of its temperature, is determined by one factor only—the amount of sun-heat it receives.

How very erroneous is this assumption, may be shown by the contrasted climates of places on the east and west sides of the Atlantic, due to the influence of both ocean-currents and prevalent winds; but even more strikingly by a comparison (which I made in my "Tropical Nature") between certain tropical and temperate climates. In Java, about 8° south of the equator, the altitude of the noonday sun in June is about $58\frac{1}{2}°$, while at London during the same month it is 62°, the length of the day at the same time being $5\frac{1}{2}$ hours greater with us. The sun-heat received in London must therefore be *considerably greater* than that received in Java, and, according to the rule that the *amount* of sun-heat determines temperature, London should then have the warmest climate. The fact, however, is that our mean temperature in June is more than 20° lower than that of Java and our mean highest temperature about 18° lower, a result due, as I have shown, to a variety of causes, of which the temperature of the atmosphere in all surrounding areas, the action of aqueous vapour in reducing the loss by radiation, and the accumulation of heat in the soil, are probably the most important. These facts prove, I think, that the amount of heat received by the whole hemisphere, through its influence on both oceanic and aerial currents, must be taken account of in estimating temperatures under different phases of eccentricity; and that any determination of the amounts of sun-heat received at particular latitudes, considered by themselves, are necessarily misleading and must usually indicate a difference of climate far below the truth.

But there is another consideration of even more importance which entirely invalidates the arguments of those who, like Mr. Culverwell and Prof. Darwin, treat the problem as one to be determined by a simple mathematical calculation of amounts of sun-heat received on the same area at different times. This is, the remarkable difference in the behaviour of air and liquid water on the one hand and snow and ice on the other, as regards climate; the former from their great mobility tending to the diffusion of heat, the latter by its comparative immobility to the accumulation and perpetuation of cold. Without this power of accumulation perpetual snow on tropical and temperate mountains, and glaciers in hot sub-alpine valleys and at only 705 feet above the sea-level in latitude 43° 35′ south in New Zealand, would be impossible. In either of these cases, if an elevation of about a thousand feet should double the area of the snow fields, which might easily be the case, the outflowing glaciers would be greatly increased in magnitude and might either descend to much lower levels or spread out over large areas of the lowlands—and all this without any change whatever in the total amount of sun-heat received by the countries in which they occur.[1]

For some years past there has been a persistent attack by astronomers and physicists on the explanation of the glacial epoch put forth by Croll and adopted with some modifications by many students of glacial phenomena. But as these writers have all treated the problem as a question of the direct effect of the amount of sun-heat received at different epochs in corresponding latitudes, completely ignoring the great distributing and accumulating agencies which are always and everywhere in action, their theoretical conclusions appear to us to be entirely beside the question. We have to deal with a highly complicated problem in physical meteorology, which cannot be solved by an appeal to the well-known facts of the amounts of sun-heat received, any more than can the June climates of London and Batavia or the general climates of Ireland and Manitoba or Terra-del-Fuego (in about the same latitude) be explained from similar data. The great merit of Croll was, that he fully realised the complexity of the problem; that he took account of the various relations and reactions of the oceanic and aerial currents, and the physical characteristics of air and water, snow and ice; and that he showed how these causes reacted on each other so that the winds and ocean currents of one hemisphere might have an influence on the accumulation of snow and ice in the other. Whatever errors he may have made in matters of detail, his method was undoubtedly a sound one, and it is because so many recent writers on the subject have wholly ignored his method without even attempting to prove that it is erroneous, that their views appear to us to be both retrograde and scientifically unsound.

ALFRED R. WALLACE.

[1] This remarkable property and its effects are explained in some detail in my "Island Life," p. 131 (second edition), under the heading "Properties of Air and Water, Snow and Ice, in Relation to Climate," and in the four following sections.

The Dying out of Naturalists.

THE dying out of the distinguished school of "naturalists" which this country once produced, and which culminated in Darwin, is a fact which scarcely admits of dispute. I am informed on good authority that it has not escaped the notice of the French scientific world.

I drew attention to it in the address which I delivered to the new Botanical Section of the British Association at Ipswich. I rather described the phenomenon than attempted to explain its causes. But what I said has brought me many interesting communications. It has been suggested to me that as far as botany is concerned, I have much myself to be responsible for. It may be so. But this I may say, that in entering the laboratory I did so with the natural history spirit. I only looked at interesting things with a closer vision. So, if I may go to the other end of the scale, did Darwin when he made use of all the newer appliances of biological research in his later work.

Nothing, it seems to me, is more difficult than to trace to their right causes the springs of human endeavour. Its results are familiar to us, because we live amongst them. We are so prone to assume "motives" off-hand for any human action that we see about us, that nothing seems easier than to explain any new departure that comes in our way. But the process is almost certainly superficial, and the real causes of a social change which breaks upon us suddenly have in all probability been of slow growth, and do not at the moment either reveal themselves or readily lend themselves to analysis.

A friend, a well-known naturalist, gives me his explanation. I suppress his name, as I have not his permission to quote it; but I think what he says is worth printing, as affording ground for reflection. Whether the cause he assigns is or is not well founded, I confess I do not know.

But generalising from experience I can say this: all distinguished naturalists whom I have known have gone ahead in defiance of any and every obstacle. Looking back upon their lives, it was as if fate had conditioned them. It was once said to me that if one ever came across a possible artist of merit, the right thing to do would be to offer him every discouragement. If he had real genius he would transcend his ordeals; if he had not, the world would not be appreciably the poorer if he was quenched.

But I must discriminate. English naturalists of the generation which is now passing away have belonged to two groups. Some have been born to wealth, some to poverty. Class prejudice was against the one; means of livelihood against the other. The richer disciples of our art seem now to have gone irretrievably, and to have no successors. The poorer have changed their tone; they tend to treat science as a career like the Civil Service. They approach those who have any hand in the matter in an extremely business-like spirit. I do not blame them. But this is not the *métier* of the scientific hero. Nor in their memory shall we assemble to found a national memorial or raise a statue.

What is the force that now-a-days quenches the old enthusiasm? My correspondent says that it is the schools, and here is his story. I believe that, at any rate, what he says is the outcome of sincere conviction, or I would not publish it.

"I am pleased to see your remarks upon the dying out of the study of systematic botany, and I see in other papers, too, attention called to this and the diminution of field naturalists. One starts one's natural history usually on these lines when a boy—or, rather, used to—but I noticed things had altered much when I visited my old school last winter. In my day we had lots of naturalist boys; we knew all the localities for insects, plants, shells, &c. Now hardly any one knows anything of the country beyond the playing fields. The 'skipper field,' famous for skipper butterflies; the heath, with its localities for all kinds of insects and plants, are absolutely unknown. The great object of education appears to be to have every boy competing for something absolutely useless to him in later life. They were practising cricket or other games, or cramming for exams. al the while. This remarkable system begins, the masters of this and other schools told me, at about eight years old. There is no time to learn to think or observe. The boys must beat some other school in tennis or football, or must beat some one else in the history of the Punic Wars. Science was taught, but much in the same way. They were neither taught, nor did they get a chance of teaching themselves, any natural history. What the result of this will be it is difficult to foresee, but it certainly accounts a good deal for the diminution in systematists and field-naturalists."

W. T. THISELTON-DYER.

Royal Gardens, Kew, December 27, 1895.