterranean region even in the region of the shotts may be dismissed as valueless. It is that of Prof. Partsch given in a review in *Petermanns Mitteilungen*, 1889, *Litteraturbericht*, 965 d. (p. 61), according to which the North African shotts, having no outlet, are natural ombrometers, and the fact that old Roman buildings are found on their margins, as they are at the present day, is, in the words of Prof. Philippson, who has adopted this argument,† “a strict proof that there the precipitation is the same as in antiquity.” But it is obvious that the cogency of this argument depends on the volume of the shotts being the same as in past times, whereas the fact mentioned as to the position of Roman buildings proves only that the area is at least as great as it was in Roman times. But meantime for two thousand years wadies have been depositing sediment in those lakes, and desert winds have been blowing into them large quantities of dust, which does not get blown out again, so that to have the same volume as in the past the lakes must be greater in area than they were in antiquity. The argument accordingly proves nothing either way. On the other hand, some of the other arguments used by Prof. Philippson in the same place, such as that the whole life of the Greeks of antiquity their unroofed theatres, their open-air festivals, are adapted to the same kind of

* “Griechenland und seine Stellung im Orient,” Lzg., 1897 (Sonderabdruck aus der Geog. Zeitschrift), pp. 11, 15.

† ‘Das Mittelmeergebiet,’ 2nd edit. (1907), p. 131.

climate as that of the present day, seem to me to have a good deal of cogency in them, and one would like to know what Mr. Huntington has to say in reply to them.

Mr. Huntington sends the following remarks :—

In the present incomplete state of knowledge as to the climate of the past, diversity of opinion is highly desirable. My own opinion is in constant process of change, or rather growth. In 'Explorations in Turkestan' the theory of a progressive change from the past to the present seemed sufficient to account for all the known facts. In 'The Pulse of Asia' I felt obliged to modify that theory by the introduction of fluctuations having a period of hundreds or thousands of years. Further research has confirmed the belief that the climate of past historic times was distinctly different from that of to-day, and that the change from former conditions to those of to-day has been marked by pronounced fluctuations having a periodicity of hundreds of years. Now, however, I am inclined to believe that the fluctuations have been more numerous than I formerly supposed, and that they have sometimes taken place with surprising rapidity. I welcome such a discussion as this before the Research Department, for it brings out the weaknesses of a theory and adds new points of confirmation. For example, Dr. Stein's remarks as to the relatively dry conditions prevalent when the western end of the Chinese wall was constructed agree with certain phenomena in Syria and elsewhere in suggesting that the second century B.C. may have been a somewhat dry period, not so dry as the present, but more so than the periods before and after. This modification of the original theory, together with others of like nature which must doubtless be made, will probably bring it into harmony with the chronological requirements set forth by Prof. Haverfield.

Professor Myres calls attention to the fact that, if the theory of climatic pulsations be true, Arab invasions of Egypt from the east and Libyan invasions from the west ought to have taken place at approximately the same time. I regard such suggestions as especially valuable, because they show that the problem of climatic changes is a subject for study by the historian as well as by the geographer. North Africa is a critical region. The *Bulletin* of the American Geographical Society for September, 1910, contains an article in which I have considered the climate of the Libyan oasis of Kharga. There the evidence seems to agree with the conclusion of Professor Myres, and to be opposed to that which Professor Gregory has derived from the neighbouring region of Cyrenaica.

The discrepancy of opinion as to North Africa and other parts of the world may arise from an overestimate of the degree of change demanded by the theories under discussion. Mr. Hogarth, Professor Gardner, and Mr. Chisholm all refer to the fact that the habits of the ancient Greeks and their accounts of the climate indicate dry, clear air, abundant sunshine, long droughts, and other characteristics such as prevail to-day. This is undoubtedly true. But is there any part of the ancient statements which is not equally applicable to both the western and eastern sides of modern Greece? Yet parts of the west coast have twice as much rain as Attica. The ancient descriptions of Greece prove beyond question that the country then, as now, lay in the subtropical zone. It had rainy winters and dry summers. The dry summers in themselves would suffice to give rise to the out-of-door life of the Greeks, even if the winters were not relatively warm. The demands of our theory are satisfied by the supposition that in ancient times the winter rains were more abundant and lasted longer than now. In an earlier period, the rainy season may have been still longer. This would be expected, if we are right in supposing that the change which now seems to be in progress is merely the final stage of the passing away of a glacial epoch. The remarks of Plato's critics, quoted by Professor Gardner, seem to confirm this view.

No. VI.—DECEMBER, 1910.]

3 A

Climatic changes are undoubtedly meteorological phenomena, but, as Dr. Shaw points out, they cannot be studied by meteorological methods. This does not mean, however, that exactness and precision of results are unattainable. No one doubts the occurrence of the glacial period, and practically no one questions its division into epochs. The evidence of post-glacial climatic changes afforded by ruins, traditions, historic records, old strands of salt lakes, river terraces, ancient vegetation, and other phenomena is as abundant as that which forms the basis of our theories as to glaciation. It can be far more accurately dated than can any glacial phenomena. Numerous other lines of evidence may some day be available. For example, Mr. Allorge says that at Delos there has been practically no change of the sea-level in over two thousand years.* But may there not have been a slight change? In the *Geological Magazine* for April, 1901, Mr. H. W. Pearson published an article dealing with oscillations in the sea-level. Fact and theory were there so intermixed, and theories were carried so far that the article failed to receive the attention which it deserves. From some two thousand recorded observations Mr. Pearson constructed a curve showing a slight rise and fall of the ocean in certain latitudes with a periodicity of approximately six hundred years. He called my attention to the fact that his curve of oceanic fluctuations agrees closely with mine of climatic fluctuations, as given in 'The Pulse of Asia,' but that his shows one cycle which mine does not. Further study, as has been said, has convinced me that the number of climatic fluctuations is greater than I had supposed. I think, although I am not sure, that, when both are worked out with care, the oceanic and climatic cycles will agree. I mention the matter here, not as something which is proved, or even which is sufficiently certain to serve as a basis for theorizing, but merely as a suggestion of the ramifications of the subject. It is to be hoped that Mr. Pearson may carry his studies further, and may give us a full account of the facts on which his curve is based, so that we may form our own judgment as to the reality of his cycles of oceanic fluctuation.

To prolong the discussion of the many interesting and important points raised at the meeting of the Research Department is here impossible. The objections to the theory of climatic changes as applied to Greece, especially those brought forward by Mr. Hogarth and Professor Gregory, are so important that they demand most careful consideration. Some of them I have already answered in a volume entitled 'Palestine and its Transformation,' which is shortly to appear as the result of the Yale Expedition to that country in 1909. The others must await further study.

CLAUDIUS CLAVUS AND THE EARLY GEOGRAPHY OF THE NORTH.†

By EDWARD HEAWOOD, M.A.

THE work under review is a revised and much extended German version of a memoir brought out (in Danish) in 1904, and briefly noticed in the *Journal* for July, 1905 (p. 93). It sheds, however, so important a light

* Cf. note in the Monthly Record, *infra*. For references to papers taking the opposite view see *Journal*, vol. 24, p. 483; 31, p. 570; 32, p. 91.—[Ed.]

† 'Der Däne Claudius Claussøn Swart (Claudius Clavus), der älteste Kartograph des Nordens, der Erste Ptolemäus-Epigon der Renaissance.' By A. A. Björnbo and C. S. Petersen. Innsbruck: Wagner. 1909. 24m.